Alison Wylie

THINKING
FROM THINGS

Essays in the Philosophy of Archaeology

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To MFW with warmest thanks;
in loving memory of LHW

Contents

Preface

I first learned that philosophy and archaeology might have something to do with one another in an archaeological field camp. At the time (the summer of 1973), I was working for Parks Canada at Fort Walsh (Saskatchewan) as an assistant field supervisor—a summer job after my first year of college. As luck would have it, the director of that project was an ardent New Archaeologist, trained at the University of Arizona in Tucson; he had been hired by the National Historic Parks and Sites Branch of Parks Canada to help develop an ambitious field research program that was to provide the archaeological foundation for interpreting and developing historic sites across Canada. What made archaeology worth doing, in his view, was not just the intrinsic interest of the enterprise—the wholly absorbing process of recovering tangible evidence of past human aspirations and accomplishments—but what it could teach us about the conditions of life, the reasons for cultural change and persistence, affinity and diversity, that manifested themselves in the gritty particulars of the archaeological record. For him, as for many others at the time, "archaeology was anthropology or it was nothing." It was in this context that I first learned, and learned in a way that was viscerally connected to the *doing* of archaeology, that archaeology was undergoing a revolution.

In the spirit of bringing revolution to the hinterland, we were incited to commit philosophy at every opportunity, especially when immersed in the most earthbound of archaeological labors. We read not only the most up-to-date theoretical statements by prominent the New Archaeologists (L. Binford 1972a; P. Watson, LeBlanc, and Redman 1971; Deetz 1967; J. Fritz and Plog 1970; Flannery 1967) but also a selection of work in the history and philosophy of science. Because archaeology needed to break the grip of traditional "paradigmatic thinking," we read Kuhn (1970); but because the hoped-for new paradigm was to be resolutely scientific, we read positivists on the structure of scientific confirmation and explanation. I remember laboring at least as long and hard, in preparation for that first field season, over the intricacies of Hempel's account of general laws (1942, 1966) as over the complexities of the fort's construction sequence. In the process we learned what it could mean to incorporate into even the most mundane archaeological practice a philosophical injunction to design research always as a problem-solving, hypothesis-testing exercise.

After that summer, in the fall of 1973, I returned to the second year of a liberal arts program and took an introduction to philosophy of science. I read Hempel and Kuhn again, this time in the company of Norwood Russell Hanson (1958) and other critics of logical positivism who were intent

on "contextualizing" the enterprise of science in various ways. They challenged settled convictions about the stability of evidence and its independence from theory, the distinctive logic of explanation and of hypothesis testing, and the ambition of "rationally reconstructing" the fundamental principles of scientific practice. It was then that I began to puzzle about the philosophical foundations of the New Archaeology. I had the good fortune to discover, almost immediately, that a number of others were already energetically transgressing disciplinary boundaries, exploring the possibilities not just for fitting philosophical models more neatly to the practical exigencies of archaeology but also for doing a new hybrid philosophy of science: philosophical analysis that takes its cue as much from the fields it studies as from its own intellectual tradition; philosophy from the ground up. I begin with a brief account of what I learned from the field project at Fort Walsh, by way of setting the philosophical essays that follow in the archaeological context from which they arose.

AN ARCHAEOLOGICAL PARABLE

Fort Walsh is located in the southwest corner of Saskatchewan, next to Battle Creek, in the heart of the Cypress Hills.[1] The hills comprise an uplifted plateau, some twenty square miles in area. It was described in the nineteenth century, and is still regarded, as a kind of oasis in the prairie; its exposed benches offer dramatic long-range views of the prairie, and its deeply cut streambeds are thick with ponderosa pine and wolf willow that, in times past, supported rich stocks of game. Fort Walsh was the epitome of a western frontier site, and highly romanticized at that. One of the first summers I worked there I came across a full-page newspaper advertisement for the fort that conjured up the vision of an isolated garrison of brave Northwest Mounted Police (NWMP), facing down hordes of unruly and unprincipled U.S. whiskey traders on one hand, and several thousand battle-hardened, angry, and dangerous Sioux on the other. The headline ran something like "100 Police . . . 5,000 Indians . . ." and featured the stereotypic image of a fierce horse-mounted Plains Indian in the foreground, with the rugged outpost of a fort in the background. This was truly the stuff of the old West nostalgia industry, which, I subse-

quently learned, was already well under way when the fort was originally founded.

Fort Walsh of the 1870s represents a fascinating moment in the "conquest" of the Canadian West, as Limerick describes it (1987), and the archaeological project developed for Parks Canada by James V. Sciscenti in the 1970s was remarkable in a number of ways. The fort had been founded by the NWMP in 1875 close to the site of an infamous massacre — the Cypress Hills massacre of 1873 — in which twenty to thirty Assiniboines camped near a pair of trading posts were killed by a party of (U.S.) American and Canadian traders. The official mandate of the NWMP was to control the rapacious U.S.-based whiskey traders and to settle the "Indian situation" on the outer edge of what the new Canadian federation liked to think of as *its* western frontier.[2]

By the time the NWMP appeared on the scene, the Cypress Hills had been occupied and exploited with increasing intensity by a growing number of displaced tribal groups for at least a century. The standard view was that before this time, the hills had been a no-man's-land exploited by a number of neighboring groups but occupied by none (summarized in Wylie 1978: 18–22). It had become the focus of operations for a number of independent Métis traders by the 1830s, for two short-lived Hudson's Bay Company posts, and, after 1846 when Fort Benton was established, for American Fur Company traders (Sciscenti and Murray 1976: 1–2; Sciscenti et al. 1976: 6–14; McCullough 1977: 2–10; Wylie 1978: 18–22). As Karklins has described the situation, "in the late 1860s, the Canadian prairies were invaded by a horde of American whisky traders who callously peddled their rotgut product to the local Indians" (1987: 1). In 1875 the North-West Territories Act made it illegal to import, sell, trade, or produce intoxicants of any kind (Environment Canada Parks Service [ECPS] 1981: 3), and the NWMP were dispatched to the Cypress Hills, a major center of the whiskey/fur trade, to "bring law and order to the Canadian West" (Karklins 1987: 2). How effective they were in stemming the flow of whiskey remains an open question. Certainly a high volume of trade in buffalo robes continued for several years after the NWMP arrived in the area, until the herds were depleted; "in 1878 approximately 20,000 buffalo robes were shipped from Fort Walsh[,] . . . dropping to 300 robes by 1880" (Klimko et al. 1993: 4).

A sizable town grew up next to the fort soon after it was established in the late summer of 1875. At its most expansive it boasted two permanent trading establishments representing Fort Benton–based businesses (I. G. Baker and T. C. Powers), several more short-lived stores, two hotels and at least one restaurant, pool halls, a laundry, a barbershop, a photography studio, a tailor, and numerous other services and suppliers (McCullough 1977: 17; Karklins 1987: 2; Klimko et al. 1993: 1). There was no church of any denomination (McCullough 1977: 17), though Karklins indicates that "several halls were used for various purposes including church services when a clergyman was in town" (1987: 2), and there is some disagreement about the presence of a blacksmith shop (McCullough 1977: 17; Karklins 1987: 2). Connected with the town and its various business establishments was a civilian population that ranged from a core of perhaps 200 to as many as 4,000 multiethnic, multiracial, and multinational inhabitants, some seasonal, others relatively permanent (Sciscenti and Murray 1976: 2; Sciscenti et al. 1976: 11; McCullough 1977: 14–17; Karklins 1987: 2). At the time I joined the Fort Walsh archaeological project in the mid-1970s, standard historical accounts routinely acknowledged the presence of townspeople who were identified as Asian, African, Anglo-Canadian, Anglo-American (U.S.), Métis, and First Nations representing half a dozen different regions and tribal groups (Sciscenti and Murray 1976: 2; McCullough 1977: 16–18).

In 1878 Sitting Bull and some 5,000 Sioux arrived in the Cypress Hills seeking refuge after their confrontation with Custer (Sciscenti et al. 1976: 14). They followed an earlier group of 3,000 Sioux—led by Black Moon, who had moved north into the Wood Mountain area, east of Cypress Hills, in the spring of 1876 (Chambers 1972 [1906]: 47)—and joined smaller groups of Tetons, Yanktons, and Assiniboines (Sharp 1973: 250–254). The presence of the Sioux, described in the newspapers of the day as warlike and extremely dangerous, not only alarmed Canadian government officials, who had hoped to begin moving settlers into the region, but was also reported to have created intertribal tensions with the local Blackfoot and Cree (Chambers 1972 [1906]: 51). It was at this point that Fort Walsh was made the NWMP headquarters for the region. Its force was doubled to 110 enlisted men and officers in 1878,

and the fort itself was substantially expanded (Sciscenti and Murray 1976: 2; Murray 1978b: 4).

Five years later, by 1883, the Sioux had returned to reservations in the United States; the Canadian-Indian treaties signed in 1874, 1876, and 1877 had been "implemented" (Sciscenti et al. 1976: 14; cf. Karklins 1987: 2); the last buffalo herds were effectively destroyed; and the Canadian Pacific Railway line had been completed, 30 miles to the north. In short, there was no further need for a fort in the Cypress Hills, and no means of support for the townspeople who had settled next to it. The NWMP headquarters was moved to Regina and the regional center shifted to Maple Creek, a railroad town on the northern outskirts of the Cypress Hills (Sciscenti and Murray 1976: 3). The region has never been so populous or cosmopolitan since. After several generations of experimentation with various sizes and forms of ranching and farming operations—a process made famous by Bennett's study, *Northern Plainsmen* (1969)— those who now live in the vicinity of the fort typically operate family-owned cattle ranches that run to an average of 20 sections each (20 square miles). The fort and massacre site trading posts have been reconstructed, and they sustain a small tourist industry as a national historic park (ECPS 1989a, 1989b, 1991).

The archaeological project in which I was involved in the mid-1970s was originally conceived in the tradition I have since learned to identify as "historicalist" (Schuyler 1978a: 1). An archaeological crew was dispatched by the head office (at that time located in Ottawa) with a mandate to establish where the walls of the fort depicted in sketch maps and occasional photographs had *actually* stood, to determine the details of construction and building sequence, and to recover some local memorabilia, useful for establishing the authenticity of a reconstruction and in personalizing it for visitors. As it happened, Sciscenti, the field director assigned to the project, resolved to do much more than this. We would certainly establish the baseline information necessary for site management, but this alone could not justify excavation; it was crucial, Sciscenti insisted, that we address more ambitious questions about the hows and whys of fort structure and location, that we use the archaeological record to interrogate the wisdom gleaned from well-worked documentary sources and, ultimately, do what we could to situate the

fort in the context of the larger cultural, historical, and political-economic processes that constituted the frontier of which it was just one short-lived and tangible manifestation.

Those were exciting times. We read the philosophers and historians of science who were then a source of inspiration for a generation of archaeologists committed to more thoroughly anthropological and more rigorously scientific modes of practice, and we read the luminaries of this burgeoning New Archaeology. At the same time we were assigned a substantial dose of what then seemed, by contrast, highly conventional local and regional history. The official historical account of Canadian frontier life and events became the interpretive framework against which we strained, the source of conclusions we irreverently cast as hypotheses and undertook to test, as we followed Lewis Binford's directive (1972a) always to treat interpretive claims as the starting point, not the end point, of inquiry. We were given to understand, as an article of faith, that one should never put trowel (or spade, or pick, or backhoe) to ground without first clearly formulating a question and a research strategy for addressing it (see "Research Considerations" in Sciscenti et al. 1976: 14–24). We debated the issue of what counts as a research question, and the fine points of how to implement a testing strategy. Most important, we wrestled with the implications—practical, methodological, and theoretical—of finding that our best hypotheses, especially those based on well-established documentary history, were all too often subverted by an unforgiving and unobliging archaeological record.

As naive as this seems in retrospect, the constant injunction to think had a wonderfully enlivening effect; everything we did was animated by a commitment to make our presuppositions explicit, however mundane and obvious they might be, and to try to conceptualize a larger problematic in terms of which the descriptive details we were recovering, with a vengeance, might take on significance as answers to "why" and "how" questions. We did establish a number of interesting factual details about the occupational history of Fort Walsh (e.g., Sciscenti et al. 1976; Murray 1976, 1979), but in the process we also learned a great deal about this short-lived frontier settlement that did considerably more than add fleshy detail to extant historical accounts (J. Harrington

1955: 1126, 1129). Despite the heroic image so ingenuously reproduced in the travel advertisement I had seen—an image that lurks not far beneath the surface of many more respectable representations of western history—it quickly became apparent that Fort Walsh was never a serious military operation; at least, it was never defensible. The fort was sited at the bottom of the river valley, accessible on all sides and surrounded by benchtop overlooks that gave every advantage to those the NWMP were meant to subdue. And its palisade was a shabby affair by any standard: logs of varying diameters were set irregularly in a shallow trench with nothing substantial to stabilize them, open to assault at any number of points and incapable of supporting a firing platform (Sciscenti et al. 1976: 25–42; Murray 1978b: 8). There was considerable evidence that the NWMP, far from facing off hostile Indians, had been dependent on local tribal groups for crucial supplies, at least through their first winter. Remnants of beadwork embedded in earthen floors that were later covered by floorboards suggest that uniforms had been "modified," or at least supplemented (Sciscenti et al. 1976: 239), and the faunal assemblage recovered from the earliest subfloors and garbage pits indicate that diets had been substantially augmented by wild game butchered by Métis or Native suppliers (Murray 1978b: 11). After the NWMP established themselves, the function of the force stationed at Fort Walsh was mainly diplomatic: the settling of "Indian treaties," peacekeeping between tribal groups from Canada and the United States, and the distribution of rations in times of famine (Sciscenti et al. 1976: 2).

Where the villainous U.S.-based traders were concerned, it seems that once their original (illegal) business in the hills was "curtailed," they made substantial profits supplying their adversaries, the NWMP (e.g., McCullough 1977: 23–33). And the goods they supplied included not just foodstuffs and hardware, medicines and clothing, but alcohol in remarkably diverse forms.[3] I was struck at the time by the sheer *volume* of beer, whiskey, wine, champagne, and medicinal alcohol that must have been consumed by the NWMP as they enacted their mandate to secure the Canadian frontier against the illegal trade in whiskey from the south. Telltale evidence of contraband spirits turned up in the most unexpected locations—not just in garbage pits, but on the earthen subfloor

under the collapsed floorboards of the officers' quarters, and in privies that served all ranks and periods of occupation. As I remember one summer's work on the officers' and commandants' privies, differences in rank and changes in command were marked not by any discernible variation in the volume of consumption but by sharp differences in the alcohol of preference. Another summer, we excavated a footing trench along the side wall of the enlisted men's quarters; our aim was to establish how the building had been stabilized, given well-documented drainage problems in the area. But as we reached the bottom of the trench we came upon a cache of cough medicine bottles of a brand that boasted a heavily alcoholic content (Sciscenti et al. 1976: 159), no doubt deposited by a work-weary NWMP building detail as they dug foundations for their newly expanded barracks. Initial assessment of the bottles retrieved in the course of the first field season suggested that the higher ranks enjoyed quantities of wine, cognac, champagne, bottled beer, and possibly some laudanum, while the enlisted men consumed beer and patent medicines with high alcohol content (Sciscenti and Murray 1976: 5; Murray 1978b: 10–11); subsequent analysis indicates a more egalitarian distribution of alcoholic beverages (ECPS 1981: 3–4). We also learned that as the fort was expanded, the spatial and functional differentiation of men by rank—the military hierarchy of the force—was much more rigidly enforced; these distinctions were reflected not only in the proliferation, over time, of rank-specific living quarters, privies, and storerooms but also in the cuts of meat the NWMP were eating and in the extent to which men of different ranks supplemented their diets with game (Murray 1976: 5–6, 1978b: 7; ECPS 1981: 2).

Finally, although we were not meant to do any work on the civilian townsite associated with the fort, we did survey the area for surface features and we tested a sample of the pits and depressions we had identified—a stratified random sample (of course), designed along lines recommended by Redman and Watson (1970). This was a fascinating project; though we could not pursue it very far, given the demands of excavation at the fort, our survey and testing made it clear that the Canadian frontier was by no means the exclusive domain of single, adult, white/Anglo men, nor was it an egalitarian haven for the Canadian counter-

parts to the bearers of Turnerian democracy (Turner 1893). Several substantial depressions in the center of the townsite proved to be cellar holes filled with burned and collapsed building material; they were the remains of solid two-story buildings, suggesting a core of relatively permanent structures. This much was well understood from the documentary record, but the richness of domestic material—including children's toys and fragments of what seemed to us at the time a highly refined tea service—suggested that these buildings housed not only commercial establishments but also some substantial residences. Karklins (1987: 15–16) has since assembled a detailed inventory of this material that records toys and a child's ring, a coffee grinder, and some jewelry, among other domestic and personal items, but he describes the ceramics as quite ordinary.

As we moved outward from the core of the townsite, the surface depressions we tested revealed not cellars but garbage or storage pits associated with what seem to have been smaller domestic structures, themselves surrounded, in another concentric circle, by depressions and disturbed areas associated with stables and workshops of various sorts. Extensive lithic scatters and teepee rings testified to a substantial First Nations presence on the periphery of the townsite. These preliminary observations are summarized in the report on a subsequent park property survey (Wylie 1978: 49–53), and they have been corroborated by recent salvage excavations conducted on the eroding bank of Battle Creek at the core of the townsite (Klimko et al. 1993). These revealed, among other things, the foundations of a small, well-constructed building "with full floor, glassed window, and a door with porcelain doorknobs and a key lock" (Klimko et al. 1993: 26), as well as a range of domestic artifacts, including fragments of a porcelain figurine, bone china, teaspoons, a necklace of glass beads, a harmonica, and the remains of liquor and beer bottles (Klimko et al. 1993: 19, 27, 31, 39). Building on this recent work, Parks Canada is developing an interpretive trail through the townsite area (ECPS 1991: 9; Kevin Lunn, telephone conversation with author, 1993).

It was not until after the fort had been extensively excavated, and a visitor reception center built and opened to the public, that a walk-over survey was authorized for the entire park property, the area around the fort, and the townsite.

When I conducted this survey in 1978 the lessons we drew from the townsite testing project were much expanded (Wylie 1978; Murray 1979: 1–3). We found an enormous number of sites, features, and historic trails throughout the park. On the basis of comparisons with excavated features on other sites in the region, I argued that a number of the larger pits located along historic trails and in the creek valley bottom might well testify to the early presence of Métis traders in the area and to other pre-NWMP trading establishments. Several seemed substantial enough to have been the sites of wintering camps as well as seasonal posts and rendezvous (Wylie 1978: 45–63). Early-nineteenth-century ranching activities were also evident in several areas, especially at a well-known but undocumented site just above the fort. But most interesting, virtually all of the bench tops overlooking the fort proved to be thick with lithic debris, tepee rings, and features described elsewhere as cairns, with smaller lithic scatters and clusters of rings appearing at almost every point in the system of bisecting creek beds and valleys from which the fort was visible. Many of these sites had been disturbed by park operations, never having been documented (Wylie 1978: 79–84), and surface visibility in forested areas was limited. Nevertheless, the density of material testifying to Native settlement in the immediate vicinity of the fort was striking, especially given the lack of attention paid to these sites in previous Parks Canada research and site interpretation.

The summer we did the park survey, archaeological testing at the bottom of a construction trench along the palisade revealed a deeply stratified deposit with no fewer than twelve (of twenty-eight) distinct strata showing evidence of prehistoric occupation (Murray 1978a: 144–176, 1979: 3–40); corroborating evidence of precontact occupation is summarized in Karklins's inventory (1987) and by Klimko et al. (1993: 42). This testing fortuitously confirmed our suspicion that the site and park property incorporate a rich prehistoric component, one that might challenge the no-man's-land construct we had taken as a baseline for protohistoric and contact period occupations. Clearly, we argued, the brief seven-year presence of the NWMP could not be treated as a moment out of time or wider cultural context. The complexity and plurality of this cultural history is now emphasized by the interpretive programs developed by Parks Canada for Fort Walsh (ECPS 1989a: 3–5).

THINKING FROM THINGS

The lessons I learned at Fort Walsh—theoretical and philosophical, as well as practical and technical—were repeated on half a dozen other historic and prehistoric sites on which I worked in the 1970s and 1980s, but nowhere else were they quite as explicit. Taken together, this fieldwork taught me profound respect for a conundrum that structures the long-running philosophical debates with which I have been preoccupied since the mid-1970s. What you find, archaeologically, has everything to do with what you look for, with the questions you ask and the conceptual resources you bring to bear in attempting to answer them. And yet, you almost never find all or only what you expect. As often as not, the process of inquiry forces you to rethink your questions, to envision possibilities that are very different from any of the prospective answers you might have entertained at the outset. As enigmatic and fragmentary as it is, the archaeological record has infinite capacity to surprise, to subvert even our most confident presuppositions about what must have been the case and why, even when the subject of study is a cultural past of such proximity that its surviving material record seems immediately familiar. Although the data-intensive practice of archaeology can rarely establish secure, incontrovertible conclusions, it is the key to expanding our understanding of the cultural past to include "the inarticulate" (Ascher 1974: 11), the "endless silent majority who did not leave us written projections of their minds" (Glassie 1977: 29) or who were not of interest to those who did construct a documentary record of their activities and interests. Most of the philosophical questions that have intrigued me arise from this paradoxical nature of archaeological evidence: its status as an interpretive construct and its intransigence; its ambiguity and its capacity to challenge and reframe what we think we know about ourselves as much as about the past.

In retrospect, the philosophical dimensions of archaeology would never have come into focus for me had I not been exposed, that first season at Fort Walsh, to the early interchange between archaeology and philosophy. From the outset, I was in-

trigued by the dynamic of debate that arose when, as it were, archaeologists attempted to wrestle philosophy to the ground and make it do some practical work. As I learned more about recent developments in philosophy of science—in particular, the widely touted "demise of positivism" (Suppe 1977a)—I was all the more curious about the conjunction of philosophy and archaeology I had encountered in the field at Fort Walsh. Philosophers were themselves arguing, with increasing vigor and assurance, that "received view" philosophy of science had gone wrong because it had gotten entangled with technical problems of its own creation; philosophical analysis, however exact, could shed little light on science unless grounded in a thorough understanding of the actual problems and practice of science. The engagement between philosophy and archaeology, initiated by archaeologists in the 1960s, has given rise to a thriving interfield characterized by just the kind of hybrid approach to the study of science that many postpositivists were vigorously recommending in the early 1970s. I came away from my initial introduction to this fledgling domain of inquiry with two key insights. First, fieldwork taught me that dirt archaeology is an intrinsically philosophical enterprise; there is very little you can do innocently, to paraphrase Clarke (1973a). And second, the early exchanges between philosophers and archaeologists made it clear that interesting philosophical work in this area would require an amphibious form of practice (Bunge 1973), grounded as much in archaeology as in philosophy.

Acknowledgments

It would never have been possible to pursue the various lines of inquiry represented in the essays that follow without the support of and, most important, the example set by all those who have been intent on pushing the limits of disciplinary boundaries, following interests that extend well beyond the confines of archaeology, or philosophy, or even (increasingly) the conjunction of the two.

In particular, I thank those who taught me not just the technical skills of archaeology but the intellectual excitement that goes along with exercising them: James F. Pendergast on the St. Lawrence Iroquois sites he was investigating in the 1960s; James V. Sciscenti and all those I worked with on Parks Canada projects at Fort Walsh and the Fortress of Louisbourg through the 1970s and early 1980s; and fellow students and staff on projects outside Mexico City (1975), at the Grasshopper Field School (1977), in the Delaware River Valley (1980), and, more recently, along the Green River in Kentucky and at Cahokia Mounds in Illinois.

I thank, too, those philosophical mentors who, at key junctures in my graduate and undergraduate training, saw the connections between philosophy and archaeology more clearly than I, even though they had no special interest in archaeology: Paul A. Bogaard, Leon J. Goldstein, and Rom Harré. And I am deeply indebted to those amphibious philosophers and archaeologists who were already there, working at the interface between these disciplines; their generosity made it possible to imagine working in a field that, by standard reckoning, did not exist. In particular, I thank Merrilee H. Salmon, George L. Cowgill, Marsha P. Hanen, Jane H. Kelley, Thomas C. Patterson, Bruce G. Trigger, Patty Jo Watson, and Richard A. Watson, as well as participants in the Cambridge archaeological theory seminar in 1979 and the archaeologists who were teaching at SUNY-Binghamton in the late 1970s, especially Charles L. Redman, Margaret W. Conkey, and John M. Fritz. There are a great many others whose influence is evident in the essays that follow; I hope I have done justice to their contributions.

I am also indebted to those who helped bring this particular project to fruition. I thank William E. Woodcock, who waited a long time to see it crystallize, and Elisabeth A. Lloyd, who saw the potential for broadening its horizons. I am grateful to Elaine Brown, Kent Hogarth, and Andrea Purvis for technical support that has been indispensable. And I particularly thank the referees who read this manuscript at Bill Woodcock's behest, as well as the colleagues who provided me wonderfully candid comments on it at various stages of its development: Jeremy Cunningham

Introduction

Philosophy from the Ground Up

THE TRAFFIC BETWEEN PHILOSOPHY AND ARCHAEOLOGY

Despite earthbound appearances, archaeology has always been a deeply philosophical discipline, or so I will argue. Certainly Anglo-American archaeology is remarkable for the extent and visibility of the philosophical soul-searching it has undergone in the past three decades. Many suggest that this represents a significant break with the past, whether it is to be welcomed as a timely waking from dogmatic slumbers or regretted for marking the loss of an idyllic time untroubled by unresolvable complications and uncertainties. In 1973, for example, Clarke declared that archaeology was struggling with a "loss of disciplinary innocence" (1973a).[1] Reflecting on the state of British archaeology, he described its emerging critical self-consciousness about goals and presuppositions as a consequence of postwar technologies that had dramatically expanded the powers of archaeological inquiry. Now archaeologists

would have to make choices they had never previously considered; and it was essential, Clarke argued, that these be explicit, reasoned, and informed by clearly articulated goals and principles. To that end he called for the development of a vigorous program of "internal philosophy," cautioning against the imposition of external models of science developed by philosophers for their own purposes, usually with reference to sciences that bear little relation to archaeology.

At the time Clarke was writing, the North American advocates of the New Archaeology were fomenting a revolution that was framed in explicitly philosophical terms. If archaeology was ever to contribute a genuinely anthropological understanding of the cultural past, they insisted that its goals and practice must be recast in resolutely scientific terms. Unlike Clarke, however, the New Archaeologists drew inspiration from models of science developed externally, by analytic philosophers,[2] and characterized their ambi-

tions in explicitly positivist terms: the central goal of a scientific archaeology was to be explanation conceived along the lines of Hempel's covering law (deductive-nomological) model of scientific explanation, and its mode of practice was to be a problem-oriented strategy of hypothesis testing, following the pattern of a hypothetico-deductive model of confirmation. More of this shortly.[3] In every respect, the New Archaeologists hoped and insisted, this deductive research program represented a decisive break with the heterogeneous cluster of practices that they designated "traditional archaeology."

In fact, new technologies and new ambitions routinely provoke internal disputes about goals and standards of practice in archaeology; innocent disregard for such questions is the luxury (or liability) of those whose horizons are defined by the normal science of localized, usually hard-won and temporary moments of consensus. The New Archaeologists contested what they took to be just such a consensus. But despite their distinctively positivist, Hempelian commitments, their programmatic demands for a more scientific mode of practice and their intense interest in the philosophical presuppositions of the discipline were by no means new. These concerns have been central to recurrent field-defining debates in North American archaeology since the late nineteenth century; in part II (chapters 1 and 2) I argue that antecedents can be found for most aspects of the program of research that the New Archaeologists advocated, as a new departure, in the 1960s and 1970s. Indeed, a spate of articles appeared at the time of World War I declaring the advent of a (first) genuinely scientific "new archaeology" that bears a striking resemblance to the "fighting articles" of the New Archaeology that appeared in the 1960s and 1970s (as L. Binford describes them in 1962, 1972a).

Philosophical influences are frequently evident in these archaeological debates. The critics of empiricist tendencies in the late 1930s drew on Whitehead and, later, on the work of philosophers of history such as Teggart and Mandelbaum (see Kluckhohn 1939 and, later, Taylor 1967 [1948]). Dewey was a critical influence for Raymond Thompson, who insisted, in the 1950s, that archaeologists cannot avoid a degree of subjectivism in their research (R. Thompson 1956)—a position that became an important foil a decade later

for the New Archaeology. But it was contemporaries of Thompson's, especially the vociferous proponents of objective (statistical) methods of typological construction, who were first influenced by the "liberal positivism," as Spaulding described it, of Bergman, Kemeny, and Feigl (Spaulding 1962: 507; see Patterson 1995b: 77, 84–87). Spaulding later drew on Hempel, Brodbeck, and Kaplan to develop an account of the explanatory goals of a scientific archaeology that anticipates, by several years, the most detailed of the arguments by which a younger generation renewed the argument for a scientific archaeology (Spaulding 1968: 34); Clarke and, later, Renfrew invoke Braithwaite when they take up the task of refining internal counterparts to some of these arguments (Clarke 1968; Renfrew 1989a); and Meggars draws on Reichenbach as well as the philosophical reflections on scientific practice published by a wide range of prominent scientists (Meggars 1955). Most recently, those who reject the positivism of the New Archaeology appeal to a range of postpositivist critics within the tradition of analytic philosophy, most notably Popper and Toulmin (e.g., Peebles 1992) and some of the early scientific realists (e.g., Gibbon 1989), as well as to Continental philosophers, especially those associated with critical theory (Habermas), phenomenology (Husserl, Heidegger), and philosophical hermeneutics (Gadamer).[4]

Although philosophers of science took little systematic interest in archaeology before the 1970s, some influential nineteenth-century studies of science include substantial discussion of archaeology, and intriguing references to archaeology regularly crop up in more recent work.[5] Collingwood stands out as one important twentieth-century philosopher who had a long-standing interest in and reciprocal influence on archaeology. He was directly involved in archaeological research throughout his life, beginning as a child with excavations directed by his father; he later pursued a long-term program of research on the archaeological history of Roman Britain (Collingwood 1978 [1939]: 120–146; Collingwood and Richmond 1969; Collingwood and Myers 1936). His examples of historical reasoning, which frequently involve the use of material evidence, are in this regard distinctive in the philosophical literature on history (Collingwood 1978 [1939], 1946: 205–334). And he was quite explicit about

the philosophical lessons taught by archaeological practice:

> It was time [by the mid-1930s] to begin arranging and publishing the lessons which all this archaeological and historical work had taught me about the philosophy of history. . . . For example, long practice in excavation had taught me that one condition—indeed the most important condition—of success was that the person responsible for any piece of digging, however small and however large, should know exactly what he wants to find out, and then decide what kind of digging will show it to him. This was the central principle of my "logic of question and answer" as applied to archaeology. (1978 [1939]: 121–122)

Although Collingwood has not been especially prominent in recent archaeological debates, Clarke drew on his nuanced analysis of observation as early as 1970; in addition, his account of historical interpretation has been an important inspiration for some critics of the New Archaeology (e.g., Hodder 1991).

But despite these points of contact it was not until the 1970s, with the advent of the New Archaeology, that there developed a sustained engagement between philosophers (especially analytic philosophers of science) and archaeologists.[6] The catalyst for this new and initially rocky relationship was the use made by the New Archaeologists of the logical positivist models of explanation and confirmation that they drew primarily from the work of Hempel. Such logical positivism was, by the 1960s, the "received view" in philosophy of science (Suppe 1977b), the product of fifty years of careful rational reconstruction of the logic of scientific reasoning. It was rooted in the empiricist conviction that legitimate knowledge is properly grounded in experience; inspired by the tools of formal analysis developed by Frege, and by Russell and Whitehead, at the turn of the century; and shaped by the modernist zeal of the Vienna Circle for establishing clear criteria of demarcation by which to separate genuine science from metaphysics, idealism, and pseudo-science (see "Philosophical Interlude" in chapter 1 for a more detailed account of the sources and legacy of logical positivism/empiricism). Above all, the New Archaeologists sought a way of articulating their vision of a scientific archaeology that would set them apart from traditional archaeology, which they characterized as mired in data recovery and description.[7] On their diagnosis, traditional archaeologists were resolute inductivists in their approach to research; they made it their first priority to assemble and systematize the observable facts of the archaeological record on the principle that conclusions about past lifeways could be drawn only when all the relevant facts were in. This put them in an impossible position. Given the limitations of archaeological data, they were forced either to defer interpretation and explanation indefinitely, giving up the ambition of advancing inquiry beyond empirical description of the record itself, or to venture conclusions that amount to little more than "just so" stories. The New Archaeologists saw in Hempelian deductivism a vehicle for articulating ambitions that went well beyond the cautious inductivism of traditional archaeology without sacrificing its commitment to a rigorously empirical form of inquiry.

The hallmark of the New Archaeology was its strongly positive attitude (P. Watson, LeBlanc, and Redman 1971) about the prospects for using the archaeological record to understand the cultural past. In particular, the New Archaeologists insisted that their objective should be to produce not just richer, more accurate *descriptions* of culture history and past lifeways but rather an *explanatory* understanding of the underlying structure and dynamics of cultural systems—the cultural processes—that are responsible for the forms of life and trajectories of development documented by culture historians. As Flannery put it in an early assessment of processual archaeology: "the process theorist is not ultimately concerned with 'the Indian behind the artifact' but rather with the system behind both the Indian and the artifact" (1967: 120). Although this might seem a daunting ambition, the New Archaeologists were motivated by a strong conviction that the limitations of their enterprise lie not so much in the record itself as in the ingenuity and resources that archaeologists bring to its investigation (L. Binford 1968a: 22); if they could develop more effective ways to use archaeological data as evidence of the past, they would have at hand a resource of unparalleled time depth and global scale for developing general theories about social and cultural systems.

When articulated in positivist, Hempelian terms, this ambition of realizing a different kind of understanding of the past took the form of a directive to address the challenge of establishing

law-governed deductive explanations for the cultures archaeologists study. According to positivist theories of science, an event is explained when you can show that it was to be expected, given laws that specify what conditions must hold for events of this kind to occur; the claim that one event or set of conditions was responsible for another is genuinely explanatory only if you can identify a lawlike regularity linking the two. By extension of Hempel's classic examples of "explanation sketches" in history (1942), this requires archaeologists to show that specific features of the cultures they study—their technologies, forms of subsistence, social relations, material culture—fit invariant regularities in the patterns of organization and development (the cultural processes) that are typical for cultural systems generally or for the types of cultural systems under investigation. New Archaeologists applied this covering law model of explanation not just to the ultimate (anthropological, processual) goals of the discipline but to all the intermediate levels at which archaeologists rely on reconstructive inference as well. Their central objective was to establish general laws of cultural process capable of explaining large-scale, long-term cultural dynamics, but at the same time they were committed to reframing claims about specific cultural events and forms of life—the particulars to be explained in processual terms—as explanatory hypotheses backed by more narrowly specified laws, some of them developed by archaeologists but many derived from other fields and concerned with noncultural dimensions of human life.

The conviction that cultural systems and processes can be treated as law-governed phenomena at all these levels of analysis was both reflected in and supported by a collateral commitment of the New Archaeology to an ecosystem theory of culture. In standard formulations, this model suggests that all aspects of cultural life are interrelated and can be understood as a function of adaptive responses of the system to its external environment (e.g., L. Binford 1972a). At their most extreme, the advocates of an ecosystem approach, and now of an evolutionary archaeology (Dunnell 1989a, 1992; Lyman and O'Brien 1998; see the discussion of this approach in Schiffer 1996), endorse an explicitly reductive materialism and functionalism according to which the

intentional aspects of cultural systems—the animating beliefs and intentions of cultural agents, and the symbolic or ideational dimensions of collective cultural life—are ruled out of consideration both because they are considered inaccessible to properly scientific investigation and because, as dependent variables, they are presumed to be explanatorily irrelevant for understanding the form and development of cultural systems (L. Binford 1983: 12).

The New Archaeology was also characterized by commitment to the goal of setting explanations of the cultural past on a firm empirical foundation. As critics of traditional inductivist modes of practice, New Archaeologists found especially compelling the condemnation of speculative theorizing that was a prominent feature of logical positivism. Certainly their arguments for systematic testing reflect the empiricist view, which twentieth-century logical positivists tried to make precise, that meaningful ("cognitively significant") knowledge claims must be held accountable to observation. But few were prepared to embrace the stronger prohibition of "speculation after unobservables" associated with nineteenth-century classical positivism, or the requirement, developed by the most stringent of twentieth-century logical positivists, that theoretical claims must be reducible to the observables that they systematize. These strands of positivist thinking sit uneasily with the ambitions of the New Archaeology—specifically, their uncompromising rejection of the descriptivist tendencies of traditional archaeology and their concern to make underlying cultural processes the primary subject of investigation. Hempel himself endorsed the positivist view that the proper goal of scientific inquiry is to systematize observables: "scientific systematization is ultimately aimed at establishing explanatory and predictive order among the bewilderingly complex 'data' of our experience, the phenomena that can be 'directly observed' by us" (1958: 41). But here, too, the New Archaeologists interpreted positivist directives in liberal terms. The key to securing archaeological knowledge claims empirically, they argued, was to treat all aspects of archaeological research as a problem-oriented testing program. Explanatory hypotheses should stand at the beginning of inquiry, as its point of departure, rather than emerge inductively at the conclusion of the enterprise af-

ter all the data are collected and analyzed. Following the broad outlines of a hypothetico-deductive model of confirmation, the New Archaeologists required that prospective explanations be articulated clearly enough to (deductively) entail test implications concerning the surviving archaeological record. Archaeological survey, excavation, and data analysis were all to be designed to determine whether those implications were born out, that is, whether the archaeological record actually contains the kinds of evidence that should be present if, in fact, it was produced by the events or conditions postulated by the test hypothesis.

The clean deductivist profile of this program was complicated and qualified in innumerable ways. In chapters 3, 4, and 7, I describe the senses in which the program of scientific practice advocated by the New Archaeology was never as straightforward as its positivist rhetoric might suggest. Indeed, my thesis is that by virtue of its positivism, the New Archaeology was compromised by fundamental contradictions—conceptual and practical—that necessarily generated precisely the kind of internal crisis of confidence and sharp external reaction that emerged in the early 1980s, heralded by anti- and postprocessual critics of various stripes. For better or for worse, however, the deductivist ideals associated with Hempelian positivism provided a powerfully galvanizing rhetoric that the New Archaeologists used to articulate the main lines of programmatic argument that defined their vision of a scientific archaeology.

When the positivist commitments of the New Archaeology were made explicit in the early 1970s (e.g., in J. Fritz and Plog 1970; P. Watson, LeBlanc, and Redman 1971; Leone 1972a), they generated intense debate within archaeology. Many questioned the applicability of Hempelian models to archaeological practice; some pointed to internal philosophical problems with this family of models, drawing on the arguments of postpositivist philosophers and historians of science; still others raised more general questions about the relevance of philosophy to archaeology. Philosophers were involved from the beginning. Some were sympathetic to the goals of the New Archaeology and undertook to elaborate and refine the philosophical underpinnings of the program in publications addressed primarily to archaeologists.[8] Others used these internal debates as a point of departure for analyses aimed primarily at a philosophical audience: they expanded on and sometimes challenged the positivist models invoked by the New Archaeologists, using examples drawn from archaeological practice.[9]

At the same time, however, several less constructive interventions were published by philosophers who reacted sharply to errors in the archaeological literature that reflected, in their view, the grossest of philosophical ignorance. The most contentious of these were rebuttals to two extended arguments for positivist models published by New Archaeologists in the early 1970s: Morgan's review of *Explanation in Archaeology: An Explicitly Scientific Approach* (P. Watson, LeBlanc, and Redman 1971) and the exchange that ensued (Morgan 1973, 1974, 1978; P. Watson, LeBlanc, and Redman 1974); and Levin's response (1973) to John Fritz and Plog's account (1970) of covering law explanation. Morgan and Levin objected that the New Archaeologists were dangerously naive about the state of philosophical debate; by the time they embraced late logical positivism, it was in disarray. It had been progressively undermined, first by intractable conceptual difficulties that were already evident in the 1930s and 1940s and then by a proliferation of internal critiques dating to the 1950s that brought the central tenets of the program under attack.[10]

By the late 1960s and early 1970s, when the New Archaeologists invoked Hempelian models, this growing internal crisis was reinforced by several important external critiques. The most prominent of these was Kuhn's historical argument that all aspects of science, including its evidence, are paradigm dependent (1970). Also important, though less widely influential, was the Wittgensteinian account of theory-ladenness elaborated by Norwood Russell Hanson (1958), and a range of related arguments for what Suppe described as *Weltanschauungen* (worldview) theories of science (Suppe 1977a, 1977b). These contextualist critics challenged the conviction, central to logical positivism and to empiricist theories generally, that observational evidence constitutes a stable foundation on which systems of empirical knowledge can be securely built, a body of experientially given facts that are clearly distinct from any theoretical claims that might be based on it or tested against

it. They showed, in quite different ways, that observations are theory-laden and richly dependent on extended networks of theoretical claims and assumptions—"webs of belief" (Quine and Ullian 1978)—that include generalizations about observables as well as claims about unobservable dimensions of the reality under study (e.g., its microconstituents, underlying mechanisms and connections, and emergent macroprocesses). These constitute a conceptual framework without which observations have no meaning or evidential import—indeed, without which they cannot be identified as observations. This contextualist thesis was generalized and given historical dimension by Kuhn.

Ironically, Hempel played a pivotal, if unintended, role in the widely touted demise of positivism heralded by these contextualist critiques. Using the tools of exact philosophy that were the hallmark of logical positivism and of analytic philosophy generally, he exposed many of the difficulties that later proved decisive. He amended his original deductive-nomological model of explanation to accommodate inductive-statistical patterns of explanation, and he grappled (unsuccessfully) with problems posed by the nondeductive forms of inference required to establish the import of evidence for a test hypothesis on the hypothetico-deductive model of confirmation. Most significant, he acknowledged the profound difficulties positivists face in reconciling their accounts of cognitive significance, which require that the content of scientific theories must be reducible to (or translatable into) descriptions of the phenomena they subsume, with the richly theoretical claims of contemporary physics (Hempel 1965). In the passage cited earlier, in which Hempel reaffirms the positivist/empiricist view that the "ultimate aim" of science is to systematize observables, he goes on to observe, "It is a remarkable fact, therefore, that the greatest advances in scientific systematization have not been accomplished by means of laws referring explicitly to *observables,* but rather by means of laws that speak of various *hypothetical,* or *theoretical, entities,* i.e., presumptive objects, events, and attributes which cannot be perceived or otherwise directly observed by us" (1958: 41; emphasis in the original). It is with an air of profound puzzlement that Hempel pursues the question of how one might make sense of the propensity of scientists to indulge in "de-

tour[s] through the realm of not directly observable things, events, or characteristics" (49) in terms that are consistent with logical positivist convictions about the nature and goals of science. His answer, ultimately, is that these detours can be explained and justified only on pragmatic grounds; speculating about unobservables is a heuristic that aids in systematizing observables. He did not consider the possibility that such theorizing might actually be constitutive of the goals, explanatory understanding, and even the observational base of science; this was later suggested by Kuhn and Hanson and by some of the realist critics of received view positivism and empiricism.

The most vocal philosophical critics of the New Archaeology objected not only that archaeologists had ignored these important arguments against logical positivism but that they did not appreciate the open-ended, disputatious nature of the philosophical enterprise. As Nickles put it, more judiciously than some, the models of scientific explanation and confirmation that had inspired the New Archaeologists should be treated as "theses to be argued," not as "established truths" (1977: 164); they cannot be detached from ongoing philosophical debate and applied to practice as an authoritative definition of what it is to be scientific.

Although the main philosophical objections to logical positivism were also raised by internal, archaeological critics, the caustic tone adopted by some of the philosophers who entered the first rounds of debate rankled for most of a decade. In the early 1980s the hostility of these exchanges was still widely commented on by archaeologists such as Schiffer (1981) and, indirectly, Flannery (1982), who called for more practical, down-to-earth philosophical analysis. In some quarters practice-minded archaeologists declared a plague on all houses and withdrew from theoretical debate altogether. It held nothing for them and had manifestly failed to deliver clear-cut answers to their quandaries. Others took up Clarke's call for the internal analysis of archaeological problems. For example, Bruce Smith (1977) and Sabloff, Beale, and Kurland (1973) distinguished the promising new initiatives that were emerging in practice from the positivist rhetoric used to justify them, arguing that it was only through careful analysis of the former that a conceptual framework appropriate to the goals of the New Archae-

ology could be developed. And Flannery (1982) insisted that any reflective wisdom required to improve archaeological practice was best gained from senior archaeologists functioning as seasoned coaches. Some philosophers responded in kind, declaring that philosophy proper concerns problems that, by definition, have no bearing on scientific practice. This is a central theme in Richard A. Watson's later defenses of the New Archaeology against postprocessual critics (1990, 1991). It is also taken up by Embree, who, despite his allegiance to a very different philosophical tradition (phenomenology), agrees that what philosophers do is fundamentally different from what archaeologists do, even when they seem to engage the same issues (e.g., 1989b, 1992). I respond to these arguments in chapter 6.

At the same time, however, a number of philosophers and archaeologists did persist in discussion across disciplinary boundaries; by the mid-1980s, they had begun to break the tyranny of the asymmetrical pattern of exchange that put archaeologists in the position of importing readymade philosophical models, often ill-suited to their needs, and philosophers in the role of external experts, offering corrective advice. Increasingly philosophers are working collaboratively with archaeologists and, in the process, they have moved substantially beyond model fitting and critical commentary. The demands of constructive engagement bring into play a much-expanded range of philosophical perspectives and often require the development of innovative models that do not conform to any established philosophical tradition of thinking about science. In 1992 Embree argued that this growing body of work had achieved sufficient maturity to be recognized as a subfield that he designated "metaarchaeology": a loose-knit family of research programs that make use of historical and sociological as well as philosophical modes of inquiry (both analytic and Continental) to address second-order questions about archaeological practice. A year later Merrilee Salmon distinguished "analytic philosophy of archaeology" from "philosophical approaches to archaeology" (1993: 324), characterizing the former as an established field of practice concerned with "metaphysical, epistemological, ethical, and aesthetic problems that arise in the theory and practice of archaeology" (323). I am concerned here, and throughout, with analytic metaarchaeol-

ogy as it has been engaged by both philosophers and archaeologists.

THE EMPIRICAL GROUNDING OF PHILOSOPHY OF SCIENCE

Although this hybrid subspeciality, analytic metaarchaeology, has emerged primarily from debates within archaeology, it is also very much a product of developments in philosophy of science that have unfolded in the last thirty years in response to the demise of positivism. I consider here, and in a discussion of logical positivism and empiricism in chapter 1, some philosophical counterparts to the archaeological debates just outlined. At the end of this introduction I return to the question of what it is that constitutes the problematic of analytic metaarchaeology and identify a number of issues that have been central to its formation since the early 1980s.

AMPHIBIOUS PHILOSOPHY OF SCIENCE

One diagnosis of the many ills of twentieth-century logical positivism is that it had foundered on puzzles-cum-anomalies generated by its own formalism; it had lost touch with "real science." What defines postpositivist philosophy of science has been, above all, the rejection of abstract a priori analyses of science and a commitment to develop a philosophical understanding of science that is grounded in the sciences themselves in two quite distinct senses.

The first sense in which philosophy of science has turned to science is now unexceptional. It takes the form of a reaction against philosophical models based on what Glymour refers to as a "fantasy image of physics" (1980: 292): this "science fiction philosophy," as Bunge had earlier characterized it (1973: 18), proceeds, apologetically or naively, by fitting science to preconceived philosophical models or by spinning out rational reconstructions of science as it is presented in popular overviews, prefaces, textbooks, stock historical examples, and isolated cases (Bunge's examples, 1973: 1–23). The antidote, widely endorsed in the decade immediately following the appearance of Norwood Russell Hanson's *Patterns of Discovery* (1958) and Kuhn's *Structure of Scientific Revolutions* (1970 [1st ed., 1962]), was to insist that philosophy of science must be grounded in

a substantial understanding of the technical, empirical, and theoretical details of actual science, current or historical. This inspired the much-contested marriage between history and philosophy of science that spawned the History and Philosophy of Science (HPS) programs in which many contemporary philosophers of science have been trained (Nickles 1995: 140), and it has been the impetus, more generally, for the widespread conviction that philosophers of science must be trained as partial insiders to, if not practitioners of, the sciences they study. As Bunge describes it, the properly a posteriori study of science requires the training of "amphibious" philosophers who have as much depth in science as in philosophy, who "work in—not just study—some science," and whose metascientific questions arise from critical reflection on science as much as from critical reflection on philosophy (1973: 16).[11]

To see the significance of these proposals as a break with the forms of philosophical analysis that had come to dominate philosophy of science, consider the lead essay in the inaugural issue of *Philosophy of Science* (published in 1934), in which Carnap defined the goals of this fledgling field and set the agenda for his own logical empiricism. He began by asking how philosophical problems differ systematically from those addressed by the empirical investigator,[12] and concluded that they are distinctive not only because they arise when you "step back and take science itself as the object" but because they concern science "only from the logical point of view" (1934: 6). The proper subject of inquiry for *philosophy* of science is the language of science (specifically, its syntax), not its content; no other questions are "discussable" (17).[13]

Although many philosophers of science retain the ambition of carving out a niche for their field that sets it clearly apart from the sciences, and many still regard formal analysis as the key to this distinctive identity, even loyalists to the cause of logical empiricism had largely abandoned Carnap's prohibition against considering questions of content by the 1950s. In 1954, for example, Suppes recommended that if philosophers of science were to achieve successes comparable to those that had distinguished mathematical logic—if they were to secure a "hard core" of formal results—they must "set themselves the task of axiomatizing the theory of all developed branches of the empirical science" (1954: 246).[14] Suppes here applauds Carnap's pioneering work in philosophy of science—it was certainly rigorously formal—but he does not recommend it as a model for current practice because Carnap had not analyzed the particulars of any of the developed physical sciences (245). Suppes's was both a more grounded and a more expansive vision. He urged that the tools of exact philosophy be extended to precisely the questions of content that Carnap had set aside, thereby requiring philosophers to wrestle with the details of particular scientific theories and types of inquiry.

By the mid-1970s, when Bunge declared the need for fully amphibious philosophers of science, many still conceptualized the field as a second-order metaenterprise that requires its practitioners to "step back and take science itself as the object," as Carnap had put it forty years earlier (1934: 6); but they did not distinguish their interests from those of scientists nearly so sharply as did Carnap. As Suppes had recommended, questions about the conceptual foundations of science (its metaphysical and theoretical presuppositions) were central to philosophy of science, as were questions about scientific methodology; and increasingly, questions of both sorts derived as much from the practice of science as from philosophical traditions of reflection on science. In addition, however, it had become an article of faith that philosophers' theories of science should meet at least minimal requirements of descriptive adequacy with respect to the sciences whose presuppositions or practices they purport to describe and explain.[15] There was growing impatience with the "logical 'escapism'" (McMullin 1970: 14) in which formal analysis turned inward, to problems of its own creation, whatever dimensions of science were the focus of philosophical inquiry: the Carnapian tradition "got entangled with itself (paradox of confirmation; counterfactuals; grue) so that the main issue is now its own survival and *not* the structure of science" (Feyerabend 1970: 181; emphasis in the original).[16] Many shared the assessment, put most forcefully by McMullin, that the resulting technical analyses do not constitute philosophy of *science;*[17] they provide little understanding of "how scientists actually operate" (1970: 13–14) and, in the process, forgo the possibility of contributing constructive, creative insights "that would enable us to attack important scientific problems in a new way or to better un-

derstand the manner in which progress was made in the past" (Feyerabend 1970: 181; see full quote in n. 16). The questions of what constitutes empirical adequacy where philosophical analysis is concerned and what accountability philosophers have to science are centrally at issue in the uneasy relationship between history and philosophy of science (e.g., Giere 1973; Burian 1977; Nickles 1995). They are also at the heart of the second move to ground philosophy in science that characterizes postpositivist philosophy of science.

NATURALIZING TRENDS

The second sense in which philosophy of science has drawn closer to the sciences is a matter of active current debate: many now insist that philosophy of science should be naturalized, and perhaps also socialized or humanized. By this they mean that the philosophical study of science should be more closely integrated with various forms of empirical research that take as their subject matter scientific practice and the various cognitive, behavioral, social, institutional, and historical conditions that make science possible. The point of departure for naturalizing arguments is typically rejection of the Carnapian view that philosophy is, properly, an autonomous enterprise. Naturalizers insist that the questions philosophers address are, at least in part, *empirical* questions about the goals, production, and justification of scientific knowledge that arise in the context of scientific practice and can only be effectively addressed using the tools and resources of the sciences themselves (see Callebaut 1993). Often this naturalizing turn grows directly out of the requirement that philosophy be grounded in the sciences in the first sense. As McMullin argues, the challenge of ensuring that philosophical analysis makes sense of actual scientific practice is a relatively empirical undertaking; in this respect philosophy is "not very different . . . from an empirical science itself" (1970: 27). Beyond these very general motivating commitments, however, naturalizers are divided on a number of fundamental issues. They hold widely variant views on the question of which scientific disciplines they should naturalize to. In particular, should these be limited to the behavioral and cognitive sciences, and perhaps other areas of psychology and neuroscience, or should they include various social sciences and history?

They also disagree about the degree or kind of continuity they should seek between empirical and philosophical inquiry (Kornblith 1994; Schmitt 1994). And, crucially, they differ on the question of whether normative questions or, more generally, the "advice-giving" role of philosophy in our lives is consistent with a thoroughgoing naturalism; many resist the view that it should be eliminated in favor of empirical studies of science but see little prospect for reframing normative inquiry in empirical terms (see, e.g., Maffie 1990).

The main inspiration for naturalizing moves in contemporary philosophy of science is Quine's essay "Epistemology Naturalized" (1969), specifically his arguments that epistemology should be conceived as science self-applied. But there are a number of intriguing antecedents in philosophy of science. One that offers a particularly striking contrast to Carnap's 1934 discussion is a programmatic article that appeared in *Philosophy of Science* in 1938 in which Benjamin insists that "the data and problems of philosophy of science can be found in science itself" (1938: 422). He goes on to argue that the central problems of the enterprise—those of "describ[ing] and explain[ing] science"—can be effectively addressed only if philosophers make full use of the methods of inquiry and insights about "scientific cognition" that have been developed by various sciences (423). Evidently there were naturalizing impulses present in philosophy of science well before postpositivists took up the Quinean project of "reciprocal containment," reconceiving epistemology and science so that each informs the other (Quine 1969: 83).

With this second move to ground philosophy in science, philosophers of science are required not only to establish substantial insider knowledge of the scientific discipline(s) they study but also to develop, or to avail themselves of, the expertise of a range of other (collateral) fields that offer an empirical understanding of how these subject sciences operate, how they produce what we count as scientific knowledge. A resolute naturalizer must be prepared to draw on the work of psychologists and cognitive scientists who investigate the individual capacities necessary to do science, as well as to undertake social and historical studies of the conditions that make possible the exercise of these capacities in particular forms of scientific inquiry. On the most thoroughgoing of naturalisms, neither the problems nor the methods of

philosophy of science are considered distinctively philosophical, though they may arise from traditional philosophical questions. Although I endorse the impulse to naturalize, suitably broadened to include "reciprocal containment" by social as well as natural sciences, I resist the arguments, summarized by Maffie, that "unlimited naturalists" use to reject what he describes as "stock philosophical methods, e.g., conceptual analysis, reflective equilibrium, or intuitionism as non-naturalistic and of dubious epistemic merit" (1990: 288). It seems unlikely that there are any methods of inquiry that are uniquely or exclusively philosophical, not because the strategies of inquiry associated with philosophy are bankrupt or eliminable in favor of empirical, scientific methods but rather because standard philosophical methods are a crucial component of effective scientific practice, including those that philosophers use to negotiate normative issues. Nowhere is this clearer than in archaeology.

Together, these two rather different moves to ground philosophy of science in science are transforming the enterprise as a whole. For example, a great deal of work in the empiricist/positivist tradition has been predicated on a commitment to the unity of science as a working hypothesis for philosophy of science (Oppenheim and Putman 1958). Sometimes this takes the form of a methodological thesis, in which case the goal is to identify distinctively scientific methods or forms of reasoning; more often it is framed as a metaphysical or epistemic thesis about the essential unity of the subject domains studied by the sciences and the prospective integration (often by piecemeal reduction) of the content of various sciences (for the details, see chapter 15). When philosophers began (again) to attend to real science, they confronted a degree of complexity and diversity in scientific practice that has significantly undermined faith that the sciences embody a common method and form of rationality, or that they can be expected to produce domain-specific theories that will ultimately converge on a comprehensive, unified system of knowledge (e.g., through a series of interfield reductions), or even that the world they study is itself systematically structured in the manner required by theses of metaphysical unity (Dupré 1993).[18] As attention has shifted from features that unify the sciences to those that distinguish them or that link them in more localized and piece-

meal ways, vigorous programs of research have emerged that treat an increasingly wide range of scientific disciplines as philosophically interesting in their own right, not just as an export destination or a resource for testing highly abstract models of science based primarily on physics; substantial bodies of philosophical literature, conferences, journals, and in some cases autonomous professional societies have crystallized around the life sciences, the earth sciences, and various of the social sciences.

The empirical and conceptual results of these science-specific research programs reinforce theoretical arguments against residual positivist presuppositions about the unity of science. And this outcome, in turn, reinforces the move to naturalize (and socialize) philosophy of science. As philosophers learn more about the specifics of diverse sciences, they have had to take seriously the influence of a range of contextual and historical factors—the psychological and social dynamics, political economy, and institutional settings that are constitutive of scientific practice—that were excluded from positivist/empiricist analyses of science because they were considered to be nonepistemic. Time and again, even the best science proves to be inexplicable in terms of evidence and good reasons alone; its successes, even its distinctive epistemic attributes, are shaped by noncognitive factors that philosophers have been prepared to invoke only to explain the failure, the miscarriage of science.[19] In short, close attention to the sciences in all their diversity has done more than abstract argument ever could to expose the limitations of received view philosophy of science and to establish the need for much richer, more multidimensional and hybrid models of scientific practice and its products.

In this period, postpositivist *sociology* of science has converged, from the opposite direction, on some of the same insights as are transforming philosophy of science. Through the 1960s and 1970s a critical sociology of scientific knowledge (SSK) produced a series of empirical challenges to the ideals of logical positivism that are embodied not only in received view philosophy of science but also in the functionalist approach to sociology of science developed by Merton in the 1940s to 1960s (1973), and in the legitimating images of science embraced by many defenders and advocates of science. SSK practitioners argued that far

from transcending the play of local, contextual interests, virtually all epistemic concepts and ideals can be reduced to sociological factors (see, e.g., Barnes and Bloor 1982). One argument for this position proceeds from philosophical premises: specifically, from the arguments developed by contextualists concerning the theory-ladenness of evidence, the interdependence of observational and theoretical claims more generally (the Quine-Duhem thesis of holism), and the empirical undecidability of many theoretical questions (the underdetermination of theory by evidence). If it is the case that interesting hypotheses invariably overreach the available evidence, perhaps even all imaginable evidence, and that the evidence itself is often ambiguous—it can be used to support or refute a test hypothesis only under interpretation, given the mediation of auxiliary hypotheses and ladening theories—then it follows that empirical adequacy alone cannot account for the choices scientists make among competing hypotheses. With ingenuity, alternative hypotheses can always be formulated that account for the evidence just as well as the hypotheses we favor. At the very least, scientific judgments about the credibility of these alternatives must depend on additional considerations such as their explanatory power, simplicity, internal coherence, and intra-theoretic consistency. Considerations such as these are manifestly conventional; they are subject to shifts in interpretation and relative weighting that are notoriously context- and problem-specific. It seems unavoidable, then, that all science, not just failed science, is much more open-ended and much more profoundly shaped by contextual factors—including social and historical factors, as well as methodological conventions and theoretical commitments—than had been acknowledged by traditional positivist and empiricist philosophers of science.

The advocates of SSK extend this line of argument, insisting that contextual factors—interests, conventions, and sociopolitical, economic, and institutional conditions of practice—not only enter into the process of formulating questions, generating hypotheses, and determining how to use scientific knowledge; they also play a key role in the evaluation of scientific hypotheses. They establish what counts as evidence and they shape all other aspects of the judgments by which the credibility of knowledge claims is assessed.[20] These are pre-

cisely the cognitive, epistemic dimensions that logical positivists and empiricists insist must be free from the contaminating influence of noncognitive factors if inquiry is to count as genuinely scientific.[21] Given this principled argument for a contextualism that foregrounds the social determinants of inquiry, SSK practitioners undertook to demonstrate, through detailed case studies, that virtually all epistemic concepts and standards—the internal cognitive or constitutive values of truth, evidence, sound argument, and objectivity—are, in fact, contingent cultural and historical conventions that reflect local social and political interests; they argue for a form of social constructivism. The conclusion they draw from such research is that the special epistemic authority of science arises from an exceptionalism that cannot be sustained. Scientific knowledge is accorded special status in part because it is presumed to embody a "view from nowhere" (T. Nagel 1986), and yet scientists never escape the social, historical conditions under which they practice. It is a mistake to assume that the knowledge produced by science can be justified by appeal to abstract, context-transcendent epistemic standards; such metaclaims should be subject to the same kind of critical, empirical scrutiny as scientists require for all other beliefs.

The most radical challenges issued by SSK practitioners in the 1970s and 1980s have been substantially refined and qualified over time. The results of their own fine-grained sociological and historical research suggest that the sociological reduction of science to external (noncognitive) factors is just as untenable as its philosophical reduction to internal epistemic (cognitive) factors. Reviewing the state of sociology of science in 1992, Pickering objects that the traditional SSK program was compromised from the outset by a conception of science he describes as "idealized," "abstract," "thin," and "reductive"; as a consequence, "SSK simply does not offer us the conceptual apparatuses needed to catch up the richness of the doing of science[;] . . . to describe practice as open and interested is at best to scratch its surface" (1992a: 5). In many respects SSK emerges in Pickering's account as the mirror image of the received view of philosophy of science it was intended to displace. Sociologists and philosophers of science alike now increasingly argue that if you take seriously the complex and multi-

dimensional nature of the sciences—a feature of science that is inescapable when you attend to its details—you must give up the expectation that they can be understood in strictly sociological or philosophical terms, or indeed in terms of any single discipline-specific approach to the study of science (e.g., historical, psychological, anthropological, political, economic). As Pickering puts it, scientific practice "cuts very deeply across disciplinary boundaries. . . . [It] is situated and evolves right on the boundary, at the point of intersection, of the material, social, conceptual (and so on) worlds" (1990: 710). It is simply implausible that any one of the existing science studies disciplines could do justice to such a subject, taken on its own; each is inherently limited and dependent, ultimately, on the tools and insights of the others.

Thus the challenge that confronts postpositivist science studies scholars—philosophers, historians, and sociologists alike—is to recast our problems and develop categories of analysis that are adequate to the "multiplicity, patchiness, and heterogeneity" of actual science and its practice (Pickering 1992a: 8). The first example Pickering gives of a promising shift in this direction is the work of a philosopher: Ian Hacking's "philosophy of experiment" (Hacking 1988a, 1988b, 1992a). What Hacking describes as a "down-to-earth materialism" (1992a: 30) is an approach to the study of science that treats it as a body of practice made up of constellations of instrumental and interpretive procedures, natural phenomena, and theoretical understandings (Pickering 1992a: 10)— what Hacking calls complexes of ideas, things, and marks (1992a: 44)—that evolve over time and stabilize, in specific contexts, in "self-vindicating" structures (Hacking 1992a: 35; Pickering 1992a: 10). A striking feature of this approach is that once the contingencies of scientific practice are foregrounded,[22] the really puzzling aspect of science is not so much that its history reveals continuous and sometimes dramatic change—this is to be expected if the enterprise is understood in the historical, cultural terms Hacking recommends—but that this history also testifies to substantial stability in some aspects of scientific method and results. There is, Hacking observes, an "extraordinary amount of permanent knowledge, devices, and practice" produced by the sciences (1992a: 30), knowledge that remains stable through changes in styles of reasoning (1985,

1996). The durability and success of scientific inquiry require jointly philosophical, historical, and sociopolitical explanation just as much as do its transformations and failures.

Hacking's is a resolutely amphibious and naturalized (or, properly, historicized and socialized) program of science studies research; he combines insider knowledge of the technical practice and conceptual (theoretical, empirical) content of the research programs he studies with a historical, sociological interest in the processes by which all their constituent elements were brought into being, and into durable connection with one another. At the same time, this is not strictly a descriptive undertaking; normative questions about the political entanglements and ethical implications of the constellations he examines are never far from the surface, as he traces the legacy of research driven by military interests (Hacking 1986) and the real-life implications of social scientific constructs that play a pivotal role in "making up" kinds of people (Hacking 1992b, 1995).

The upshot, then, is that for those engaged in philosophical science studies, now more than ever before the question of just what sorts of factors contingently shape the practices, goals, standards, regulative ideals, and products of the sciences is genuinely open-ended—an empirical, a posteriori question. Philosophy is thus returned to active engagement with the sciences on several dimensions, an engagement that in turn erodes the boundaries dividing the various fields of science studies among themselves.

ANALYTIC METAARCHAEOLOGY

At stake in the "demise of positivism" was not just a localized crisis of confidence about a particular family of models of science (those of logical positivism and empiricism) but a way of studying science and, associated with this, the animating self-conception of philosophy of science. This contestation of both the content and the practice of received view philosophy of science was pivotal in shaping the terms of recent philosophical engagement with archaeology; in many respects philosophical metaarchaeology exemplifies the reorientations of theory and practice that are transforming philosophical science studies.

Emerging in the 1970s, analytic philosophy of archaeology was from the start a product of the

first philosophical turn to science, though this initial commitment to rapprochement was a highly local affair, taken at the initiative of archaeological practitioners as much as of philosophers. It took shape when frustration with imported models of science showed that Clarke was right to insist on the need for an internal philosophy of archaeology; any useful, sophisticated philosophical analysis of archaeology would have to be grounded in archaeology itself. At their most productive, attempts to fit archaeology to a philosophical template led to reconfiguring the template; to borrow a Lévi-Straussian phrase, archaeology has proven "good to think with," challenging philosophers to refine and extend their models of science, often by strategically complicating them. In the process, however, traditional philosophical preoccupations have often been displaced. New questions have come into focus that arise from the exigencies of archaeological practice and require philosophical analysis that begins close to the ground, reversing the direction of much that has gone before. What began as straightforwardly epistemic or methodological questions quickly led into more complex clusters of issues. It became clear that what counts as an explanation, or as compelling evidence for or against explanatory claims, depends fundamentally on theoretical, metaphysical questions: on how the cultural subject is conceptualized. And these issues are often, in turn, inflected by normative questions about ethical and political accountability. By the mid-1980s all were forced to recognize that to answer the questions generated by the interchange between philosophy and archaeology, it would be necessary to understand not just the cognitive content of the field—the knowledge it produces, its conceptual foundations, its methodology—but also the institutional contexts in which archaeology is taught and practiced, the social and political dynamics that shape this practice, and the history of its formation as an enterprise (see contributors to Gero, Lacy, and Blakey 1983, and to Christenson 1989, for arguments concerning the relevance of sociopolitical and historical analyses, respectively). Increasingly, metaarchaeology of all stripes is reframed so that its philosophical components are grounded in, and are continuous with, historical, sociological, anthropological, and, prospectively, psychological and economic studies of archaeology. Not surprisingly, a persistent theme in the literature of meta-

archaeology is a reflective concern to define its own location, role, and practice.

MODELS OF EXPLANATION

Nowhere is the diversifying evolution of metaarchaeology more clearly evident than in the course taken by analyses that began with philosophical questions about deductivist models of scientific explanation. Although external philosophical commentators are often credited with bringing the philosophical inadequacies of these models to the attention of archaeologists, in many cases the most systematic and cogent reviews of the relevant philosophical critiques were published by archaeologists, or by philosophers working in collaboration with archaeologists.[23] One early response to the Hempelian covering law model of explanation, which combined philosophical and archaeological considerations, was the systems analysis approach argued for by Tuggle, Townsend, and Riley (1972). They endorsed many of the epistemic commitments distinctive of logical positivism but rejected the requirement that explanation be law-governed; on their account, explanation is accomplished by building a formal model that captures the structure of interrelations among the variables that constitute a cultural (or culture:environment) system. There were two critical motivations for this alternative: the observation that archaeologists rarely depend on, or expect to produce, anything like the universal laws specifying invariant correlations of the sort required by Hempel, and the fact that this feature of Hempel's covering law model had been widely contested on philosophical grounds.

A complex debate unfolded through the 1970s about the merits of this systems alternative to the covering law model. Almost immediately, archaeological critics objected that explanatory understanding requires more than just the formal modeling of cultural systems; such models do not provide, in themselves, an understanding of how and why the constituent elements of a culture (and its environments) interact in the way they do, producing the specific cultural forms and histories that archaeologists study using the surviving material record (e.g., Flannery 1973). Merrilee Salmon elaborated these criticisms in philosophical terms (1978), and subsequently proposed a "causally supplemented" statistical-

relevance model of explanation in archaeology (M. Salmon and Salmon 1979; M. Salmon 1982). On this account, explanation is accomplished not by meeting the formal requirements of a particular form of argument, a logical subsumption that establishes grounds for expectation, but by identifying all the factors that make a difference, causally as well as statistically, to the occurrence of the event to be explained. This alternative to a covering law model is recommended not only because it captures the "something more" that led many archaeologists to endorse robustly processual goals, but also because it has the resources necessary to make sense of various forms of functional ascription and functional explanation that are ubiquitous in archaeology (M. Salmon 1982: 111–112). Salmon is compelled by the cases she considers to foreground a type of archaeological construct that transgresses the standard distinction between descriptive and explanatory claims, and thus requires innovative philosophical analysis.[24]

In two later analyses (Kelley and Hanen 1988; Gibbon 1989), the scope of this discussion is broadened, bringing into play additional philosophical models as well as an expanded range of other considerations. Gibbon argues for an even more robustly causalist view of explanation than that proposed by Salmon, following the line of argument developed by scientific realists in critiques of logical positivism that appeared in the 1970s; especially influential are those who advocated scientific realism as a promising framework for research in the social sciences (e.g., Bhaskar 1978, 1979; Harré 1970, 1974; Keat and Urry 1975; Pratt 1978; subsequently D. Little 1991). Contra Hempel, these realists insist that the ultimate aim of science is not to systematize observables. Identifying reliable patterns of association and succession among phenomena is, properly, a means to the larger end of building and testing theoretical models of underlying, sometimes radically unobservable, dimensions of reality: the microconstituents of the things and events we observe, and the causal mechanisms and processes that produce manifest regularities in their behavior. On this account, the "detours" through the realm of theory that puzzled Hempel are the essence of the scientific endeavor, not a heuristic concession to the complexity of its systematizing task. Explanation is indeed the primary goal of inquiry; but, realists insist, it is realized only by giving an account of the underlying causes responsible for observable phenomena, not by showing that the event to be explained fits a generalizable regularity. Gibbon recommends scientific realism as a research program, a heuristic for archaeological research on both philosophical and archaeological grounds; it resolves a number of outstanding difficulties inherent in positivist models and captures much more directly than they do the process-modeling, anthropological ambitions of the New Archaeologists (1989: 142–172).[25]

By contrast, Kelley and Hanen (1988) argue that a nonrealist, pragmatist view of research best captures the complexities of the archaeological cases they consider and offers the most compelling response to philosophical arguments against positivism. On this account, inspired in part by rebuttals to realism published in the early 1980s by van Fraassen (1980), explanation is by no means the primary goal of scientific understanding; it is a by-product of scientific inquiry that ultimately provides nothing more (or less) than a systematic description and analysis of observable phenomena. Explanations are, properly, answers to "why-questions" that deploy whatever scientifically credible information will satisfy a specific inquirer; they have no distinctive logical structure (as logical positivists/empiricists had proposed) or content (as required by realists). Consequently, what counts as an explanation is much more context- and interest-dependent than positivists or realists are generally prepared to allow (Kelley and Hanen 1988: 216–219). At the same time, explanation remains a pivotal concept in Kelley and Hanen's account. When they turn from the "more general and philosophically oriented discussion" of postpositivist debate to sustained analysis of archaeological cases, they find that explanation is crucial to inquiry but "not in the way many people have taken it to be" (276). What matters to archaeologists is not so much what form explanation should take but how to choose among alternative explanatory accounts (277). In practice, Kelley and Hanen argue, the process by which these evaluative judgments are made is irreducibly comparative; it is a matter of assessing candidate explanations on a number of dimensions. These include not only their empirical adequacy but also their plausibility, given elements of a core of beliefs that constitute a conceptual framework for inquiry. The advantage Kelley and Hanen claim

for conceptualizing inquiry in these terms—as a process of "inference to the best explanation" (Hanen and Kelley 1989)—is that it makes clear the dynamic, provisional nature of hypothesis evaluation.[26] That archaeologists can rarely establish conclusive grounds for accepting one hypothesis over others does not make their judgments wholly subjective, a matter of arbitrary speculation or convention. None of the diverse factors that enter into archaeologists' assessments is decisive, but all can provide a basis for eliminative induction by which the field of alternative hypotheses can be narrowed and nuanced judgments made about their relative credibility.

Despite taking quite different positions on the question of explanation, however, Hanen and Kelley share with Gibbon a strong commitment to broaden the scope of analytic philosophy of archaeology. All make the case that philosophical analysis cannot proceed alone; it must be aligned with an investigation of the history and sociopolitics of the discipline. The debates that unfolded in archaeology in the 1970s and 1980s demonstrate, Gibbon argues, that the problems, conventions of practice, even the epistemological commitments that define dominant "research programmes," are profoundly shaped by nonepistemic factors; in this regard, archaeology is "a more uncertain, open, challenging and perhaps anxiety-ridden enterprise than our positivist heritage has indicated" (1989: 180). If practitioners are to make well-informed choices about how to proceed under these conditions, they must not only make broader, more discerning use of the resources offered by philosophy of science but also must understand how archaeology "relate[s] to its historical and social contexts" (Gibbon 1989: 180; see also Trigger 1989b: 27–72; Meltzer 1989 and other contributors to Christenson 1989). In particular, Gibbon argues, to systematically assess the New Archaeology and its alternatives we must understand why Hempelian positivism exerted such a powerful influence on North American archaeologists in the 1960s and 1970s, and this, he insists, can be provided only by an "anthropology of [archaeological] knowledge" (1989: 178).

Kelley and Hanen argue the need for a robust sociology of archaeology on principled as well as pragmatic grounds that reflect their conviction that philosophy of science must be "rooted in the science" and, more specifically, that "the interaction between philosophy and archaeology must be an on-going, two-way exchange" (1988: 22). One of the central conclusions they draw from the range of case studies they develop is that epistemic considerations always operate in conjunction with "various non-scientific or contextual/sociological factors" in determining which ideas will be "accept[ed] into the working body of knowledge" (277). To understand archaeological practice accurately and in detail, it is crucial that these sociopolitical factors be as much the object of investigation as more traditional cognitive, epistemic considerations. Kelley and Hanen hasten to add that in advocating this holistic approach (350)—one that not only roots philosophy in archaeology but aligns it with an empirical sociology of archaeology—their purpose is not to endorse a corrosive relativism. There is a pressing need, they argue, to rethink notions of epistemic justification; philosophical approaches that conceptualize science as rational in a narrow sense are inadequate empirically and normatively (161), but so too are the constructivist conclusions drawn by advocates of the most uncompromising SSK research programs. If science is reduced to the play of (nonrational) sociopolitical and ideological factors, its growth and its successes become inexplicable (160–162). The point is to take stock, realistically, of the status of archaeological knowledge claims, and of the prospects for enhancing their accuracy, scope, and credibility, given the conditions under which they are produced.[27] Here, then, is a brief for analytic metaarchaeology that is oriented to the goal not just of understanding but also of improving archaeological practice; it is, in consequence, a resolutely hybrid and naturalized (qua socialized and humanized) enterprise.

ANTI-SCIENTISM

By the early 1980s, when philosophical studies of archaeology were taking shape, the archaeological debate about disciplinary goals was fundamentally reframed from within, by challenges from postprocessual critics. They rejected not just specific aspects of the New Archaeology as a research program but its whole scientific orientation. These challenges had the effect of displacing questions about explanation in favor of those that Kelley and Hanen foreground: questions about whether or in what sense archaeological claims

are ever justified, whether they be descriptive or explanatory.

Where the explanatory goals of archaeology are concerned, postprocessual critics focused their critical attention on the ecosystem conception of the cultural subject with which the covering law model advocated by the New Archaeologists was aligned. They rejected the programmatic claim, which Lewis Binford still defends, that archaeologists should concern themselves exclusively with interactions between cultural systems and their environments, bracketing the ethnographic lifeworld and "paleopsychology" on the grounds that it is explanatorily irrelevant (as causally inefficacious) and epistemically inaccessible. Ironically, postprocessualists offer empirical arguments for an enriched conception of the cultural subject; they point to a range of archaeological cases in which the variability evident in material culture cannot be explained in strictly functional and ecological terms, and they identify ethnohistoric contexts in which the intentional, ideational dimensions of cultural life play a crucial role in shaping the large-scale, long-term development of cultural systems (e.g., Hodder 1982a, 1983b).

With these arguments, postprocessualists renew the case for humanistic and historical approaches to archaeology that had been displaced by the New Archaeologists (e.g., as articulated by MacWhite 1956; Lowther 1962; and, most famously, by J. Hawkes 1968). If archaeologists are to understand the cultural past as *cultural,* they must grasp what Collingwood described as the "insides of actions" (Collingwood 1946; Hodder 1991)—the intentions and beliefs of agents and the systems of intersubjective meaning that inform their actions—however inscrutable or inferentially distant these may be. And to do so, they must explore strategies of inquiry that make possible the interpretive understanding of cultural material as the meaningful products of "rule-following" action, encoding or bearing meaning, rather than of "law-governed" behavior. Postprocessualists have drawn inspiration, in this connection, from symbolic and structuralist trends in anthropology (Hodder 1982a, 1982b), hermeneutics (Johnsen and Olsen 1992; Hodder 1991), phenomenology (see n. 4 to this chapter; Byers 1992, 1999; Tilley 1990, 1993; P. Watson and Fotiadis 1990), and critical theory (Leone 1982a; Preucel 1991a; see also Hesse 1992). In an elegant philo-

sophical analysis of systematic ambiguity in what archaeologists mean by "the archaeological record," Patrik (1985) identifies the interpretivist approaches emerging in the mid-1980s as part of a long-standing tradition in which archaeological material has been treated as a textual record of intentional action rather than as a fossil record that requires scientific modes of explanation.

Despite the tendency to regard strategies for "recovering mind" (Leone 1982b) as non- or anti-scientific alternatives to the New Archaeology, the case is often made that they are in fact a necessary complement to, or component of, a scientific methodology. Some of the earliest arguments for structuralist approaches came from historical archaeologists who were as intent on making their field scientific as they were on grasping the intentional dimensions of their subject (see, e.g., the commentary offered by Fitting 1977: 63–67; Deetz 1967). Consider, too, the case Trigger made in the late 1970s for recognizing that the scientific goals of the New Archaeology depend on historical reconstructions of the cultural past, that is, on culture history; he insists that it is a mistake to treat these as independent and opposed alternatives (1978). In a similar spirit Deetz advocates a pluralism that can accommodate a "scientific humanism and humanistic science" (1983), a theme that recurs in Young's argument for drawing on narrativist philosophy of history for models of interpretation in prehistoric archaeology (1988).

Increasingly, those who advocate explicitly scientific approaches, including some who have been outspoken critics of postprocessualism, declare that the tools of a scientific archaeology can and should be used to investigate the cognitive dimensions of the cultural past (Cowgill 1993; Gardin and Peebles 1992; Bell 1994; Renfrew 1982b, 1993a). Even direct heirs of the New Archaeology call for a renewal of the behavioral archaeology, originally advocated by Reid, Rathje, and Schiffer (1974), that emphasizes questions about such intangibles as religious practice (see, e.g., Walker 1995 and other contributors to Skibo, Walker, and Nielsen 1995). Some unreconstructed eco-materialists and evolutionists do reject outright any such humanizing of the cultural subject and now favor an even more reductive scientism than that originally embraced by New Archaeologists (e.g., Dunnell 1989a; O'Brien and Holland 1992, 1995; Lyman and O'Brian 1998 in response to Schiffer

1996 and Wylie 2000b). For the most part, however, all parties to the debate about the conceptual foundations of archaeology agree that if you are committed to empirical inquiry, you cannot presume to settle, a priori, questions about what factors will prove to be causally or explanatorily relevant for understanding the cultural past. Consequently, most now accept that models of explanation must be flexible enough to accommodate reconstructions of beliefs, intentions, cultural conventions, and social institutions—the ideational dimensions of human life—even though these are unlikely to be law-governed (Nickles 1977) or accessible to material-causal analysis (M. Salmon 1982; Levin 1976).

What made postprocessual arguments so contentious a challenge to the New Archaeology was the fear that if the ideational dimensions of the past are in fact radically inaccessible to scientific modes of investigation, then a commitment to understand them will force a return to the speculative induction that a resolutely scientific archaeology was meant to displace. Indeed, the most confrontational postprocessualists did endorse precisely this conclusion. They insisted that there was, in effect, nothing to lose by expanding the scope of inquiry to include even the most elusive aspects of the past; the scientific ambitions of the New Archaeology are unrealizable in any case. If archaeological evidence is inevitably theory-laden, then it must be admitted that archaeologists simply "create facts" (Hodder 1983a: 6; see also 1984a): there are thus no independent empirical grounds for testing reconstructive or explanatory claims about the cultural past (Shanks and Tilley 1987: 111). Pushing this antifoundationalism to its limit, Shanks and Tilley make the case for an uncompromising social constructivism. All claims to objectivity are a pretense; the best archaeologists can do is to make their interests explicit and hold their claims about the past politically accountable. In the end, few postprocessuals have consistently maintained so strong a constructivist line (see chapter 12). When they move beyond the critique of processual archaeology and advance counterclaims of their own, they typically endorse a pluralism; on most formulations this allows for multiple interpretations of the past but also leaves room for the judgment that some claims are more plausible than others—indeed, some are simply untenable.[28] It is as important to postprocessual-

ists as to processualists to exploit the capacity of archaeological data to selectively resist "theoretical appropriation" (Shanks and Tilley 1989: 44).

MIDDLE-RANGE THEORY AND ACTUALISTIC RESEARCH

Although these challenges to the scientism—specifically, the objectivism and foundationalism—of the New Archaeology generated a highly polarized debate that has persisted since the early 1980s, postprocessualists were not alone in raising difficult questions about the status and stability of archaeological evidence. As soon as the New Archaeologists undertook to implement the testing methodology they hoped would obviate dependence on inductive inference, they confronted the problem that to assess the implications of archaeological data for a particular test hypothesis, they had to develop "arguments of relevance" (J. Fritz 1972: 140), or "bridging arguments" (B. Smith 1977: 611), that link surviving elements of the archaeological record to the past events and conditions that produced them. In this archaeologists necessarily rely on auxiliary hypotheses—various forms of background and collateral knowledge—to establish the significance of archaeological data as evidence (M. Salmon 1975). By 1977 Lewis Binford had taken the point that archaeological data stands as evidence only under interpretation: "the scientist must use conceptual tools to evaluate alternative conceptual tools that have been advanced regarding the ways the world works" (1977b: 3). And a few years later, writing with Sabloff, he invoked Kuhn in an argument to the effect that theory-ladenness (and paradigm dependence more generally) is an unavoidable fact of scientific life (L. Binford and Sabloff 1982).

None of this undermined the positivist commitments of New Archaeologists like Binford; their confidence in empirical testing was unshaken so long as the argument could be made that there are means of rationally, empirically evaluating the background knowledge, and even the paradigms, on which archaeologists depend (e.g., L. Binford and Sabloff 1982: 139). The response of processualists, prefigured by a long-standing interest in ethnoarchaeology and experimental archaeology (e.g., the "action research" advocated by Kleindienst and Watson in 1956), was to declare that a scientific archaeology must

systematically develop, or selectively borrow, the background knowledge necessary to establish reliable arguments of relevance—"ascriptions of meaning"—to archaeological data. Some advocates of actualistic research[29] maintained the deductivism of Hempelian models; their goal was to establish universal laws capable of retrodicting past events or conditions of life from their surviving material record. For example, John Fritz's central concern, in characterizing "systems for indirect observation of the past," was to show how arguments of relevance could be formulated that "meet the requirement of deducibility[,] . . . permit[ting] us to deduce the characteristics of the data from those of the past sociocultural phenomena we hope to observe" (1972: 149). This is a theme that recurs in the literature on actualistic research and, later, in that on "middle-range theory"; it is evident in Gould's uncompromising rejection of analogical inference (1980; see discussion in chapter 9), and in Schiffer's insistence that "arguments of relevance" are "nothing less than laws of cultural process" (1972b: 155). Schiffer drew the conclusion that the first priority for a scientific archaeology must be to establish a body of universal laws governing the natural and cultural "transforms" responsible for the archaeological record (1972a, 1972b, 1975, 1976).[30]

Both philosophers and archaeologists have argued that the inability of all but a few of the claims archaeologists make about the import of archaeological evidence to meet Fritz's deducibility requirement does not entail the "hyperrelativism" associated with some forms of postprocessualism (Trigger 1989b). Archaeological claims are always defeasible, as postprocessual critics have argued; there are no absolutely stable and transparently meaningful empirical foundations on which they can be grounded. However, the very analyses that expose error demonstrate the potential for systematically adjudicating the (relative) credibility of competing hypotheses, whether they be explanatory, interpretive, or descriptive. The challenge is to articulate models of archaeological inference that capture the range of interlinked considerations bearing on these judgments.

In this spirit most philosophical commentators and a number of archaeologists have argued for a more complex and open-ended account of hypothesis evaluation than deductivist ideals allow. For example, Merrilee Salmon proposes a modified Bayesian account; she conceptualizes judgments of evidential support as a matter of assessing the difference that new evidence makes to the prior probability of a hypothesis, and the likelihood that this evidence could occur even if the hypothesis were false (1982: 49–56). Building on some early suggestions of Salmon's (1975, 1976) and anticipating her later, more fully developed account of archaeological testing, Bruce Smith (1977) argued the case for a hypothetico-analog model of evidential reasoning that puts particular emphasis on its inductive character and the role of plausibility judgments. Hanen and Kelley push this line of argument further (1989; Kelley and Hanen 1988), stressing the importance of intra-theoretic consistency—the fit of new hypotheses with a conceptual core of established and background knowledge—in judging the relative credibility of competing explanatory claims. This is an approach Gibbon shares (1989), though as a realist he regards "best explanations" as those that afford the most comprehensive and plausible causal explanation of the available data. In a sophisticated argument for "typological instrumentalism," William Adams and Ernest Adams (1991) make a case for recognizing the role played not only by background knowledge but also by pragmatic considerations in constructing typologies and other tools of analysis.[31] And in a series of analyses of the inferential processes underlying all forms of "archaeological construct," Gardin likewise eschews top-down, philosophically driven models, using what he describes as a "logicist" approach to capture the range of operations by which archaeologists proceed in even the most mundane practices of observation, description, compilation, and explanation. Despite the formalism of these models, Gardin is compelled by the practice he considers to foreground the selective, the interpretive, and even the normative dimensions of archaeological inquiry (Gardin 1980, and in Gardin and Peebles 1992; also Gallay 1989).

Increasingly, the justifications archaeologists offer for the development of middle-range theory suggest a range of alternatives that mediate between the extremes of a strict deductivism, on the one hand, and radical constructivism on the other (e.g., Tschauner 1996). This repositioning has led Kosso to argue that there is actually very little difference between the practice of processualists and that of postprocessualists; they exploit linking

principles in essentially the same ways, whether their goals are to establish causal explanations or interpretive readings of the archaeological record (1991).[32] The philosophical theory that best captures these forms of practice, Kosso argues, is a sophisticated antifoundationalism that shares a number of key features with Kelley and Hanen's broadly coherentist account (1988); although there are no self-justifying grounds for belief (empirical or otherwise), the various constituents of networks of belief constrain one another in ways that can stabilize evidential claims (Kosso 1993).

The debate continues, however. In 1994 Bell renewed the arguments of the New Archaeologists against inductivism, translating the central insights of Popper's refutationism into a set of methodological guidelines for archaeological practice. Invoking Popper's famous rejection of Vienna Circle verificationism, he urges archaeologists to treat hypothesis evaluation not as a process of building evidential support for hypotheses but rather as a matter of subjecting bold conjectures to the most rigorous tests they can devise; what distinguishes genuine science from pseudo-science, on Popper's account, is not the degree of empirical support or the empirical content (the cognitive significance) of its constituent claims, but the uncompromising critical attitude that scientists bring to bear in evaluating these claims (Popper 1989: 50–52). To give these general guidelines purchase on archaeological practice, Bell extracts from Popper's critical methodology what he describes as a checklist of questions archaeologists should ask about the hypotheses they mean to evaluate.[33] Although Bell's objective is to bring philosophy into closer contact with the practical concerns of field archaeologists, this engagement between fields remains largely an exercise in exporting philosophical wisdom. He gives no indication that the Popperian models he advocates will be held accountable to archaeological practice. In fact, in cleaving to quite traditional, normative scientific ideals Bell sets aside the whole range of contextualist, antifoundationalist critiques, both philosophical and archaeological, that call into question faith in the capacity of evidence to decisively refute a test hypothesis. These are a long-standing source of intractable difficulties for Popperian theories, and they capture a methodological conundrum with which archaeologists have struggled with growing intensity and

sophistication since the advent of the New Archaeology: how to interpret archaeological data as evidence so that despite its theory-ladenness, it retains a capacity to challenge our expectations about the past and even, on occasion, to subvert the framing assumptions that inform the research enterprise as a whole.

THE SOCIOPOLITICS AND ETHICS OF ARCHAEOLOGY

For all its acrimony, the polarized debate between processualists and postprocessualists has had the salutary effect of giving new prominence to questions about archaeologists' social and historical location, and about their political and ethical accountability. These questions have been taken up in two different connections.

On the one hand, a growing contingent of critical archaeologists have used historical and sociological tools to document the influence on archaeology of its colonial, nationalist, and imperialist entanglements (Trigger 1989b); its relationship to intranational and international elites and its class structure (Patterson 1986a, 1986b, 1995b); its assimilation of racist and sexist presuppositions (Trigger 1980; Gero and Conkey 1991; Moser 1996); and myriad features of its funding base, internal communication patterns, institutionalization, recruiting and training, and reward structures (Gero, Lacy, and Blakey 1983; Kelley and Hanen 1988; contributors to Pinsky and Wylie 1989; Gibbon 1989; Moser 1993, 1996, 1998; Molyneaux 1997; Shelley 1996). These studies reinforce contextualist and constructivist arguments for rethinking ideals of objectivity that make a primary virtue of neutrality and value freedom; they have both arisen from and provided the impetus for postprocessual challenges to the scientism of the New Archaeology. At the same time, however, the goal of critical archaeology is often centrally constructive (Kelley and Hanen 1988): it is to ensure that archaeologists are accountable for their presuppositions and to provide a basis for better-informed judgments about the credibility and likely limitations of archaeological knowledge (e.g., Leone, Potter, and Shackel 1987; Preucel 1991a).

On the other hand, a broad cross section of archaeologists have taken up normative, sociopolitical issues in connection with questions about

their professional and public responsibilities. Pressure to consider such issues has been mounting since the early 1970s, when it became clear that the future of archaeology was threatened worldwide by rapidly accelerating destruction of archaeological resources and an unprecedented expansion of the international antiquities market (e.g., Lipe 1974; E. Green 1984). In the same period, archaeologists have faced increasingly vocal and powerful challenges, at home and abroad, from a range of external interest groups who oppose their use of archaeological sites and materials; most prominent among them are indigenous peoples around the world, especially Native Americans, who object that scientific investigation does not serve their interests in preserving what they regard as their cultural heritage. At the same time, as a growing majority of archaeologists find employment in government agencies and industry, internal debate about professional accountability has intensified. Attention focuses on such questions as whether archaeologists are ever justified in making professional use of looted or illegally traded material; whether the goal of preserving archaeological resources should be as central as that of investigating the record for scientific purposes; what responsibilities archaeologists have to the diverse communities affected by their research, especially descendant communities; and how the goals of scientific investigation are to be weighed against heritage interests when these conflict (see, e.g., E. Green 1984; M. Salmon 1997, 1999b; and contributions to M. Salmon 1999a and to Vitelli 1996). Although the discussion of these issues has taken a course of its own, it does impinge in important ways on questions about the goals and epistemic status of archaeology; the need to ground analysis of archaeological practice in an understanding of the contexts of practice now takes on an explicitly normative, as well as sociological and historical, dimension.

What has emerged as analytic metaarchaeology is thus a motley, disunified subfield—or, more accurately, interfield—located at the intersection of archaeology, philosophy, and a growing body of internal historical and sociological research on archaeology. *Analytic* metaarchaeology was initially the product of a philosophical engagement with archaeological practice, but it has generated a body of work that is neither strictly internal to archaeology nor altogether assimilable to the preoccupations of philosophers. Its status is, in part, a function of the naturalizing turn that is substantially reshaping philosophy of science; many philosophical questions about science are now recognized to require forms of investigation that integrate conceptual analysis with empirical (historical, social scientific, psychological) studies of scientific practice. In the case of analytic philosophy of archaeology, the resolve to take this naturalizing turn has been reinforced by the collateral growth within archaeology of highly sophisticated normative, historical, and sociopolitical studies of archaeological practice. In this regard, analytic metaarchaeology exhibits all the strain and uncertainty attendant to grounding philosophy in science. At the same time it illustrates concretely just what can be gained when philosophers reopen the question of what their tools of analysis have to offer an empirical discipline like archaeology and also what they stand to learn from an understanding of real rather than idealized ("fantasy") research practice.

WHAT FOLLOWS

Coming into analytic philosophy of science and archaeology in the early 1970s, I took it for granted that both fields were undergoing a sea change. The analyses that follow are all, in one way or another, a legacy of the interfield connections forged both by the intense philosophical interest of archaeologists and by a growing commitment among philosophers of science to ground their analyses of science in the sciences themselves. In this spirit I have proceeded as a hopeful amphibian and naturalizer; I address questions that arise as much from the philosophical complexity of archaeological practice as from reflection on the archaeological fitness of philosophical models of science.

At first I was struck by incongruities that had drawn the attention of philosophical commentators in the early 1970s. The positivism endorsed by the New Archaeologists seemed fundamentally at odds with their expansive anthropological, processual ambitions. They insisted that "space-time systematics" must not define the limits of archaeological inquiry, and yet they advocated positivist models of explanation and confirmation that presuppose a view of science according to which its primary aim is to systematize observables. In-

deed, the New Archaeologists demanded no less than a Kuhnian revolution, but they invoked precisely the empiricist/positivist "building block" model of scientific inquiry that Kuhn repudiated. What I found puzzling at the time was not so much how such philosophical incongruities could arise—what misconceptions about the content or purposes of philosophy they revealed—but why philosophical models of science should have seemed relevant to archaeological practice in the first place. In Collingwoodian terms,[34] I wanted to understand what the questions were to which logical positivism seemed a compelling answer, despite philosophical contraindications.

I learned that the New Archaeology was not altogether new; it is structured by debates about the goals and strategies of inquiry that have deep historical roots. At the heart of those debates is a methodological dilemma that has resurfaced, in increasingly polarized terms, every twenty or thirty years since the early twentieth century, when North American archaeology was rapidly becoming professionalized and institutionalized: if archaeologists pursue anthropological goals it seems unavoidable that they will overreach the limits of their evidence, risking the pitfalls of armchair speculation; and if they honor a commitment to rigorously scientific modes of practice (construed in empiricist terms) it seems that they must largely restrict inquiry to the recovery and systematic description of the archaeological record. The New Archaeologists sought to circumvent this dilemma by showing how, properly conceived, the tools of science might be harnessed to anthropological goals; if archaeological data were used systematically to test speculative hypotheses, the requirements of empirical rigor might actually support, rather than mitigate against, ambitious explanatory and interpretive goals. Ironically, however, when the New Archaeologists invoked Hempelian positivism as a source of guidelines for reframing research practice, they reinscribed at the conceptual core of their program the very dilemma that they sought to escape. The conclusion I drew in a doctoral dissertation titled "Positivism and the New Archaeology," written between 1979 and 1981 (Wylie 1982c), was that a conceptual fault line ran through the New Archaeology; the inherent tensions between the substantive objectives of the program and its positivist commitments could not but generate a new internal crisis. In part II, I outline the history of the philosophical and methodological debate that prefigured the New Archaeology, elaborating the analysis I have given here of the tensions that were inherent in the program at its inception.

As processual archaeology took hold in increasingly diverse research settings, tensions also emerged between what New Archaeologists recommended and what they actually did. When they followed the directives of a strict deductivism, the results were often acknowledged to be trivial. Flannery caricatures the fruits of these labors as "Mickey Mouse laws" and invokes the wisdom of a colleague: "if this is the 'new archeology,' show me how to get back to the Renaissance" (1973: 51). But when New Archaeologists kept in view a fundamental commitment to explanatory goals, they made much more complex and interesting use of the standard resources of archaeological inquiry, both conceptual and evidential, than could be captured by Hempelian idealizations. Part III consists of essays in which I undertake to disentangle these promising and innovative aspects of the New Archaeology from its positivist commitments. In chapter 7 I make the case in general terms that in their practice if not in their programmatic statements, the New Archaeologists make good use of a number of research strategies that go some distance toward finessing their recurrent interpretive dilemma.

It is a mistake, however, to expect that these forms of practice will establish archaeological conclusions with deductive certainty. With few exceptions, archaeologists depend at every turn on broadly inductive forms of inference: interesting conclusions inevitably extend well beyond any evidence or reasons that can be provided in their support.[35] What is obscured by the New Archaeologists' uncompromising anti-inductivism, but made clear by their practice, is that this need not be a counsel for despair. There is no question that the kinds of explanatory and interpretive claims New Archaeologists hope to establish are, to varying degrees, uncertain. Nonetheless, it does not follow that all claims about the cultural past are equally and radically insecure. The challenge is to give a clear, closely specified account of how systematic distinctions can be made between relatively speculative and relatively secure claims: how the degree of support offered by ampliative inference can be assessed.

I take up these issues of epistemic credibility in more specific terms as they arise in connection with the New Archaeologists' rejection of any inference concerning the "insides of actions" (in chapter 8), their repudiation of analogical inference (chapter 9), and the arguments of critical archaeologists who question the strong objectivist claims of self-consciously scientific archaeology (chapter 10). My thesis is that a commitment to scientific, empirical rigor should not be construed so narrowly as to exclude these areas of inquiry or forms of inference. In chapter 11, I offer a general outline of the process of inferential tacking by which archaeologists put localized strategies of evidential argument to work; taken together, they exemplify the promise that there are options "beyond objectivism and relativism" (Bernstein 1983).

In part IV I take up these themes again, but in connection with the sharply polarized debate between processual and post- or antiprocessual archaeologists that took shape through the 1980s. In chapter 12 I argue that when archaeological strategies of inference are understood in more realistic terms than deductivist models allow, they can be seen to play as central a role in the critical arguments of postprocessualists as in the practice of self-consciously scientific New Archaeologists. In the three chapters that follow, I refine a model of the empirical and conceptual checks and balances that can ensure virtuous rather than vicious circularity in the theory-ladenness of evidence; my aim is to show how archaeological evidence can be an interpretive construct at every level, as postprocessual critics argue, and still (sometimes) impose significant empirical constraints on what we can plausibly claim about the cultural past. The key, I argue, lies in the role played by background and collateral knowledge in evidential argument: specifically, in considerations of the soundness of these sources, the variety of evidence they support, and various dimensions of epistemic independence that can be established within and between lines of evidence.

To illustrate how this model works, in chapter 14 (as in chapter 10) I focus on examples that illustrate how deeply archaeological inquiry is shaped by its normative, sociopolitical, and historical contexts. In particular, I consider feminist analyses that throw into sharp relief the gendered dimensions of the archaeological enterprise. I argue that at the same time as these undercut objectivist pretensions to "a view from nowhere," they reinforce the conclusion that situated interests do not necessarily determine the outcomes of inquiry. As a thoroughgoing naturalist might expect, what balance of contributing factors must be considered in explaining the course and consequences of any given program of archaeological research is an open (and empirical) question. Part IV closes with a recent essay on models of explanation in which I examine arguments for and against treating the unifying power of an explanatory account as evidence of its credibility (chapter 16).

In a concluding essay, chapter 17 (which alone constitutes part V), I explore the nexus of ethical and epistemological issues raised both by internal and by external critics who ask "who owns the past?": whose interests are served by archaeology and what accountability do practitioners have to descendant communities, to others who are affected by their work, to a broader public, and to the range of interests evoked by the conservationist slogan "save the past for the future"? It is increasingly in this arena of debate that questions about the goals of archaeology, its identity, and its standards of practice are addressed, recast as normative questions of accountability. Clarke's injunction to abandon innocence is more apposite now than ever before; there is very little an archaeologist can do that is epistemically, ethically, or sociopolitically innocent.

How New Is the New Archaeology, and Other Historical Essays

In the essays included in this section, my aim is to clarify what is at issue in the debates sparked by the programmatic claims of the New Archaeology. This first involves setting them in the context of a long history of debate within North American archaeology. A number of common themes run through these debates, centering on the question of how archaeology is to get beyond fact gathering—the antiquarianism opposed early in the century, or the "empiricism" condemned by some in the 1950s and, again, by the New Archaeologists in the 1960s and 1970s—without lapsing into arbitrary speculation. While New Archaeologists claimed to have made a decisive critical break with "traditional" archaeology, they in fact retained several limiting features of the forms of practice they rejected. The internal contradictions at the heart of their program generated a second critical break, marked by the proliferation of anti- and postprocessual archaeologies. The polarized positions that structure contemporary debate have intriguing

antecedents in several earlier rounds of critical engagement.

To frame these recurrent patterns of debate, I briefly identify three key tenets of the New Archaeology that are pivotal to my analysis. The point of departure for the New Archaeologists of the 1960s was a conviction that the failings of "traditional" modes of research could be attributed, in part, to conceptual limitations that archaeologists bring to their research, not to constraints inherent in the archaeological record. The first such failing was epistemological: on the analysis of the New Archaeologists, traditional archaeology was predicated on an empiricist theory of knowledge that, in principle, limits inquiry to the systematizing of observables. The second compromising factor was a normative conception of the cultural subject according to which culture, per se, consists of animating beliefs and norms that must be inferred from the observable behavior of human agents or, more indirectly, from the material things they produce.

In combination with empiricist commitments, this normative theory was the source of a paralyzing pessimism about ever using archaeological data as a basis for anthropological (or indeed historical) inquiry; a cultural subject conceived in these terms, as archaeologically unobservable, is patently unknowable on narrow empiricist assumptions. Finally, the New Archaeologists objected that these first two constraints arise from a third: the tendency to treat the framing presuppositions of inquiry (theoretical or epistemic) as a given or even to presume that research is, to paraphrase Clarke, innocent of presuppositions altogether. The result was a tendency to take for granted the empiricism and normative theory of culture that had become entrenched and to assume that the fragmentary and ephemeral nature of the archaeological record imposes an absolute constraint on what could be learned about the cultural past.

The central tenets of the New Archaeology constitute a rebuttal to each of these assumptions. Advocates of the program insisted that assumptions about the nature of the cultural subject and the limits of inquiry must be made explicit: archaeologists should consider their options systematically and critically. In this spirit, an explicitly positivist epistemological stance was proposed as an alternative to the self-defeating empiricism of traditional archaeology, and a materialist-functionalist conception of the cultural subject (an "ecosystem" theory) as a counter to the limitations of the normative conception. If cultural norms are just one element of an integrated system whose components are all shaped in interaction with one another and, ultimately, in adaptive response to the material environment of the system, then cultural phenomena become archaeologically tractable; in principle, the explanatorily salient features of cultural systems are accessible through analysis of the "exoskeleton" of its material culture. And if archaeological data are used to test claims about the cultural past, rather than treated as the premises of radically insecure (inductive) interpretive inference, then, it was hoped, inference that extends beyond the observable record might be set on a firm, deductive, and empirical foundation.

The details of this analysis of the conceptual core of the New Archaeology are developed in chapters 3 and 4, where I explore the legacy of contradictions internal to the epistemological component of the program. In chapters 1 and 2, I describe a recurrent cycle of debate: roughly every twenty years since the turn of the century, precursor "new archaeologists" and their critics have wrestled with the epistemological issues made famous, most recently, by the New Archaeology of the 1960s and 1970s. Although these earlier episodes of debate are rarely acknowledged, they prefigure the controversy about the New Archaeology that erupted almost as soon as its programmatic core had been articulated; the unfolding of these debates is the focus of essays included in parts III and IV. Part II closes with two previously published essays. The first (chapter 5) is an analysis of the philosophical debate generated by arguments for scientific realism, a theory of science that, I argue, offers a much more congenial framework for the New Archaeology than does Hempelian positivism. In the second (chapter 6) I return to the metaphilosophical questions raised in the introduction as these were posed by critics of the philosophical turn taken by the New Archaeology; I address the question of what philosophy can usefully contribute to an empirical research discipline like archaeology.

How New Is the New Archaeology?

CONTINUITY VERSUS DISCONTINUITY

From the inception of the New Archaeology, its newness has been a matter of lively debate.[1] Its strongest proponents have insisted that it represents a revolutionary break with the past. Certainly it is true, and uncontested, that a generation of archaeologists with a great diversity of backgrounds and interests were drawn together by common disaffection with traditional archaeology. But the more contentious and interesting claim is that this convergence of critical sympathies produced a comprehensively new departure in archaeological theory and practice. Kuhnian theories of scientific revolution were invoked to valorize the initiatives of the New Archaeology and to secure their identity as elements of an integrated and decisively new research program.[2]

Critics of the New Archaeology, and even some of its friends, judge this assertion of radical discontinuity with the past to be hubristic. Some argue that the new paradigm in fact represents no departure at all from previous forms of practice or their orienting commitments. Meltzer takes a particularly strong line, insisting that despite concerted efforts to "manufacture a Kuhnian revolution . . . to become a different kind of discipline" (1979: 649), the changes wrought by the New Archaeology were narrowly methodological, leaving the conceptual core of the discipline intact. He notes that archaeologists before and after the alleged revolution were united in conceiving the archaeological record as "a special case of anthropological phenomena"—as a body of distinctively cultural material—and share a commitment to the associated goal of "discover[ing] . . . an underlying ethnological reality" (653). In this regard they subscribe to a common paradigm derived from cultural anthropology. Meltzer therefore concludes that "there has been no revolution in archaeology"; there is "very little of the New Archaeology that cannot fit in the same linear continuum with the Old Archaeology" (Meltzer 1979: 654; see Trigger 1989b: 5–6).[3]

This strong thesis of continuity turns on the assessment that a genuine Kuhnian revolution requires "a change in the discipline's ontological structure—its metaphysic—[whereby] a new and revolutionary view is introduced" (Meltzer 1979: 649). When, through revolution at this level, a research community adopts a fundamentally different conception of what it is that it studies, practitioners are bound to rethink their aims and strategies of inquiry. For this reason revolution is often accompanied by dramatic changes in practice, though such changes do not in themselves constitute revolutionary change on Meltzer's account. The New Archaeology demonstrates such change

without revolution, Meltzer argues: methodological and technical innovations were introduced that, far from reflecting a fundamental shift in aims and ontology, simply manifest a "desire to work more convincingly and efficiently within the traditional metaphysic" (653), namely, the metaphysic defined by anthropological concepts of culture.

It is easy enough to demonstrate that archaeologists with very different polemical stances, writing at various times before and after the emergence of the New Archaeology, all conceptualize their subject in broadly cultural, anthropological terms. But this commonality obscures the degree to which the conception of culture endorsed by the New Archaeologists was oppositional, underwriting the methodological reorientation of archaeological practice that Meltzer does acknowledge. The New Archaeologists categorically rejected the normative conception of culture associated with traditional archaeology, endorsing instead a thoroughly materialist ecosystem theory. Cultural phenomena were to be understood, first and foremost, as the "extrasomatic means of adaptation for the human organism" (L. Binford 1962: 218); they were not to be identified with the animating ideas or norms that inform behavior and the production of material culture, as traditional archaeologists had done.[4]

More radical breaks might be envisioned. Meltzer may have had in mind a shift away from any conception of cultural, human phenomena that treats these as distinct, in their intentionality, from biological and ecological phenomena. Such a position has been vigorously defended by Dunnell (1989a) and is at the crux of recent debates about the viability of various evolutionary approaches to archaeology (see, e.g., contributions to Teltser 1995, and the exchanges published with Lyman and O'Brien 1998). Gumerman and Phillips have more broadly argued the case for expanding the range of fields with which archaeology is affiliated; "perhaps there is no single home for all of archaeology's activities," but by the late 1970s, they urged, the time had come for questioning "the near sacred principle in American archaeology that at present sociocultural anthropology provides the most appropriate grounding for archaeological research and for archaeological training" (1978: 189). If revolution requires nothing less than complete dissociation from its traditional disciplinary affiliations, then certainly archaeology has seen no revolution;[5] the New Archaeology does leave archaeology where it found it, aligned with anthropology. But whether or not theoretical shifts *within* a broadly anthropological paradigm should be dignified as revolutionary, they have had far-reaching implications for archaeological practice. The methodological stance adopted by New Archaeologists—their insistence that research be integrated around specified problems and designed as a test of explanatory hypotheses—reflects possibilities opened up by the particular eco-materialist conception of the cultural subject that they endorsed. The New Archaeology is not simply a cumulative elaboration of the technical dimension of a stable, monolithic paradigm; to a significant extent it is driven by changes in how the (cultural) subject is conceptualized.

Meltzer is right to counter implausible claims of revolution; there is indeed significant continuity between the New Archaeology and its antecedents. But these points of connection are selective, conditioned by a tradition of debate in which rival visions of an anthropological, scientific archaeology had already been articulated and repeatedly contested. Continuity within this tradition is by no means static or strictly linear; the question of what, exactly, persists and where divergence arises between the New Archaeology and its antecedents is much more complicated. My thesis is that the New Archaeologists were responding to a set of epistemic and methodological problems that have resurfaced as a matter of explicit debate in North American archaeology roughly every twenty years since the early twentieth century, with roots in the late nineteenth century. Their attack on traditional archaeology extends the themes central to a genre of radical critique that was already clearly articulated by the beginning of World War I; and their constructive proposals articulate, in newly philosophical terms, key features of what I identify as an integrationist (as opposed to a sequent stage)[6] approach to archaeological practice that emerged most clearly in the late 1930s and 1940s. The main locus of continuity is the problematic engaged by the New Archaeologists; the break they make is with the conservative element's past practice and the alternative they propose is innovative in many of its specifics even if it does not represent an altogether new departure.

ANTECEDENT NEW ARCHAEOLOGIES

One point of continuity is clear. The problems that drew the attention of New Archaeologists in the 1960s and 1970s were well-entrenched and widely recognized within the discipline; they were responding to long-standing discontent with traditional forms of practice. The most immediate antecedent to the New Archaeology of the 1960s and 1970s were post–World War II initiatives that Meggars saw as creating a "new look," a coming-of-age of scientific approaches to inquiry (1955: 128); Caldwell heralded them four years later as signs of a "new American archaeology" (1959: 303).[7] Meggars focused attention on a number of questions that were to become central to the New Archaeology: specifically, on what counts as scientific practice and how archaeologists should construe a commitment to anthropological goals. Caldwell's review of the state of archaeology, which appeared in *Science* three years before the first of Lewis Binford's "fighting" articles (1962), puts Meggars's assessment in a larger context; he traces the development of a promising transition, already well under way, in which North American archaeologists were moving decisively beyond both a prewar "natural-history stage of inquiry" and an immediate postwar preoccupation with systematization and culture history (Caldwell 1959: 303). These two traditions of practice were the foils against which the New Archaeologists of the 1960s and 1970s defined their own distinctive research program several years later.

As Caldwell describes the "new archaeology" that had taken shape in the 1950s, it incorporated most of the goals and constructive proposals that later became the cornerstones of the New Archaeology. The "new archaeologists" of the 1950s were resolutely anthropological in just the sense championed by the New Archaeologists: they were "more concerned with culture process and less concerned with the descriptive content of prehistoric cultures" (Caldwell 1959: 304). And though they did not conceive their subject, cultural phenomena, in explicitly systemic terms, they did understand it to be structured by underlying, generalizable processes and connections; this structure suggested that the cultural past is archaeologically accessible, and that archaeologists could reasonably set their sights on the goal of investigating cultural dynamics. Meggars traces in this change

a promising shift away from a dominant conception of culture as "essentially a psychological phenomena," largely inaccessible to archaeological investigation, which had reinforced a tendency within sociocultural anthropology to "stigmatize" archaeology and its results "as being hopelessly deficient and relegated to secondary importance" (1955: 128).

Perhaps most significant, the case for expanded ambitions that Meggars and Caldwell outline was supported by epistemological arguments to the effect that the archaeological record is a much richer evidential resource than skeptics typically recognize. In some formulations these arguments anticipate Kuhnian themes; Caldwell notes a recognition that "a given body of archaeological materials [can represent] different historical or cultural facts" depending on the interests of investigators and the nature of their interpretive resources (1959: 305). At the same time, Caldwell is quick to argue that this plasticity is not unlimited and that archaeological data can provide a very effective test of interpretive and explanatory hypotheses: in fact, "the pathways of archaeology are strewn with the wreckage of former theories which could no longer be supported in the light of new data" (306). Meggars reinforces this optimism by discussing at length the caution with which eminent natural scientists define their goals and assess the uncertainties of their results when they reflect on what they actually do (1955: 119–127). Their appraisal of the importance of treating theories as "working tools," always defeasible in light of new evidence and subject to requirements of plausibility rather than definitive proof, suggests a set of standards for practice that are much more amenable to the vagaries of archaeological practice than popular accounts might allow (118, 123). The upshot is an realignment of disciplinary ambitions that directly anticipates the "positive attitude" that became the hallmark of the New Archaeology, based on arguments that parallel the critiques of traditional archaeology—of empiricism and of normative conceptions of culture—developed by the New Archaeologists a decade later in the first two tenets of the program identified above.

Finally, Caldwell begins and ends his discussion by strongly insisting that theoretical and methodological developments are a crucial locus for progress in archaeology: "where we have im-

proved on the older archaeology is by asking different kinds of questions of the materials, and this is directly bound up with the new interests [in culture process and in problems of far greater generality] we have noted" (1959: 304).[8] He thus finds immanent in the "new American archaeology" of the 1950s a commitment to reflective, conceptual analysis that I have identified as the third key feature of the New Archaeology. If Caldwell is accurate in claiming that these developments were general trends in the discipline by the late 1950s, it is clear that what later came to be known as the New Archaeology did not emerge, ex nihilo, after 1962. Not all archaeologists practicing in the 1950s were traditional in the sense to which the New Archaeologists so strenuously objected; some were already actively debating the issues brought to prominence in the 1960s and 1970s.

EARLY DEMANDS FOR A NEW ARCHAEOLOGY

Caldwell characterizes the new postwar trends in archaeology as the culmination of critical initiatives that were taken in the late 1930s in reaction against the persistent tendency among archaeologists to treat fact gathering as an end in itself. In fact, these concerns had been articulated some twenty years earlier, just before World War I; moreover, they have recognizable antecedents in the nineteenth century, when professional archaeology was first taking shape.[9] In each of these self-reflective episodes and in those that have followed, similar questions about the aims and ambitions of the discipline have been raised and vigorously debated, generating a repertoire of responses that prefigure the most recent round of engagement between the proponents of a New Archaeology and their critics.

Writing in 1913, Dixon inveighed against research that continued to be "woefully haphazard and uncoordinated," showing "too little indication of a reasoned formulation of definite problems" and an inexcusable "neglect of saner and more truly scientific methods" (1913: 563). He insisted that "the time is past when our major interest was in the specimen. . . . We are today concerned with the relations of things, with the whens and the whys and the hows" (565). The problems he thought archaeologists should pursue were both descriptive and explanatory, culture-historical and processual, cutting across distinctions that were articulated in later debates. They include questions about the arrival and diffusion of people in America, questions about the histories of specific cultural groups in America and the "growth of American culture as a whole," and, most interesting, "still wider problems about the development of culture in general" (563). To address these questions effectively, Dixon argued, archaeologists must approach their labors from an "ethnological point of view" (565); they must recognize that their understanding of the past depends on ethnological knowledge of the present and should exploit a strategy of reconstructive inference—moving stepwise from ethnohistorically documented contexts to ever more distant antecedents in cultural forms and affiliations—that later came to be known as the "direct historic method" (e.g., F. Johnson 1961). Most important, Dixon was also a strong advocate for bringing more systematic, scientific methods to bear on archaeological problems.[10] It is particularly significant, in light of proposals made by critics in the 1930s, by Caldwell and those he identifies as engaged in the "new American archaeology" of the 1950s, and by the New Archaeologists of the 1960s and 1970s, that Dixon explicitly recommended a strategy of hypothesis testing. He urged archaeologists to design every aspect of their research so as to ensure that they recover evidence relevant to the problems they ultimately intend to address: "If there are gaps in the evidence, why not make a systematic attempt to fill them? On the basis of evidence at hand a working hypothesis or several alternative hypotheses may be framed, and material sought which shall either prove or disprove them" (Dixon 1913: 564).[11]

Dixon's call for attention to questions about archaeological aims and methods was not uncontroversial at the time. His 1913 article was published with several comments,[12] including a lengthy response by Laufer, a contemporary who vigorously defended the existing modes of practice. On Laufer's account, the responsibility for any apparent failure to contribute to ethnological understanding resides "solely in the material conditions of the field," not in any "alleged or real deficiency of methods"; archaeologists are plagued by a lack of data and by the consequent incompleteness of their empirical analyses, especially where chronological sequences are concerned (1913: 576; see Trigger 1989b: 187). Their most ur-

gent need was not to explore wholly new strategies of inquiry but to press on with the business of building a rich and orderly data base. In the end, Laufer insisted, the merits of any method, including scientific methods, could be established only in practice, by "the fruits which it yields," not in abstract theoretical terms (1913: 573). His conclusion, which is cited with some relish in later historical reviews of the period (e.g., F. Johnson 1961), was a spirited condemnation of any reflective preoccupation with questions of method:

> We should all be more enthusiastic about new facts than about methods; for the constant brooding over the applicability of methods and the questioning of their correctness may lead one to a Hamletic state of mind not wholesome in pushing on active research work. In this sense allow me to conclude with the words of Carlyle: "Produce! Produce! Were it but the pitifullest infinitesimal fraction of a produce, produce it in God's name! 'Tis the utmost thou hast in thee: out with it then!" (Laufer 1913: 577)

Despite this impassioned defense of existing practice, many did seem to share Dixon's concerns. At least two other discussions of archaeological method had appeared in the previous five years that affirmed archaeology's need to move beyond a myopic preoccupation with the data and adopt more scientific forms of practice (Hewett 1908; H. Smith 1911). And four years later, in 1917, Wissler opened an article titled "The New Archaeology" with the observation that though "there was a time when being an archaeologist meant being a mere collector of curious and expensive objects once used by man," by 1917 that time was decisively past: "such an archaeology could make no just claim to a place in anthropology, the science of man" (1917: 100). Wissler was pleased to report that the exemplars of an emerging "real, or new archaeology" had begun to explore possibilities beyond antiquarianism. It was widely recognized, he claimed, that something more than "the mere finding of things" would be required if archaeology was to make any anthropologically significant contributions to our understanding of the cultural past; the accumulation of data, on its own, is "impotent to answer the very questions we are all interested in" (100). Wissler was not specific about what procedures distinguished the "new archaeology" of 1917 from the antiquarianism it was meant to supersede, although he did emphasize the importance of research that attends to "the conditions and interassociations" of the material recovered (100), specifically its geological associations, chronological sequences, and ethnic affiliations. He gave no more explicit directives except to say that "the real equipment of an archaeologist is a scientific mind": a mind that "turns to problems" as soon as it realizes the futility of antiquarian practices and "ceases to strive for the mere collection of fine objects or curios" (101).[13]

By 1917 this early cohort of professional archaeologists was thus explicitly self-conscious about, and divided on, questions concerning the research aims and methodology of their new field. While many identified systematic, professional practice with rigorous data collection and a commitment to avoid speculation at all costs, a number of others insisted that more was needed. They argued that if archaeologists were to address anthropological problems (culture-historical or processual)—if they were to make a decisive break with antiquarianism—they must institute explicitly problem-oriented, scientific modes of inquiry designed to ensure the recovery of data relevant to questions about the whys and hows of prehistory.

THE PROBLEMATIC OF THE 1930S AND 1940S

The critical debates of the first two decades of the twentieth century were reviewed in "A Quarter Century of Growth in American Archaeology," a paper presented by Frederick Johnson (1961) at the twenty-fifth anniversary of the Society for American Archaeology in 1960. Unlike Caldwell's assessment of the previous year, Johnson's position is that by the time of the SAA's founding in 1935, the field had been professionalized in ways that had obviated Dixon's criticisms: "archaeology had been completely divorced from the business of collecting curios and the stigma of antiquarianism had practically disappeared" (1961: 2), "how" and "why" questions had become central concerns, and the direct historic method that Dixon and others had advocated was a well-established form of practice. To maintain this optimistic view, however, Johnson had to ignore a spate of intensely critical assessments of the discipline that had begun to appear in the late 1930s, shortly after the SAA was founded. Steward and Setzler

published the first of these in 1938; Kluckhohn (1939, 1940) and Bennett (1943a, 1946) followed in quick succession, and a parallel set of criticisms from Tallgren, a Finnish archaeologist, appeared in *Antiquity* in 1937 (see Patterson 1995b: 77). A theme that runs through all of this literature is the concern that despite espousing anthropological and historical objectives, for the most part archaeologists remained "but slightly reformed antiquarians" (Kluckhohn 1940: 43). Their main preoccupation was still the recovery of facts—principally facts about the contents of the archaeological record—now augmented by a desire to bring some systematic order to these facts. Their research was not informed by any clearly specified set of problems, anthropological or otherwise, and they made little effort to develop interpretive reconstructions of the cultural or historical significance of the data that were being recovered at a rapidly accelerating pace.[14] Steward and Setzler observe, with reference to the "intense interest in specimens *per se* . . . betrayed in many archaeological monographs," that "candid introspection might suggest that our motivation is more akin to that of the collector than we would like to admit" (1938: 6). Five years later, Bennett drew a similar conclusion. He found that archaeology was "still in its intense historical, fact-gathering stage" (1943a: 218) and was showing few signs of a maturing interest in anthropological questions. Indeed, at just the point when, in Bennett's view, such questions might have become a priority they were being displaced by an intense preoccupation with classification schemes.[15]

Through the same period a number of more conservative proposals for improving archaeological methodology were made by such practitioners as Strong (1935, 1936) and Wedel (1938), and by McKern (1939) and other proponents of newly synthetic typological schemes. Although these more cautious reformers rejected the most radical critiques published in the 1930s and 1940s—they were confident that if archaeologists undertook to systematize their data, fact gathering would ultimately yield "broader truths" (Wedel 1945: 386)—they too worried that by the 1930s, North American archaeologists had accumulated vast stores of archaeological data and yet had made relatively little progress in answering "why" and "how" questions about the cultural past. Clearly, contra

Johnson's assessment, the issues raised by the critics of antiquarianism around the time of World War I were by no means resolved twenty years later, when the SAA was founded in 1935. Indeed, they continued to generate debate through the late 1930s and 1940s and into the 1950s, culminating in what was seen at the time by Bennett (1943a) and by Caldwell (1959) as an extended transitional period. They were taken up again in the 1960s and 1970s by the New Archaeologists, who reaffirmed the position, articulated by earlier critics of a radical bent, that nothing short of a profound reorientation of practice was needed if anthropological aims were to be realized in archaeology.

The fundamental issues that repeatedly surface at these junctures take different forms but bear a family resemblance to one another: they all have to do with the question of how to move beyond "mere" fact gathering, how to make effective use of archaeological data as a resource for addressing historical and anthropological questions. By the late 1930s, Dixon's and Wissler's twin objectives—to address anthropological questions and to institute scientific modes of practice—were widely accepted by North American archaeologists, but tensions between these goals were apparent; a commitment to scientific rigor was not necessarily congruent with the ambition of producing an ethnographically rich understanding of the cultural past. At the time when the SAA was founded, the tradition of archaeological practice found wanting by critics was not the haphazard, opportunistic antiquarianism that Dixon had repudiated in 1913. Traditional archaeologists of the day were increasingly cautious and self-consciously systematic, distinguishing themselves from antiquarians by adhering to strict standards of methodological rigor; the scope of their interests (and results) was limited not because their primary goal was to recover objects per se, but because of their predilection to avoid speculative excess and to focus on empirically tractable questions.[16] Even the most ambitious critics of fact gathering shared the distaste of their more conservative colleagues for the debacle of nineteenth-century evolutionism: the overextended speculations of "the older evolutionists or the most uncritical of the German and English diffusionists" (Radin 1933: 156, quoted in Strong 1936: 359) and the "easy generalizations of many nine-

teenth century 'armchair ethnologists'" (Kluckhohn 1939: 328; see also Bennett 1946: 200). The challenge they faced was to demonstrate that neo-antiquarian forms of inquiry could be transcended and larger objectives pursued—archaeologists could address questions about the history, organizational form, functional integration, and dynamics of past cultures—without indulging in unacceptable forms of speculation that would compromise emerging standards of scientific practice. In this vein, Strong opens his 1936 paper with a critical response to Radin's view that archaeological reconstructions of culture history (specifically, the direct historic approach) are unavoidably insecure (Strong 1936: 361), being unsupported by acceptable historical or ethnographic evidence;[17] Steward and Setzler (1938), Kluckhohn (1940), and Bennett (1943a, 1943b, 1946) all take on directly what they characterize as a debilitating empiricist bias against any form of theorizing or hypothetical inference beyond data.[18]

Those who urged a renewal of anthropological commitments in the 1930s and 1940s responded to this ambivalence about theorizing in two quite different ways. On the one hand, the relatively conservative reformers agreed that anthropological (or historical) goals should be the ultimate objective of archaeological inquiry and that these require theoretical sophistication; at the same time, they maintained that the archaeologists' first priority must be to secure a rich, systematically ordered body of empirical (archaeological) data. Like Laufer, they held that theoretical concerns could, and should, be deferred to later stages of inquiry. On the other hand, the more outspoken champions of change were deeply skeptical about the prospects for realizing anthropological goals through a step-by-step extension of existing forms of inquiry. These radical critics, as I will refer to them, offer detailed diagnoses of why fact-gathering modes of practice must necessarily fail to produce answers to the more challenging explanatory and interpretive questions "we are all interested in" (as Wissler had put it, 1917: 100). They insist, as had the earlier advocates of a "real . . . new [nonantiquarian] archaeology" (Wissler 1917: 100), that anthropological ambitions require nothing short of a radical transformation of archaeological practice; into all its operations must be integrated an explicitly theoretical orientation to the

problems archaeologists ultimately hope to address. What crystallizes in the debate of this period is a divergence of methodological and epistemological intuitions that yields two increasingly distinct and opposed models for upgrading research practice: an integrationist model promoted by the radical critics and a sequent stage model endorsed by more conservative participants in the debate. The opposition between these programs for change produced the specific form of the perennial problematic—how to break the tyranny of a preoccupation with fact gathering and effectively pursue anthropological ends, while at the same time meeting scientific standards of rigor—to which the New Archaeologists responded in the 1960s and 1970s.

RADICAL CRITICS

The arguments against a preoccupation with fact gathering developed by the radical critics range from pragmatic, sometimes even overtly political considerations to highly theoretical and epistemological arguments. At the practical end of the spectrum, Kluckhohn asks how archaeologists can continue to justify their activities to the public if they persist in their preoccupation with "problems . . . primarily of an informational order" that are of interest only to themselves (1940: 43). This question had not escaped the attention of funding agencies, he observes; hence, the cost to archaeologists of failing to "treat their work quite firmly as part of a general attempt to understand human behavior" is obscurity, isolation, and, ultimately, the loss of public and institutional support. But when Kluckhohn considers the question of what broader interests archaeologists should serve, he does not invoke the general interests of the lay, tax-paying public; instead, he equates "the public interest" with more ambitious scholarly goals. He insists that "gathering, analyzing, and synthesizing all the data [on a given subject—e.g., Maya calendrics] is justified only if all this industry can be viewed as contributing, however indirectly, toward our understanding of human behavior or human history"; the sort of understanding at issue is explicitly identified as that sought by professional anthropologists (42, 43).[19]

More typically, the radical critics objected that a tendency to "obsessive wallowing in detail of and

for itself" is intellectually irresponsible; Kluck-hohn calls it a form of "intellectual cowardice," and even "slovenliness" (1939: 334). Worse, it represents not just the immediate loss of an opportunity to address more difficult but rewarding questions, but a short-sightedness that threatens to foreclose the possibility of pursuing historical, anthropological objectives altogether. At their most uncompromising, Kluckhohn and other radical critics of the time argued that it is dangerously naïve, epistemologically speaking, to expect explanatory insight to emerge, after the fact, through retrospective analysis of data collected for other purposes, or for no particular purpose.

The most straightforward argument for these conclusions, prominent in Dixon's (1913) and Wissler's (1917) critique of antiquarianism as well as in these later internal debates, turns on the observation that researchers can never collect all the contents of an archaeological record or describe all the attributes of the material collected; they are inevitably selective.[20] If this selection is haphazard—if it is not informed by the ultimate (historical, anthropological) objectives of the enterprise, or if researchers lack the theoretical resources to identify data relevant to these aims—it is most unlikely that the data base produced by archaeologists could support future inquiry into problems about culture history or culture process. Steward and Setzler are adamant that data collection and systematization, and the refinement of techniques for recovery and analysis, can proceed effectively "only with reference to their purpose, which involves the question of research objectives"; such questions should not be put off on the grounds that "the urgent need of the moment is to record data which are rapidly vanishing, provided it is done with proper techniques" (1938: 3). Rigorous technique alone will not ensure the recovery of relevant, usable data. So long as researchers proceed without a definite purpose in mind, Steward and Setzler insist, they will inevitably overlook data that might prove essential to these problems, and they will miss interpretive possibilities; "no one in the future will be able to interpret the data one tenth as well as the persons now immersed in them" (7). It is imperative that those actually recovering and analyzing the primary data do so with an explicit problem orientation and sound conceptual framework.

While earlier critics clearly appreciated these practical reasons for organizing research around "definite [anthropological] problems," those writing in the 1930s and 1940s took the further step of developing epistemological arguments to establish that it is not just preferable but essential to reorient all stages and aspects of practice around its ultimate goals. For example, Kluckhohn (1939, 1940) and Bennett (1943a, 1946) declare that it is a fundamental mistake, made by those who insist on deferring broadly theoretical questions until all the facts are in, to think that a body of factual information about the archaeological record can be established independently of theoretical presuppositions about its significance. Kluckhohn suggests that this caution is a practical expression of a flawed theory of knowledge: a "narrow empiricism" according to which sensorily given facts constitute the sole legitimate content and foundation of scientific knowledge, while theory, from which they are sharply distinguishable, is ruled out of scientific contexts wherever it ventures beyond the systematic description of observational fact.[21] Kluckhohn observes, in this connection, that such a "*simpliste* mechanistic-positivistic philosophy*" fails to recognize the central role played in established (natural) sciences by theoretical constructs. It is a "vulgarization of physics and chemistry" that presumes the objects of its inquiry to be strictly observable phenomena—"who has ever *seen* gravitation?"—or that laws are formulated as "straightforward *description*[s] of observed uniformities" (1940: 46; emphasis in the original). In all cases, he insists, the data systematized or cited as evidence are constituted as facts only given an interpretive theory.[22]

Kluckhohn concludes that "probably no fact has meaning except in the context of a conceptual scheme" (1940: 47). In a similar vein, Bennett flatly denies that any sense can be made of the notion that "'fact' is a phenomenological datum" and insists that archaeological facts and, most important, all systematizations of archaeological data (specifically, typological schemes) are hypothetical constructs: "what constitutes a fact or a classification is a relative affair determined entirely by the problem at hand" (1946: 198, 200). Consequently, all typologies and classifications must be regarded as "abstractions which are really bundles of testable hypotheses about the nature of correspondence of cultural objects to the dynamic culture-historical pattern which bore them" (200).

What radical critics of the 1930s and 1940s object to when they argue against the theory phobia of their colleagues is not empiricism per se but, as their term "narrow empiricism" suggests, a particularly stringent, empirically reductive, and methodologically prescriptive variant of the diverse family of theories about knowledge broadly identified as empiricist. As Kluckhohn suggests, empiricism takes as its point of departure the intuition that experience is properly the source and foundation of knowledge claims about matters of fact: it is "the conviction that the basis of knowledge is in 'experience' about the world we know" (Radnitzky 1968a: 28). This stance presupposes a distinction between synthetic statements, which make claims about the world that are true or false depending on what is actually the case, and analytic statements, which are necessarily true, whether by definition ("bachelors are unmarried men") or as a function of the axioms that define the system in which they are formulated (mathematical and logical truths).[23] Although analytic truths embody an ideal of certainty that is often associated with genuine knowledge, in practice we depend at every turn on synthetic propositions whose truth cannot be established a priori; indeed, the whole point of systematic empirical inquiry is to establish synthetic knowledge claims whose truth or credibility is empirically contingent and defeasible. A central concern of empiricists has been to develop criteria for assessing the credibility of synthetic knowledge claims and for distinguishing meaningful synthetic concepts and statements from nonsense (concept empiricism). Typically, these criteria for justification and demarcation require that for a belief or concept to be meaningful (cognitively significant) or credible, it must be connected in the right way to experience; it must be possible to show that some basis of empirical, observational, or experiential fact is the source of its content and can be used to assess its truth. This very general commitment leaves considerable room, however, for epistemological diversity.

From the seventeenth and eighteenth century on, empiricists have vigorously debated the questions of what constitutes the appropriate evidential foundation for empirical knowledge and what relationship must obtain between this evidential foundation and the claims based on it. Hume's empiricism presupposes a quasi-psychological thesis, according to which it should be possible to trace the content of all ideas of an empirical nature back to the original sense impressions from which they (or their constituents) arose and of which they are copies (1951 [1740], 1966 [1748]): "if you cannot point to any such [original] impression, you may be certain that you are mistaken when you imagine any such idea" (1951 [1740]: 65). This account of empirical content led Hume to his famously deflationary analyses of causality and of material objects, as well as to his "problem of induction." If we accept that the source and content of even the most elaborate theoretical knowledge are nothing more than patterns of constant conjunction and succession among impressions whose similarity and difference we can discern experientially, we will find, Hume argued, that we have no empirical basis for notions of causal connection, necessity, or the continuous existence of physical objects; these are ideas formed by reflection on the operations of the mind itself, moving by force of habit from one impression to the idea of others with which it is typically associated. Hume's ambition, in formulating a theory of human nature, was to set human knowledge on a firm foundation. If we systematically assess all the beliefs we hold using his strict empiricist standards of meaningfulness and credibility, we should be prepared to abandon a wide range of beliefs as "nothing but sophistry and illusion," grounded in a habit of imagination rather than in empirically given content of sense impressions (1966 [1748]: 184, 69): "In pretending, therefore, to explain the principles of human nature, we, in effect, propose a complete system of the sciences, built up on a foundation almost entirely new, and the only one upon which they can stand with any security" (1951 [1740]: xx).[24]

Hume's successors in the nineteenth century elaborated his theory of cognition, drawing on associationist psychology to account for the connections between ideas and impressions by which theoretical understanding is constructed (e.g., Mill 1893 [1843]), and they gave his prescriptive zeal a new focus. In particular, the classical positivists of the late nineteenth century undertook to elaborate methodological directives for scientific inquiry, inspired both by empiricist commitments and by analyses of research practices in the most successful of the sciences. As a form of empiri-

cism, positivism is identified with uncompromising opposition to any form of knowledge or inquiry that overreaches the domain of observables. Its first and one of its most extreme nineteenth-century exponents, Comte, argued that the evolution of human understanding had reached a critical juncture by the mid–nineteenth century (1974; see also Mill 1866); every effort should be made to foster the progressive transition from earlier, more primitive theological and metaphysical forms of human understanding to a final, culminating stage of "positive knowledge." On Comte's account, all genuine (positive) knowledge is scientific, and properly scientific inquiry is confined to the recording and systematizing of perceptually given facts about the subject phenomena. Those who seek positive knowledge must eschew altogether any "vain speculation," not only about ultimate or supernatural causes (the preoccupation of theology and metaphysics) but also about immediate and efficient causes: all are equally unobservable and therefore cannot be the subject of positive inquiry. On Mill's formulation, the primary aim of positive science must be to delineate laws that capture the "constant conjunctions" or "invariant correlations" holding among observable phenomena (Mill 1893 [1843]: 545–622).

These positivist directives for scientific inquiry raise two difficult questions that were matters of intense concern for nineteenth-century positivists. The first is how to differentiate laws from accidental regularities. Mill's answer was to maintain the prohibition against theoretical speculation about underlying causes or causal necessity, and to insist that the invariance of the patterns captured by laws is the only thing that distinguishes them; Mill's "Methods," an elaboration of procedures originally outlined in the early seventeenth century by Bacon, are inductive strategies for determining whether a particular antecedent factor is invariably associated with a given outcome (1893 [1843]: 253–266). Related to this account of the goals of inquiry is the second methodological and epistemological question: how to disembed "constant conjunctions" from the messy complexity of observational experience. Here Mill argued for an amendment of positivist ideals that originally intrigued but later was rejected by Comte. Mill was prepared to agree that strictly inductive practice enforces a random search for correlations that is unlikely to succeed in most fields. As neo-Kantians like Whewell (1967 [1847]) had argued, success in identifying lawlike regularities often depends on a highly discerning sense of where to look; it is as much a matter of the creative superimposition of order on the facts as of discovery of order among them. Mill did not concede that constant conjunctions are actively constituted in the process of research, as suggested by Whewell's account of "colligation" and "consilience" (L. Laudan 1971), but he did allow that the methods typical of many fields of empirical inquiry are partially deductive strategies, where deductive methods are otherwise the domain of analytic, mathematical inquiry; scientists posit hypothetical conjunctions that overreach all available observations and then use systematic observation of a subject domain to establish whether they hold and to what degree they are invariant. In principle, the laws that result from this "method of hypothesis" do no more than systematize patterns of association among phenomena that are subsequently observed, even though they were formulated as hypotheses projecting regularities that were not initially underwritten by observations.

By the late nineteenth century, critical arguments against the most extreme aspects of Comte's program were thus well developed, both by sympathetic and by hostile critics. In particular, these exchanges brought into clear focus the limitations of a narrowly inductive positivism/empiricism. The problem of accounting for the role of theoretical extensions beyond observation was initially a concern that Mill and Comte shared (although they subsequently parted ways on this issue). And although Mill and Whewell disagreed on many fundamentals, their detailed analyses of diverse forms of scientific practice made it clear that even the most robustly empirical inquiry is much more complex theoretically and methodologically than an idealized Baconian model would suggest. This was especially true of the fledgling social sciences, in which Comte and Mill played a founding role.[25] Nonetheless, classical positivism in its narrowest conception helped form and has had a lasting influence in many of the more naturalistic social sciences.[26]

Subsequent empiricists have largely abandoned the psychological components of Hume's analysis and of nineteenth-century positivism and empiricism; in the twentieth century they gave

the central tenets of empiricism a linguistic and logicist formulation.[27] While retaining the fundamental empiricist claim that synthetic knowledge depends on some form of empirical foundation (experiential, factual, observational), they no longer interpret it as describing how we actually acquire knowledge. For many the question of how observations, beliefs, and ideas arise is properly a subject for psychology or other forms of "material analysis"; they treat the foundationalist commitments of empiricism as claims about the formal (logical) relationship that should hold between the theoretical and the observational components of a body of empirical knowledge. A central preoccupation of logical empiricists, especially the logical positivists of the Vienna Circle in the interwar period, has been to make precise the conviction that the formal nature of this relationship is the key to assessing the credibility of empirical knowledge claims and can be used to distinguish meaningful, prospectively credible propositions from those lacking in cognitive significance: famously, the latter include any form of metaphysics, which logical positivists categorically rejected as meaningless in a quite literal sense. This approach gives rise to two problems that were the focus of twentieth-century empiricist/positivist analysis: how to specify just what constitutes the appropriate evidential foundation for empirical knowledge and how to determine what formal relations of entailment, subsumption, or inductive support must obtain between this foundation and the synthetic statements, judgments, and theoretical constructs that it supports if the latter are to be meaningful or credible.

A wide range of theses have been proposed in response to these problems. Concerning the question of empirical foundations, they include various forms of the logical positivist requirement that the factual source and ground of knowledge must consist of or derive directly from sense data, the elements of experience given in sensation (Mach 1919); variants of the physicalist requirement that this foundation consist of statements about intersubjectively observable (physical) objects or events; and more strictly linguistic formulations according to which the empirical bases of knowledge are identified with propositions or statements that are distinguished by their observational function or vocabulary. Logical positivists/empiricists have developed an equally wide

range of answers to the further question of how knowledge claims about the world, especially ambitious scientific claims (generalizing, theoretical statements), must relate to this foundation. Early logical positivism of the 1920s and 1930 is associated with verifiability theories of meaning, according to which the meaning of a (synthetic) statement is its means of verification. On the strictest formulations, verification was understood to be a matter of establishing conclusively (by entailment) the truth of a particular knowledge claim, making the content of a claim equivalent to a summary of the evidence that entails its truth; other formulations allow for partial, indirect, and inductive relations of evidential support. By extension, verificationist criteria of demarcation require that for a statement to be cognitively significant, it must be possible to determine its truth or falsity with reference to the empirical observations it purports to summarize, systematize, or explain. In this spirit, the "theory demolition" variants of late empiricism (formulated in the 1940s and 1950s) require that the content of theoretical claims must be capable of full reduction to, or translation into, their empirical base; if meaningful, they should be no more than heuristic devices that facilitate the summary or manipulation of observational data.

Partly as a consequence of the very formalism valued by logical positivists/empiricists, virtually all attempts to precisely formulate empiricist principles have been recognized as failures (see, e.g., Suppe 1977b). Strict positivist verificationism proved unsustainable almost as soon as it was proposed; it excludes, as meaningless, many types of knowledge claim that are constitutive of the best scientific knowledge (e.g., any universal generalization), as well as the verifiability criterion itself. Through the 1930s and 1940s a number of more liberal formulations were elaborated; but even strong proponents of logical positivism/empiricism, such as Ayer (1946), quickly conceded that none succeeds as a criterion of demarcation. If they are liberal enough to accommodate the rich theoretical language of contemporary physics, they will admit, as meaningful, precisely the kind of metaphysical and nonsense statements that positivists and empiricists had been intent on excluding.[28] For these reasons, among others, Popper (1959) rejected the whole project of seeking a criterion of meaningfulness as the basis

for distinguishing science from pseudo-science, proposing his falsificationist (or refutationist) account of the critical practice distinctive of science as an alternative to any form of verificationism. Ironically, this aspect of the internal breakdown of logical positivism/empiricism was recognized with particular clarity by one of its most influential late proponents, Hempel, who by the 1950s had "come to issue . . . obituary notices of the logical empiricists' way of dealing with the problem of Empirical Significance" (Radnitzky 1968a: 68). One of the most famous of these was his treatment of the "Theoretician's Dilemma" (discussed in the introduction; Hempel 1958), in which he acknowledged the paradox that on the logical empiricist principles he endorsed, the most sophisticated theories in physics seem to be either meaningless or unnecessary.

Logical positivists/empiricists also found it increasingly difficult to maintain any sharp distinction between theory and observation, and thus to sustain the foundationalism that had long been the cornerstone of empiricist theories. Internal critiques along these lines were well established by the early 1960s when Putnam argued that the "received view" of scientific theories—that they are "partially interpreted calculi"—depends on an untenable division between observational and theoretical terms (1979 [1962]: 215–220). In the early 1950s Craig had published an account of how a technique of recursive axiomatization could be used to eliminate theoretical references to unobservables, but had at the same time drawn the conclusion that this served little purpose; in the end, "it appears that empirical significance attaches to an entire framework of assertions or beliefs" and is "a matter of degree, a function of [the] empirical reliability [of these frameworks] as wholes" (1953: 52), not reducible to the empirical content or ground of constitutive concepts and propositions. Quine's more sustained arguments for holism (1951, 1960)—for recognizing that hypotheses never face the tribunal of evidence alone but always through the mediation of auxiliary hypotheses—further demonstrated how thoroughly theoretical and observational propositions are interdependent. These themes were reinforced by Norwood Russell Hanson (1958) and Kuhn (1970 [1st ed., 1962]), among other contextualists, who drew on historical, linguistic, and psychological sources to substantiate and refine the argument that observations (indeed, whatever counts as evidence) are pervasively theory-laden. Crucially, if the factual, observational, phenomenal basis of empirical knowledge cannot be assumed to be autonomous of theoretical claims, then it cannot be treated as the exclusive source of their content or as the final arbiter of their epistemic credibility; this condition undermines falsificationism as surely as it does verificationism. Symptoms of these difficulties are to be seen in the intransigent puzzles associated with empiricist theories of confirmation and explanation (see Scheffler 1963; Suppe 1977b), the debate over scientific realism (Churchland and Hooker 1985), and empiricist claims about the unity of science (Darden and Maull 1977; Dupré 1993). As Hempel describes the state of logical empiricism in the early 1960s, "The neat and clean-cut conceptions of cognitive significance and of analyticity which were held in the early days of the Vienna Circle have thus been gradually refined and liberalized to such an extent that it appears quite doubtful whether the basic tenets of positivism and empiricism can be formulated in a clear and precise way" (1963: 707).

The most general commitments of empiricism continue to animate a thriving body of philosophical analysis, but contemporary empiricists have largely abandoned the quest for principles of demarcation and criteria of meaningfulness (cognitive significance); and most eschew the prescriptive elements of positivist theories of science. As Schilpp suggests, late-twentieth-century empiricists have explored a range of "liberalizing" possibilities that have generated more realistic and plausible, but less distinctively empiricist and less robustly foundationalist, theories of knowledge. Longino argues, in this spirit, that "knowledge-empiricism" seems best defined not by principles of exclusion but by a more flexible commitment to the epistemic priority of evidence: "experiential data are the least defeasible bases of hypothesis and theory validation" (1993: 262).[29]

Writing in the 1940s, the archaeological critics who attributed a narrow empiricism to their methodologically conservative colleagues were certainly aware of the internal philosophical debates about received view philosophy of science that later resulted in its demise (Suppe 1977b); indeed, Kluckhohn was party to this debate, which he entered in 1939 when he published his first cri-

tique of empiricist influences in anthropology in the journal *Philosophy of Science*. What they objected to was not empiricism as a whole, in all its liberal and illiberal formulations, but a methodologically prescriptive and highly reductive form of empiricism: a generic positivism, derived from the nineteenth-century classical positivism of Comte and (to a lesser degree) Mill, of the type that took root and flourished in the social sciences long after it had been rejected as a viable theory of knowledge in philosophical contexts. The epistemological objections to narrow empiricism developed by archaeologists anticipate the main lines of argument associated with the contextualism that emerged a decade later as an influential philosophical antidote to late-twentieth-century logical positivism.

THE ARCHAEOLOGICAL IMPLICATIONS OF CONTEXTUALISM

The conclusion Kluckhohn draws from arguments against the implicit (positivist) empiricism of traditional archaeology is that it is not just counterproductive to avoid theorizing but, strictly speaking, impossible: "The alternative is not . . . between theory and no theory or a minimum of theory, but between adequate and inadequate theories, and, even more important, between theories, the postulates and propositions of which are conscious and hence lend themselves to systematic criticism, and theories the premises of which have not been examined even by their formulators" (1939: 330). Those who purport to collect and systematize data neutrally, free of theoretical presuppositions, simply reason enthymematically; they proceed on the basis of unrecognized and unjustified premises and in this they proceed "blindly" (Kluckhohn 1940: 48).[30] If contextualist insights are accepted, it follows that the tacit assumption of an "antinomy between 'facts' and 'theory'" (Kluckhohn 1939: 333) must be abandoned; facts are as intimately dependent on theory as theory is on facts. More to the point, there are no empirical givens, no theory-independent facts, that can be (or must be) recovered before interpretive and theoretical questions are addressed. Facts cannot be gathered in a theoretical vacuum; some set of presuppositions inevitably informs and limits research. Practitioners who deny the role of theoretical presuppositions typically depend on the "cultural compulsives" of their own unexamined (ethnocentric) commonsense assumptions (Kluckhohn 1940: 45). Consequently, their thinking can develop only within the parameters set by "traditional premises and concepts" (45). It is at least preferable, the radical critics argued, elaborating themes that were later prominent in the New Archaeology, that the presuppositions that inevitably shape and circumscribe inquiry should be explicitly chosen and held open to question; they should not be allowed to operate in the background, unrecognized and unjustified.[31]

Armed with a principled, epistemological argument against putting faith in the capacity of fact gathering to yield anthropological insight, the radical critics of the 1930s and 1940s refined and extended the constructive proposals for making archaeology a problem-oriented enterprise that had been put forward by the advocates of the first "new archaeology." They argued that proceeding by means of "passive observation" is simply not an option; empirical inquiry must be treated as an "active questioning of nature" (Bennett 1946: 200), not just because relevant evidence may be overlooked but because the archaeological record will otherwise not yield evidence at all. Bennett points to concrete ways in which data themselves are constituted as meaningful—shown "to adhere to definite structural systems" (1943a: 214)— by the interpretive frameworks that inform their recovery and analysis. Both he and Kluckhohn insist that the comparative and contextual features of the record crucial for functional analysis will be recognized only if the interpretive dimensions of inquiry are directly integrated into its fact-gathering operations. They argue, on this basis, that archaeologists must do all they can to generate more, rather than fewer, hypotheses. There must be a "multiplication of hypotheses as hypotheses" (Bennett 1946: 200), which can then be subjected to systematic testing. Echoing Chamberlin's influential endorsement of the "method of multiple hypotheses" (1890), Kluckhohn urged archaeologists to adopt what he describes as a "method of postulates" (1940: 48).[32] All of this requires that theoretical, interpretive questions be given immediate and ongoing attention; they are not separable from the operations of fact gathering and systematization if these are ultimately to support anthropological (or historical) goals. As Meggars

later put it, archaeologists cannot assume that "when the data are complete, the conclusion will be self-evident, like a ripe fruit that only needs plucking from the tree" (1955: 126).[33] Physicists make no such assumption; indeed, she argues (citing Einstein), they are clear that theory cannot be assumed to emerge "inductively from experience" and they have long been concerned to find ways of fostering the development of "disciplined imagination" even in the context of the most rigorous technical and empirical training.

For the most part, these radical critics treated contextualist arguments as grounds for methodological optimism, as did the New Archaeologists of the 1960s. The very plasticity of archaeological evidence—a function of its theory dependence—meant that the fragmentary, ephemeral nature of archaeological data is not inherently or absolutely limiting; the prospects for addressing anthropological questions about the cultural past depend, at least in part, on what conceptual resources researchers bring to their investigation of the archaeological record. Bennett insists, in this connection, that archaeology need not be confined to "the 'Baconian observation' of empirical detail" simply because it deals with tangible "sense-perceivable data" (1946: 200); Steward and Setzler argue strenuously against any assumption that the archaeological data have "intrinsic qualities" that "prohibit" its interpretive analysis as cultural material (1938: 7). To sustain this optimism, however, the critics of "narrow empiricism" had to counter the objections of skeptics who argue that their privileging of theory is simply a license for speculation. They therefore routinely acknowledge that archaeological data are *not* entirely plastic; they can provide a basis for rigorously testing interpretive hypotheses. This acknowledgment implies a qualification of their strongest contextualist arguments that is never made explicit.

It is precisely the capacity of archaeological data to disrupt interpretive theorizing that conservative reformers emphasize when they insist that any shift of priorities away from broadly fact-gathering functions is premature. More specifically, this tangible recalcitrance of archaeological data is what suggests that they have some degree of theory-independent integrity and significance. And that integrity, in turn, underwrites the continued faith of those who resist the radical critique and hold that fact gathering can proceed independently of, and will provide the necessary factual foundation for, later and more theoretically adventurous stages of inquiry. Wedel (1936, 1945) and Strong (1935, 1936, 1942) develop this conservative line of argument in greatest detail, repeatedly insisting that "archaeological research can correct as well as confirm concepts derived from historical and ethnological data" (Strong 1936: 363). When it is possible to establish independent lines of evidence that converge on a test hypothesis, they hold that such hypotheses "cannot be lightly dismissed as merely 'unjustified speculation'" (361). Strong, in particular, defends the capacity of archaeological data to provide robust confirmation, or indeed disconfirmation, of reconstructive and interpretive hypotheses when combined with ethnological, archaeological, and physical anthropological lines of inquiry (367).

Wedel's and Strong's own work in Nebraska offers a particularly compelling illustration of this strategy. They successfully challenged the assumption—deeply entrenched in archaeological, historical, and ethnographic thinking—that the presumed limitations of indigenous technology and the rigors of the Plains environment would have precluded any agricultural exploitation of the Plains before Euro-American occupation (Strong 1935: 7); the nomadic lifeways documented on the Plains in the contact period could be simply read back into the prehistory of the region.[34] Wedel and Strong established that in fact, "the late nomadic and hunting life of the central Plains appears merely as a thin overlay associated with the acquisition of the horse" (Strong 1936: 362); horticultural and semihorticultural subsistence patterns had been developed by cultural groups who lived in the region prehistorically and were subsequently displaced (Wedel 1938: 18). The crucial evidence consisted both of diagnostic plant remains recovered from the prehistoric strata of Plains Indian sites—simply recognizing that these sites were stratified was itself a significant break with tradition—and of cultural affinities identified through comparative analysis of the assemblages recovered from prehistoric sites in the central Plains with those of groups known to have practiced agriculture on the periphery of the region (Wedel 1938: 11). No doubt cases such as these were prominent in the minds of those who, as heirs to Laufer's conservatism, advocated a strategy of reform by which archaeologists could main-

tain a primary commitment to fact gathering but would turn their attention, increasingly, to the problem of making these data usable, establishing typological order among them.

DIVERGENT MODELS
FOR DEVELOPMENT

In a prescient analysis published in 1940, Kluckhohn acknowledged two possible strategies by which archaeologists might break the grip of "narrow empiricism." As practitioners in a subfield of anthropology dealing with a cultural subject, they could adopt a historical approach and construe their data as evidence of "unique events to be described and imaginatively recreated (insofar as possible) in all their particularity" (1940: 49). Alternatively, they could focus primarily on the scientific objective of contributing to a general understanding of human behavior. Kluckhohn indicates a personal preference for the second option, but he is equivocal on the question of whether these two options can be pursued conjointly or are instead mutually exclusive. Although they might conceivably stand as "two sequent phases" of a research program—an earlier, historical phase might provide the empirical basis necessary for addressing anthropological questions—Kluckhohn observes that the questions raised in the later, anthropological, stage require that archaeological data be treated as evidence of "certain trends toward uniformity in the responses of human beings toward types of stimuli (environmental, contextual, biological and the like)," and it is by no means assured that "material collected and published by the 'historically' minded" will be suitable for such "'scientific' analysis" (49).[35]

While Kluckhohn professed ambivalence about the relationship between a historical and an anthropological orientation, with few exceptions all the other advocates of change regarded these alternatives as incompatible and endorse one or the other as competing and exclusive options. For example, Bennett (1943a, 1946) and Steward and Setzler (1938) characterize anthropological ends in uncompromisingly functional, processual terms. Steward and Setzler insist that the problems archaeology has in common with ethnography, and should make its primary concern, are "problems of cultural process": questions about "the conditions underlying their origin [i.e., the origin of spe-

cific chronological and spatial associations among cultural elements], development, diffusion, acceptance, and interaction with one another" (1938: 7).[36] Bennett likewise endorses a trend toward functional interpretations that treat artifacts, at various levels of generality, "as part of a total cultural scene, integrated within social, political, and economic organizations" (1943a: 208). He particularly promotes those most sophisticated levels of functional interpretation, among five that he delineates, that take "archaeological manifestations" as a basis for investigating the "general functional relationships of the artifacts [as representative of a cultural whole] and environmental situations" (215). Because of their broadly contextualist epistemology, these critics insist that archaeologists foreclose the possibility of meeting anthropological objectives unless they ensure that at all levels and in all aspects of inquiry—from data collection and descriptive systematization through to the culture-historical reconstruction and functional interpretation of past lifeways—their explicit and primary objective is to formulate and test general theoretical models of cultural systems. In short, they endorse what I will refer to as an integrative model of research practice in which the problems appropriate to anthropology as a "generalizing" discipline are accorded both ultimate and immediate priority.

By contrast, the more conservative proponents of change, especially Strong and Wedel, reject the key features of this integrative model, despite endorsing several of its motivating considerations. They agree that it is important for researchers to move beyond the fact-gathering stages of practice, that sophisticated uses of archaeological data can support more ambitious interpretive objectives than archaeological skeptics acknowledge, and even that hypotheses are the lifeblood of the discipline. But they insist that the first priority of archaeology must (still) be to answer descriptive, empirical questions. Because "generalizations can never be more penetrating nor exact than the data on which they are based," archaeologists, qua anthropologists, must "above all . . . seek . . . objective and complete information"; so far as archaeologists are concerned, "the facts themselves are sacred" (Strong 1936: 363, 364). In fact, Strong holds that ethnography and archaeology are, at bottom, "purely descriptive"; their work becomes anthropological only when they use the results of de-

scriptive archaeological or ethnographic research "for generalizing or historical purposes" (364), in what Strong seems to envision as a subsequent, and quite independent, stage of inquiry. In the end Strong argues for precisely the deferral of interpretive and theoretical questions that Steward and Setzler deplore (1938: 3). He declares that anthropology generally, and archaeology in particular, "is still a youthful science whose primary concern is still the accumulation of essential data which in many cases are disappearing with alarming rapidity"; given this immaturity, it is the better part of wisdom to leave the interpretation of these data to "a future time of greater leisure and fullness of data" (Strong 1936: 365). Wedel similarly argues that although archaeology "obviously cannot hope to progress far without venturing generalizations and attempting reconstructions based on its accumulated observational data," the business of "accumulat[ing] observational data" must be given first priority (1945: 385). He makes clear his disagreement with the radical critics on this point when he observes that he cannot, "in any sense," accept Bennett's assessment that archaeology is "nearing 'the close of the fact gathering period'"; much remains to be done to improve the "reliability and completeness" of the existing data base, and such improvement alone will bring "broader truths" within reach (386).

This predilection in favor of a continued focus on fact gathering is sometimes reinforced by arguments that contest the conception of anthropology endorsed by the radical critics. Strong, in particular, rejects an emerging model of anthropology that gives first priority to the quest for "universal cultural laws"; he condemns this "British" approach on the grounds that it is "not only sociological, functional and generalizing, but also messianic, imperialistic, and nonanthropological" (1936: 366). The alternative, which he associates with a distinctively North American tradition, requires anthropologists to "define . . . their science as an historical discipline" (1936: 364) and to retain an emphasis on the primary value of empirical, descriptive inquiry. He is prepared not just to defer generalizing questions to a distant, data-rich future but to reject such questions altogether and redefine anthropological goals so that they are not sacrificed in the process.

On the countermodel of archaeological practice that emerges in reaction against the demands of an integrationist approach, inquiry is expected to proceed through a series of sequent stages, to use Kluckhohn's phrase (1940: 49). Fact gathering and descriptive systematization must be accomplished first; only then can archaeologists hope to undertake the historical reconstruction of particular cultural contexts and events that is, in turn, the prerequisite for any investigation of uniform processes operating across these contexts. The message of conservative reformers is, in effect, that archaeologists should not attempt to run before they have learned to walk; each stage of inquiry depends on the last as a foundation for its own activities. When the sequent stage model is aligned with a privileging of historical interests, either as the ultimate objective of anthropological inquiry or as the most accessible of several higher level objectives, it embodies the second of the two options Kluckhohn considered in 1940.

What distinguishes the sequent stage and integrationist approaches is not just a different weighting of final priorities. Despite Kluckhohn's willingness to consider anthropological and historical goals as compatible alternatives, his contextualist arguments suggest that so long as the operations of recovering and systematizing data are theoretically uninformed and lack any clear problem-orientation, archaeology will remain at a fact-gathering level of development; the resulting data base will be capable of providing only the most limited and haphazard support for historical or anthropological inquiry. By contrast, when Strong and Wedel argue that an orderly data base must be secured as a first priority, they assume that there *are* facts that are inherently meaningful and exhibit a determinate order (formal, spatial, chronological), which can and should be established independent of any theorizing about their historical or anthropological significance. In taking this position these conservative reformers embrace precisely the positivist/empiricist presuppositions about the status of archaeological facts that Kluckhohn, among other radical critics, reject as epistemically untenable. The irony here is that their own highly effective testing practices testify to the importance of making explicit the interpretive assumptions that had informed previous research and of designing research strategies that will ensure the recovery and analysis of data capable of putting these assumptions to the test. To challenge the framing assumption that prehistoric Plains-dwelling groups

could not have developed agricultural modes of exploiting this environment, they had to deliberately seek out deeply stratified sites where it was generally presumed none had existed, and they had to develop comparative analyses that had not previously been considered with horticulturalists who had been displaced from the Plains post-contact.

By 1946, when Bennett again assessed the transition he had reviewed with hopeful enthusiasm in 1943, it had become clear to him that the discipline's energies were being channeled in the directions defended by Wedel and Strong. In particular, attention had shifted decisively to problems of chronological and typological systematization with no parallel emphasis on the development of interpretive theory. This development was endorsed by the proponents of incremental change like Wedel, who, despite rejecting proposals for an extensive reorientation of practice, insisted that he "do[es] agree with those who feel the time for general synthesis is approaching in the eastern United States and the Midwest" (1945: 385). In that statement, he articulates a clear commitment to the principles of a sequent stage model; the shift of disciplinary focus from data collection to data systematization is the step archaeologists must take, beyond the recovery of data, if they are to secure the empirical foundation necessary to support historical and anthropological inquiry.

In the eyes of more radical critics, however, any attempt to systematize that is not informed by a clearly defined set of research problems and an interpretive framework is just an extension of neo-antiquarian fact-gathering forms of practice. As Bennett put it in his original critique, "a recent unfortunate trend has been the acceptance of taxonomic divisions as a goal in themselves, rather than as a tool for historical syntheses" (1943a: 208). The pitfalls of "blind" systematization were also strenuously opposed by Steward (1942, 1944) in a series of attacks on McKern's proposals for systematizing archaeological data: "Facts are totally without significance and may even be said not to exist without reference to theory. It is wholly impossible to collect bare facts. . . . [I]t is equally impossible merely to give significant order to facts without reference to some theory or problem" (1944: 99). The radical critics thus regarded the new emphasis on creating an *orderly* data base as part of the problem, not as a solution to the difficulties associated with the persistent antiquarian tendencies that concerned both radical and conservative critics. They considered it just a new, more sophisticated empiricism. Bennett's 1946 review was, in fact, a declaration that in his view, the transition away from fact gathering had stalled. The very attitudes that Kluckhohn had criticized most of a decade earlier were reasserting themselves in postwar initiatives that gave priority to data analysis and synthesis.

This conflict over the value and role of systematization in research brought the relatively abstract, philosophical disputes of the 1930s and early 1940s down to earth, setting the terms of reference for an extended debate about typological systems that dominated internal, methodological discussion through the next fifteen years. Although ostensibly concerned with practical questions about how classification schemes of various kinds should be constructed, the more fundamental questions raised by the radical critics reassert themselves again and again: the typology debates turn on questions about the nature and status of archaeological facts, and whether they can be assumed to embody inherent order. I will argue, in the next chapter, that the positions taken on these questions represent a continuous vacillation between increasingly extreme versions of the two options for disciplinary development—sequent stage and comprehensive integration—that crystallized in the late 1930s and early 1940s. It was an impasse created by this polarization of positions to which the New Archaeology responded in the early 1960s.

2

The Typology Debate

Conservative forms of traditional archaeology have coexisted with, and been shaped by, more or less radical demands for a new—anthropological and scientific—archaeology for as long as archaeology has been institutionalized as a discipline in North America. Although each of these orientations has taken a dominant role in different periods or contexts, neither has succeeded in displacing the other; this pattern continues into the present. There have always been strong voices on the side of methodological conservatism, dating at least to Laufer's ardent conviction that ethnographic insights would eventually emerge if only archaeologists pressed on with collecting basic data. At the same time, however, more radical critics such as Dixon and Wissler have long challenged this conservatism, arguing that fact gathering will do little to improve our (anthropological) understanding of the past unless it is harnessed to clearly defined problems.

By the late 1930s the concerns expressed by several generations of radical critics seemed to be borne out; by all accounts archaeologists had accumulated a vast and exponentially increasing volume of data but could not claim a commensurate gain in interpretive understanding. The radical critics of the 1930s and 1940s renewed Dixon and Wissler's objections to fact gathering, now articulated in terms of a principled argument against

the very idea of theory- or problem-neutral data collection. Although the conservatives of the day shared the discontent that motivated these critiques, they offered an alternative diagnosis. The problem was not the nature of the data base or the manner of its collection per se, but its increasing unwieldiness; as McKern (1939: 303) and others describe the situation, the complexity of the material they were recovering had long since outstripped the categories and terminological conventions typically used to describe and analyze it. Some even more conservative parties to the debate argued that archaeologists still "lack[ed] adequate information to warrant wholesale classification" (McKern 1939: 304), but most enthusiastically embraced the various "experiments" in standardized classification that were then being proposed. For conservative reformers, the development of comprehensive typological systems was self-evidently the first, most pressing order of business—a crucial preliminary to any more ambitious investigation of anthropological or historical questions. The radical critics regarded this response to the situation as an extension of the old preoccupation with fact gathering; it reinstated a scientistic antiquarianism in which *systematizing* archaeological material replaced data recovery as an end in itself. Beginning in the 1930s and 1940s and continuing through the 1950s, questions about the efficacy

and status of typologies—specifically, questions about whether they capture fundamental and inherent empirical structure or are instead heuristic, problem-specific constructs—became the primary locus of debate about the goals and epistemological underpinnings of archaeology. In the process, the differences between a relatively conservative sequent stage approach and the integrationist requirements of the radical critics were increasingly sharply drawn, setting the terms of the debate in which the New Archaeologists engaged in the 1960s and 1970s.

SYSTEMATIZATION

At their most ambitious, the proponents of what were later called space-time systematics hoped to establish a system of problem- and theory-neutral typological categories that could be used to describe the formal variability of archaeological data at various nested levels of generality, across regions and periods.[1] They were quick to point out, countering the objections of radical critics, that nothing in the nature of these typological schemes necessitates their being treated as ends in themselves; such criticisms "should be directed against the culprits who are misusing methods rather than against any given method itself" (McKern 1942: 170). At some junctures McKern suggested that formal classification, like the "Midwestern Taxonomic Method" (1939) he advocated, should be viewed as one "tool" among many that would be required to realize the anthropological and historical objectives of archaeology: "no single tool will perform all purposes equally well; we need every method which can be demonstrated as useful in advancing research toward its fundamental objectives" (1942: 170).

But McKern's use of "tool" here is somewhat equivocal. One of his most trenchant critics, Steward (1942, 1944), championed historical concerns and insisted that they could be addressed only using the explicitly interpretive, ethnographic categories developed through application of the direct historic method; archaeological material should be classified, as far as possible, by inferred cultural affiliation. In one reply McKern was prepared to concede that this approach might be appropriate for analyzing archaeological material that can be directly linked to historically or ethnographically identified cultural groups, but he argued that ar-chaeologists also need classificatory systems capable of bringing order to the vast range of archaeological data lacking such discernible cultural connections. Here he seems to envision a division of labor between typologies that serve different purposes or have different ranges of application; formal taxonomies like the Midwestern system could coexist on an equal footing with classifications based on the direct historic approach—or with "any other methods which may prove useful" to the larger "anthropological purpose [of] reconstruct[ing] an historical and cultural picture which may be integrated with and augment the time-limited concepts of the ethnologist" (1942: 172). Indeed, McKern sometimes argues that in the end, "it is convenience and orderliness in handling archaeological data that is required of the classification, not a flawless, natural regimentation of the facts required by the classification" (1939: 312). In these passages he foregrounds the heuristic, pragmatic dimensions of typological systems (formal or ethnohistoric), suggesting that they are all, to some degree, arbitrary and purpose-built constructs.[2]

More often, however, McKern insists that rather than being problem- or context-specific, formal taxonomic systems (like the Midwestern taxonomy) are fundamental to the archaeological enterprise as a whole; they constitute the "only taxonomic basis for dealing with all cultural manifestations, regardless of occasional direct historical tie-ups" (1939: 302). By focusing exclusively on formal, material dimensions of variability in the record to the exclusion of temporal and spatial factors as well as inferred historical or ethnographic attributes, McKern argues that these taxonomic systems have the virtue of being "based upon criteria available to the archaeologist" that at the same time capture cultural variability: "the cultural factor alone" (303).[3] He takes for granted that archaeological material reveals patterns of formal variability in which the associations of traits coalesce in discontinuous clusters, thereby supporting the definition of discrete taxonomic units, and he then assumes that these patterns of discontinuity and association are indicative of "cultural divisions" (communities, traditions, "types of cultures") and "fundamental cultural trends" (307; see also Cole and Deuel 1937). In short, McKern understands the Midwestern Taxonomic Method to be a strategy for "discovering order in

the world" (1939: 304), specifically the *cultural* order that he believes is inherent in archaeological data. In this case the development of such typological systems must take priority over the direct historic method; only when the formal definition of prehistoric cultural traditions is established can Steward investigate the associations linking them to ethnohistoric cultures.[4]

It would seem, then, that McKern regards taxonomic systems as arbitrary only in the sense that they are provisional, approximating an order inherent in the world. He does not regard them as one construct among many, each adequate to different purposes or capturing culturally significant variability on just one of several possible dimensions. When McKern makes the case that formal taxonomies are fundamental to the archaeological enterprise, he affirms the central commitments of a sequent stage approach: a positivist/empiricist faith in the foundational nature of archaeological facts, including facts about the structure of the archaeological record, and a methodological conviction that archaeologists must recover and systematize these facts before addressing any more ambitious interpretive questions, anthropological or historical. These are the features of McKern's method that Steward, among other radical critics, called into question in the 1930s and 1940s.

Steward objected that just as it is "wholly impossible to collect bare facts," it is "equally impossible merely to give significant order to facts without reference to some theory or problem." He continues: "a classificatory procedure, such as the taxonomic method, seriation, or sequential ordering, has meaning only with reference to problems and theories" (1944: 99). If archaeological data are to support historical inquiry, Steward's primary concern, then the use of deliberately "timeless and spaceless," nonhistoric categories of analysis (1942: 339) is at best irrelevant: "it is not obvious that the mere orderly arrangement of data in categories of similarity is a necessary or even useful step toward history" (1944: 99). At worst, such an approach may actually obscure historically significant patterns of change or development evident in the record. Steward thus invokes the contextualist arguments developed in more detail by Kluckhohn and other radical critics to establish the implausibility of the claim (or conviction) that any one comprehensive classification scheme could be expected to capture all the dimensions of vari-

ability that might prove relevant to the range of "how" and "why" questions archaeologists ultimately hope to address. Depending on what features of the cultural past they mean to investigate, quite different bodies of data and quite different selections of classificatory traits will be relevant, yielding a diversity of problem- and theory-specific typological systems.

It is surprising that Steward does not add, in the spirit of Kluckhohn's critique of enthymematic reasoning, that a great number of unsubstantiated theoretical assumptions underwrite typological schemes like those generated by the Midwestern Taxonomic Method. McKern asserts, for example, that even in the case of prehistoric cultures that have no discernible link with identifiable historic or protohistoric groups, "there are archaeologically collected data that warrant cultural segregation"; culture types may be "illustrated by trait-indicative materials and features encountered at former habitation sites" (1939: 302). But the process of constructing and illustrating these culture types depends on selecting "those trait details which have sufficient cultural significance to qualify them as cultural determinants" (302). Despite discussing at length the utility of various kinds of traits in marking formal classificatory divisions — simple as opposed to complex traits, traits relating to "shape, material, and technique of fabrication," single-medium traits — McKern offers no account of how he makes the judgment that a given trait is "culture indicative," the "determinant" of a distinct cultural entity; nor does he explain why he believes archaeological data can be expected to "objectify" a single dimension of cultural variability (see, e.g., Ehrich 1950, which criticizes McKern and others on this point, and see Radin 1933: 134, 140–144, for a parallel critique of classification schemes in cultural anthropology). The traits distinctive of cross-cultural patterns at a fourth level of taxonomic synthesis are presumed to be "the cultural reflection of the primary adjustments of peoples to environment, as modified by tradition" (McKern 1939: 309), while lower-level classificatory divisions evidently mark differences in tradition, reflecting culture-specific variability that arises within the parameters set by environmental constraints.

Far from relying on strictly formal features of the archaeological record, McKern depends throughout on a rich body of assumptions that are

never set out or defended, both about the critical role played by normative (traditional) factors in shaping cultural life (as opposed to functional considerations or environmental conditions) and about the relationship of material culture to other aspects of cultural systems, which he conceives as a matter of direct reflection. Whatever the plausibility of these assumptions, a formal taxonomy based on them is by no means a theory-neutral or problem-independent construct. In particular, it is not self-evident that McKern's "culture indicative" traits capture dimensions of variability relevant to the direct historical or functional approaches advocated by the radical critics. The danger that taxonomic exercises will become an end in themselves arises not from a failure of ambition but as the unintended consequence of a failure to consider questions and interpretive possibilities that lie outside the purview of McKern's unacknowledged assumptions. Formal taxonomies designed for one purpose, however comprehensive it seems, are unlikely to support other interpretive ends.[5]

Despite their prominence, the radical critics who argued the case for a more self-consciously theoretical, integrationist approach felt they had had little immediate impact; they expressed considerable frustration with what they saw as the continued dominance of the sequent stage approach in the postwar period. Though there were promising developments, as later described by Meggars (1955) and Caldwell (1959), many archaeologists set aside the larger issues raised by the radical critics of an earlier generation and focused on the practical, methodological problems of typology construction. Nevertheless, these issues did resurface in the context of a spirited debate about typological practice that was prefigured by the questions McKern had left unaddressed.

THE TYPOLOGY DEBATES

Two key questions structure the long-running debate about typology that began in the mid-1940s when Brew first raised a series of pointed questions in an article titled "The Use and Abuse of Taxonomy" (1971 [1946]) and Krieger offered a nuanced critique of the very idea of a purely formal typology (1944). They took canonical form in the sharp dispute between James A. Ford and Spaulding in the 1950s, and later reemerged in the early 1970s as a focal concern for the New Archaeology. These questions are whether (or, in what sense) archaeological types can be said to exist and what cultural significance they can be presumed to have. The proponents of objective, formal typological systems reaffirm McKern's view that discontinuous variability embodying antecedent cultural norms exists in the record, there to be discovered and used as the basis for systematization; in this spirit Spaulding argues the merits of using statistical techniques "for the discovery of artifact types" (1953b). By contrast, self-avowed constructivists[6] elaborate the central lines of argument developed by the radical critics, referring specifically to the exigencies of classification. Brew (1971 [1946]) and Ford (1952, 1954a, 1954b, 1954c) insist that archaeologists necessarily impose structure on archaeological material when they develop classificatory schemes; there is no unique, fundamental structure inherent in the record that a typology (or taxonomy) could be expected simply to describe in theory- or problem-neutral terms. Therefore, all archaeological classifications are constructs that serve specific analytic purposes, whether these are acknowledged or not. At the same time, a number of mediating alternatives were proposed by Krieger (1944, 1960 [1956]), by Taylor (1967 [1948]), and by Phillips and Willey (1953) and Willey and Phillips (1955, 1958).[7] The New Archaeologists take up the threads of this ongoing debate and propose a resolution that exploits elements of these intermediate positions. The sense in which their synthesis was innovative, though not discontinuous with the past, becomes most clear when their proposals are considered against the backdrop of these typology debates.

CONSTRUCTIVISM

The central tenet of the constructivist position, as developed in the late 1940s by Brew and defended in the 1950s by James Ford, is that "classificatory systems are merely tools, tools of analysis, manufactured and employed by students" (Brew 1971 [1946]: 77). The tool metaphor has quite different significance for Brew and Ford than for McKern. Brew and Ford consistently maintain that classifications are not "real," qua "inherent in the material"; they are instead theoretical constructs "inherent in our thought," constituting "the terms in

which we think" (Brew 1971 [1946]: 74). Classifications reflect an arbitrary selection of criteria and procedures that inevitably depends on judgments of relevance and significance specific to the research objectives they are intended to serve. In making this argument, Brew invokes Kluckhohn's rejection of "narrow empiricist" assumptions about the stability and foundational nature of facts. McKern-type systems simply reify the taxonomic constructs they introduce, representing them as approximations to an "ideal-complete-classification" (Brew 1971 [1946]: 86); in the process, they obscure the interests and interpretive assumptions underwriting the conviction that a uniquely significant (normative) dimension of cultural order is inherent in the record. If it is acknowledged that typologies are unavoidably problem- and theory-specific, it then follows that the quest for a single, foundational, all-purpose system of classification is fundamentally misguided. Brew therefore concludes that "we need more rather than fewer classifications, different classifications, always new classifications to meet new needs" (105).[8]

These contextualist principles were invoked by Ford when defending his proposal of a regional chronological scheme for the U.S. Southeast. The reconstruction of culture history was his main concern, and to that end he argued the case for creating purpose- (and region-) specific classification systems based on chronologically sensitive types; in "Measurements of Some Prehistoric Design Developments in the Southeastern United States" (J. Ford 1952; see also 1938), he shows how local sequences based on such types might be integrated into a hypothetical regional time frame. He delineates eight decorative traditions that appeared in various local sequences and then, on the principle that the appearance of these design traditions in different areas must represent a diffusion of ideas over space, he aligns the sequences across the region. Graphs of waxing and waning stylistic traditions over time in specific geographic locales are juxtaposed on a common template so that the frequency patterns match up, adjusted to allow time for their transmission across space; principles that had long informed temporal seriation were transposed to the spatial dimension. Ford understood variability in the occurrence of design elements to be a measure of variability in the popularity (over time and space) of cultural traits "controlled by the attitudes and ideas that were held by the makers of the vessels" (1952: 317). He further assumes what amounts to a variant of the superorganic conception of culture that so dominated archaeological thinking in this period: "culture derives from preceding culture and is not exuded by the human animal that carries it" (319).[9] This view of culture justifies an analysis of cultural variability and culture change that does not focus primarily on the beliefs, intentions, or actions of individual members or bearers of culture but rather treats cultural manifestations (of ideas, intentions, actions)—specifically material culture—as semiautonomous and self-generating, a locus of temporal process and lawlike regularities in its own right. McKern presupposes much the same normative theory of culture; the difference is that Ford is explicit about the dependence of his typological scheme on the central tenets of this theory and on a particular (culture-historical) problem orientation conceived in light of it.

Ford was pressed to justify his approach by Spaulding, who vehemently rejected constructivism in any form (in a critique described below). In response, Ford argued that archaeologists have no option *but* to engage in the "risky business of stacking hypotheses into what may be a shaky structure": "all archaeologists must regularly make these excursions into the realms of abstraction, however uneasy it may make them or however unconscious they may be that they are doing so" (J. Ford 1954c: 109, 110). While this much was already a well-worked line of contextualist argument, familiar from the radical critics of the 1930s and 1940s, Ford added an ontological thesis to the effect that "there are no inevitable, necessary breaks which will force the classifier to cut [a given ceramic distribution] into segments" (1954a: 391). As a matter of contingent fact, the variability evident in the archaeological record is sufficiently complex and enigmatic that it cannot be assumed to determine unique taxonomic categories. In Ford's view it is "amazingly naive" to think that statistical analysis will reveal "natural units" (of cultural significance) in archaeological material: he says he is "somewhat more uncertain than Spaulding that nature has provided us with a world filled with packaged facts and truths that

may be discovered and digested like Easter eggs hidden on a lawn" (1954c: 109).

Ford elaborates this crucial point by offering an ethnographic parable, the tale of an anthropologist studying material culture on the "Island of Gamma-Gamma" (1954b; see also Ehrich 1950). In this hypothetical case, even an anthropologist who has access to the living cultural context must necessarily resort to "abstractions . . . from cultural activity" when developing categories for description and analysis. These are artificial in two senses: first, they represent a choice from among a number of "different levels of apparent complexity," no one of which is more real than another (J. Ford 1954b: 47); and second, they require that boundaries be imposed on what is otherwise a continuum of cultural variability in space and time.[10] Consequently, to establish discrete descriptive or classificatory units researchers must always consciously choose diagnostic attributes and break points among many that might be used to segregate archaeological material. Ford thus concludes that anthropologists, like archaeologists, cannot be said to discover the typological categories they employ; they inevitably engage in the *construction* of categories that are relevant to their purposes and afford access to those aspects of the cultural past they hope to investigate.

The unpalatable consequence of such a position, pushed to its limit, is a debilitating circularity in archaeological analysis. Ford's critics (especially Spaulding) deplore the implication that researchers' theoretical commitments and problem orientation—or indeed their subjective intuitions and collective preferences—may determine in advance what empirical analysis can or will reveal. Ford himself acknowledged this danger in connection with his original study of southeastern ceramic traditions. He notes that his main conclusion—that there had been a "measurable evolution in the ceramics of the region" (1952: 319)—follows from framing assumptions he had had to make to construct the regional scheme in the first place. Nevertheless, Ford defends his empirical results on the grounds that they provide a valuable illustration of these presuppositions; although he could not have identified the historical patterns of change in ceramic traditions he describes in "Prehistoric Design Developments"

without assuming that cultures consist of supra-individual norms manifest in continuously evolving and diffusing cultural traits, such assumptions alone cannot ensure that empirical analysis will reveal the regular patterns of distribution he found in southeastern ceramics. Elsewhere Ford reiterates his conviction that archaeologists cannot avoid dependence on theoretical presuppositions, adding that these can only ever be evaluated and refined through their application, on pragmatic criteria: "All concepts that [humans] form . . . from sensory experience are theories to be evaluated for their usefulness in describing experience and predicting more experience. These concepts must also be evaluated in terms of the frame of reference in which they are created" (1954c: 109).

Brew takes an even stronger position, not just defending the claim that typological categories are constructs in the contextualist sense—they embody specific theoretical assumptions and are designed to serve particular investigative ends (a conceptual context)—but also arguing that they are strictly conventional. Where empirical considerations alone cannot determine how archaeological material should be categorized, Brew concludes that the typological systems are unavoidably subjective constructs; the "personal factor" must play a role in the formulation of typologies no matter how scientific the methods of empirical analysis by which they are refined or applied (1971 [1946]: 107). He therefore urges archaeologists to present their results in terms that will be useful to the public at large (107); they should explicitly use "the 'narrative approach' and subjective picturization," and should "humanize" their accounts of the cultural past to make them widely accessible.

Such conclusions were rejected outright by critics like Spaulding. In a statement that later circulated widely among the New Archaeologists, Spaulding objects that "Ford's propositions carry the logical implications that truth is to be determined by some sort of polling of archaeologists, that productivity is doing what other archaeologists do, and that the only purpose of archaeology is to make archaeologists happy. This is simply a specialized version of the 'life is just a game' constellation of ideas, a philosophical position which cannot be tolerated in a scientific context" (1953a:

590). But however objectionable these implications might be, Ford was by no means alone in advocating the constructivist view of typology from which they were said to arise. The central tenets of Ford's constructivism and Brew's conventionalism were developed in much greater detail several years later by Thompson. He was influenced by the pragmatist epistemology of John Dewey (see Thompson 1958: 1–2) and explicitly endorsed a subjectivist conception of research.

Appearing just before the advent of the New Archaeology, Thompson's analysis is significant in extending earlier contextualist insights about the essential role of theoretical presuppositions; he argues that subjective judgment can be eliminated not just from the formulation of hypotheses but from their evaluation as well. In making this case, Thompson draws on the results of an ethnoarchaeological study of the "process, limitations, and potentialities of inference in archaeological research" (1958: 30), distinguishing between the "indicative" and "probative" aspects of archaeological inference. While "indications" are features of the evidence that suggest its "inferential possibilities" (3), the initial stages of formulating hypotheses—essentially a process of discovery, on Thompson's account—are manifestly subjective; archaeologists depend on intuitions about the significance (the function or meaning) of specific aspects of the record, intuitions typically inspired by a perception of their similarity to material encountered in ethnohistoric contexts. But Thompson makes it clear that these initial hunches should never be simply accepted; they must be subjected to a process of evaluation that depends on "the introduction of probative material" (4). Because the indicative hypotheses Thompson has in mind are typically analogical, the probative process he envisions is primarily one of testing the underlying assumptions of association between the material traits found to be similar in archaeological and ethnohistoric contexts and the functional, behavioral, or ideational traits whose similarity is inferred. He recommends that archaeologists proceed by "demonstrating that an artifact-behavior correlation similar to the suggested one is a common occurrence in ethnographic reality" (6).

Although Thompson is confident that this probative process can measure the plausibility of a test hypothesis, he does not regard it as objective in any sense that would satisfy critics such as Spaulding. The correlation between behavior and artifact that is "indicated" by initial intuitions can become an object for probative evaluation only if the factors compared—the elements of material culture found in archaeological and ethnohistoric contexts, and the behavioral, functional, or ideational factors associated with them in ethnohistoric contexts—can be represented using a common store of classificatory concepts. What the archaeologist compares, in assessing the plausibility of an initial analogical hypothesis, are *types* of ethnographic behavior that are associated with *types* of material culture found in both archaeological and ethnohistoric contexts. Probative evaluations are "based on a comparison of abstractions rather than on a resemblance of individual artifacts," thereby "reduc[ing] all of the ingredients of the comparison to the same level of organization" (Thompson 1958: 6, 7). Thompson argues that these typological abstractions are constructs that incorporate an irreducible subjective element. They are designed to "produce groupings of potential cultural significance" (1958: 6; 1956); "cultural significance" is determined by the same intuitions that give rise to the indicative hypothesis they are meant to test. Here Thompson embraces, as unavoidable, the circularity Ford attempted to mitigate by appeal to evidential constraints; archaeologists can be expected to arrive at different "system[s] for observing cultural data" (Thompson 1958: 8), depending on the indicative hypothesis (the analogical intuitions) they entertain; as a consequence, they will draw different evaluative conclusions about the "probity" (the plausibility) of these hypotheses. Hypothesis testing is thus as subjective as hypothesis generation.[11]

Given this analysis of archaeological inference, Thompson explicitly endorses the conclusions that Spaulding had attributed to Ford and had rejected, out of hand, as a reductio ad absurdum:

> The final judgement of an archaeologist's cultural reconstructions [including any typological systems proposed in this connection] must therefore be based on an appraisal of his professional competence, and particularly the quality of the subjective contribution to that competence. Our present method of assessing the role of this subjective element by an appraisal of the intellectual honesty of the archaeologist who makes the inference is certainly inadequate. But, there does not seem to be any practical means of greatly im-

proving the situation despite the insistence of many of the critics of archaeological method. We can only hope for improvements in the methods of measuring the amount of faith we place in an individual's work. (Thompson 1958: 8)

What distinguishes Thompson's position from that of other constructivists who were his contemporaries is the explicitly conventionalist element that he adds. While on Ford's account judgments about the usefulness of a typology are determined both by their (contingent) empirical success and by their relevance to a particular research program, Thompson takes the much stronger line that the adequacy of typological constructs ultimately can be assessed only by appeal to the professional credibility of those who propose them; the question of "probity" reduces to a matter of whose intuitions are trustworthy, which, in turn, reduces to community conventions of plausibility and credibility.

THE DISCOVERY OF TYPES

Until the New Archaeologists took up the cause, the most outspoken opponent of conventionalist and subjectivist tendencies was Spaulding. He insisted that archaeologists have available to them "a method of scientific investigation which will disclose real truths (or approximations to real truth) about a real world if it is properly applied"; conventionalist conclusions are drawn only when the scope and potential of scientific method has been "misapprehended" (1953a: 589). Spaulding rejects both the epistemological and the ontological theses that lead to conventionalist conclusions, by implication in the case of Ford and explicitly in the case of Brew and Thompson. He insists that "the concept of a real world, i.e., one having an existence independent of the observer, is a fundamental assumption of the scientific method; questions of the ultimate nature of reality fall strictly within the province of philosophy and are obfuscations when introduced in a scientific context" (1954a: 112). In short, conventionist tendencies (here attributed to Ford) reflect a failure to accept the conceptual ground rules of the scientific enterprise; scientists must proceed on the assumption that a factual basis can be found for discriminating among competing interpretive or typological hypotheses.

Where archaeology is concerned, Spaulding argues that the real world under investigation (the archaeological record and its cultural antecedents) must be assumed to manifest inherent order that can be discovered using scientific techniques of analysis. Persistent patterns of correlation among formal (physical) attributes of artifacts can be objectively identified using statistical measures of association; the resulting attribute clusters are the appropriate basis for typological schemes. Even though factual claims about the record may be mistaken—"our most firmly established 'facts' are probably no more than hypotheses in whose favor there is a great deal of evidence" (1954a: 113)—Spaulding holds that in practice, they are not so completely underdetermined that they lead to vicious circularity. They can provide an independent, empirical foundation that serves not just as a test for interpretive hypotheses but as a ground for inductively generating typological constructs.[12]

Spaulding also insists that statistically discovered artifact types have cultural significance; they consist of "combinations of attributes favored by the makers of the artifacts, not arbitrary procedures of the classifier" (1953b: 305).[13] In some contexts he asserts this as an uncontroversial fact about material culture and the archaeological record, while in others he treats it as a hypothesis about the empirical structure of the record that must be defended on a case-by-case basis. In the first of two rejoinders to Ford, Spaulding argues that the significance of discovered order in any given case "depends on the nature of the assemblage" (1954b: 392) and urges archaeologists to give up their preoccupation with intersite variability (i.e., regional comparisons of the sort undertaken by Ford). Archaeologists should instead focus on the construction of intrasite typologies: the variability within assemblages, typically site-specific assemblages, can be objectively defined and can be assumed to reflect the behavior (and the normative or functional factors shaping the behavior) of those who made and used the material surviving in the archaeological record. Sometimes Spaulding argues, on this basis, that intra-assemblage types are fundamental to chronological (and other) analyses: "historical relevance in this view is the result of sound inferences concerning the customary behavior of the makers of the artifacts and cannot fail to have historical meaning" (1953a: 589). In this spirit he declares

that "any reasonably consistent and well defined social behavior pattern is historically useful, i.e., meaningful in assessing similarities and differences between any two components" (1954b: 392).

What underpins Spaulding's objectivism is his commitment to an epistemological stance he later describes as "liberal positivism" and to the commonsense (metaphysical) realism of a "naturalistic philosophy" (1962: 507).[14] He defends these presuppositions most explicitly in a sharply critical response to an argument for taking seriously an explicitly antipositivist (idealist, coherentist) "critical philosophy of archaeology" published by Lowther (1962) just as the New Archaeologists began to advocate positivism. Ironically, the central reason Spaulding gives for embracing his preferred assumptions is pragmatic: archaeologists should adopt the epistemic and theoretical assumptions that most effectively foster archaeological practice. Spaulding deems Lowther's analysis irrelevant because his "entanglement with idealistic philosophy" leads him to question the positivist assumption that "sensory data provide . . . information about [a world external to the scientist]" that can, in principle, provide grounds for directly or indirectly confirming any "scientifically meaningful" statements about it (Spaulding 1962: 507; see Lowther 1962: 502).[15] In fact, Lowther covers much of the same ground as did the radical critics two decades earlier, invoking contextualist arguments against the assumptions of naive realists and objectivists—assumptions that Spaulding reasserts—concerning the stability and autonomy of archaeological facts, as well as their correspondence to a real (antecedent, cultural) world. While Spaulding acknowledges that factual claims are insecure, he repudiates Lowther's tendency to slide from the recognition that "facts are conclusions," and cannot be presumed to be in any sense "given" (Lowther 1962: 496), to what he describes as the "thunderously erroneous inference that there are no facts" (Spaulding 1962: 508)—precisely the inference that underpins Thompson's shift from contextualist premises to conventionalist and subjectivist conclusions. Spaulding urges moderation: "We do indeed see the world through a glass darkly; the view is distorted and sometimes obscured by our own reflections, but nevertheless we can see something and we can verify our observations with greater or lesser credibility by comparing them with those of others" (1962: 508).

If the implications of Spaulding's concession to the contingency of observation were drawn out, I suspect they would warrant greater sympathy for constructionist considerations than Spaulding allows when he argues the case for typological objectivism against idealists like Lowther and conventionists like Ford. In these contexts he insists that with the right methodological tools, empirically adequate typologies can be constructed that are objective in two senses: they will delineate empirical patterns inherent in the archaeological record and those patterns in turn will capture the normative principles governing antecedent human behavior. Consistent with this stance, Spaulding endorses the ideal, central to McKern's proposals, that technically sophisticated formal analysis can be expected to establish what Ford describes as "ideal-complete" typologies: typological systems that are fundamental to—that provide a basis for and are perhaps a prerequisite to—any other problem-specific analysis, comparison, or interpretation archaeologists might want to develop. It is therefore not a pressing concern for Spaulding that archaeologists should clearly articulate the anthropological and historical problems they ultimately hope to address and integrate their data gathering and systematizing around these problems. The "ultimate objective of archaeology is immutable—it is to achieve a systematic interconnection of facts within the field of archaeological data, and of archaeological data to all other data" (1953a: 589).

Despite Spaulding's conviction that artifact types can be discovered free of intrusive theoretical commitments, it is clear that he, like McKern, depends at every turn on a number of substantive (and controversial) theoretical assumptions about the cultural subject. The difference is that Spaulding explicitly asserts them as general, programmatic presuppositions that inform the archaeological enterprise as a whole, but assumes that they do not determine what statistical manipulation of the data will produce by way of specific typological units. In fact, Dunnell has argued that Spaulding does not succeed in insulating the discovery of types from the influence of these background assumptions. While "Spaulding's inductive approach is designed to be theory free and claims no input from the archaeologist, only the data," he necessarily presupposes some "set of [analytic] categories" (in Spaulding's case, attri-

bute classes); he "simply makes unremarked use of those attribute classes already commonly employed by culture historians" (Dunnell 1986: 180). Dunnell goes on to make a critical point that would have found considerable sympathy among the radical critics of the 1930s and 1940s: "The real problem for both the culture-historical type and Spaulding's associational type is the lack of a general theory that specifies what qualities of artifacts should be used to describe and count artifacts for a particular purpose. Once the theoretical problem was construed as one of method, the development of appropriate theory was sidetracked more or less permanently" (180).

Here the continuity between the typology debates of the 1950s and the interchanges between radical and conservative critics in the 1930s and 1940s is unmistakable. Spaulding makes the case for a methodologically sophisticated version of the sequent stage approach, arguing that properly scientific procedures make it possible to generate typological categories that are independent of any particular (problem- or theory-specific) set of assumptions about the interpretive significance of archaeological material. By increasingly sharp contrast, Brew, Ford, and finally Thompson all insist that such separation of theory from fact is untenable. Carried into practice, the animating ideals of objectivity simply reinforce the suppression of theoretical premises that necessarily inform, in this case, the systematizing as well as the gathering of facts. Although they draw rather different conclusions about what range of theoretical commitments, problem-specific (pragmatic) considerations, subjective factors, or conventions must enter into the construction of typologies, all are suspicious of enthymematic reasoning and of the quest for an "ideal-complete" systematization; all recommend that implicit assumptions be made explicit. In doing so they articulate and, in some cases, significantly extend the central insights of the radical critics, setting up an ever starker opposition between the emergent integrationist and sequent stage approaches.

UNEASY MEDIATION

THE MIDDLE GROUND OF KRIEGER
AND OF PHILLIPS AND WILLEY

Throughout the period of the typology debates there was exploration of options mediating be-

tween the extremes that were most clearly articulated in the exchange between Spaulding and Ford. As early as 1944 and again in 1956, Krieger argued against purely formal typologies; he insists that archaeological typologies must have "demonstrable historical meaning in terms of behavior patterns" if they are to be an effective medium for historical and cultural analysis (1944: 272). Krieger also acknowledges that this position entails a degree of subjectivity (1944: 279)—a reliance on "personal experience" (1960 [1956]: 146)—but unlike Thompson, he does not regard the resulting typologies as unavoidably subjective or conventional. Phillips and Willey (in Phillips and Willey 1953, and in Willey and Phillips 1955, 1958; Phillips 1958) also refused the mutually exclusive options defined by those who insist that if typological structure cannot be treated as an empirical given, it must be an arbitrary construct; they argue, with some amendments along the way, that this sharp opposition of alternatives may be spurious.

The resolutions envisioned by Krieger, as well as by Phillips and Willey, turn on the methodological proposal that typological schemes are neither strictly arbitrary nor inherent in the data; methods of discovery and of construction are both indispensable. Willey and Phillips argue directly that "the actual procedure of segregating types is . . . a more complex operation than is suggested simply by such words as 'design' or 'discovery,' and is in effect a painstaking combination of the two" (1958: 13), but it is Krieger who gives an account of the interdependence between these dimensions of typological analysis. Although archaeologists in the early stages of formulating a typology necessarily rely on background assumptions and subjective intuitions—they initially sort their material experimentally into groupings that "look as though they had been made with the same or similar structural patterning in mind" (Krieger 1944: 279)—these trial groupings must subsequently be tested using the methods of comparative and objective statistical analysis. It is, then, the empirical adequacy of types that Spaulding's techniques can establish; they are not properly methods for discovering (i.e., inductively generating) the types themselves but rather are techniques for the "impersonal *validation* of [typological] results" (Krieger 1960 [1956]: 147; emphasis in the original).[16] To use the language of positiv-

ist/empiricist theories that was later introduced by New Archaeologists, Krieger proposes a distinction between the context of discovery and the context of verification of hypotheses. Like Thompson, who acknowledges a similar distinction, Krieger recognizes that subjective intuitions may be crucial in the generation of hypotheses; but unlike Thompson, he believes they can be excluded from the "probative" testing of hypotheses. In this way objectivity is preserved, but the various features of conceptual context and pragmatic considerations brought into view by constructivists are acknowledged to play a (circumscribed) role.

The difficulty remains, however, of specifying exactly what the process of testing establishes about the types that emerge empirically verified. On Krieger's account, it demonstrates the stability of prospective typological constructs across sites and through time; empirical testing and application determine whether the associated elements "fall together again and again, *in the same essential pattern with the same variations* (1944: 280–281; emphasis in original), demonstrating that the hypothesized typological structure is invariant in a given body of material. A further inferential step is required to establish that these invariant patterns are culturally and historically significant, and Krieger sets out its presuppositions in more detail than McKern or Spaulding had done. He argues, against those who have "stressed the artificial nature of all types," that although types are always in some sense arbitrary,[17] "it may be assumed that in any culture one generation learned from its predecessor that things were done in certain ways in order to achieve certain acceptable patterns of form and aesthetic quality" (Krieger 1960 [1956]: 145–146). Given these interpretive principles—essentially, a normative theory of culture augmented by a sketch of the mechanism by which norms are perpetuated—patterns in the form and quality of archaeological material that prove to be empirically robust can be assumed to reflect conventionally determined (normative) patterns in behavior. Initially Phillips and Willey assert this principle in quite unequivocal terms: "we maintain that all types possess some degree of correspondence to cultural 'reality' and that increase of such correspondence must be the constant aim of typology" (Phillips and Willey 1953: 616),[18] where the "cultural 'reality'" in question consists of patterns of practice that would be "rec-

ognized as norms, the 'right way,' in the societies that produced the objects being typed" (Willey and Phillips 1958: 13). At the same time, however, they acknowledge the difficulty of ever establishing a correlation between the empirical patterns in surviving archaeological material and antecedent cultural norms. In general terms, some such correlation might be assumed to hold; but in most specific cases, "the archaeologist is on a firmer footing at present with the conception of an archeological culture as an arbitrarily defined unit or segment of the total continuum" (Phillips and Willey 1953: 617). By 1958 they further qualify their position:[19] "the archaeologist is on a firmer footing with the concept of an archaeological unit as a provisionally defined segment of the total continuum [of variability in archaeological material], whose ultimate validation will depend on the degree to which its internal spatial and temporal dimensions can be shown to coincide with significant variations in the nature and rate of cultural change in that continuum" (Willey and Phillips 1958: 16–17). The claim of cultural significance remains, but it is cast in highly abstract terms; the validity of a typological unit seems to be conceptualized more in terms of correspondences between different scales of archaeologically defined patterning than between this feature of the record and the cultural norms presumed to be responsible for its production.[20]

Despite such qualifications, the advocates of these mediating positions all assume, with Spaulding and against Ford, that cultural change proceeds at uneven rates, presumably through periods of stability and slow evolution punctuated by relatively rapid transformation of cultural traditions. This presupposition justifies the further, operational assumption that archaeological material can be expected to show definite breaks in distribution and clusterings of traits that will be captured by typological constructs, if Spaulding's methods are used to ensure their empirical fidelity. Unlike the most rigorous contextualist critics of the day, Krieger, Phillips, and Willey evidently believe that even if this inherent structure cannot be discovered by direct statistical inference, it can be established (by systematic testing) on empirical grounds, quite independent of any background assumptions that might play a role in the initial process of generating experimental (hypothetical) typologies. These elements of conceptual context

—assumptions associated with a particular theoretical framework, problem orientation, or set of interpretive assumptions—as well as community conventions and more idiosyncratic subjective intuitions, may be essential to the "indicative" stages of inquiry (to use Thompson's term), but not those of evaluation; the judgment about whether a typological system captures inherent structure is not necessarily or radically underdetermined by the archaeological evidence. In many respects, then, the reconciliation of objectivist and constructivist views of typology preserves the central tenets of a sequent stage approach.

TAYLOR: MEDIATION ON A LARGER SCALE

Of all the archaeologists of this transitional period who debated issues later brought into focus by the New Archaeologists, Taylor is most prominently recognized as a sympathetic forebear and champion of the need for a radical new departure if archaeology was ever to break the grip of a residual antiquarianism and deliver a genuinely anthropological understanding of the cultural past. Taylor was a student of Kluckhohn's and an uncompromising critic of the continued preoccupation with fact gathering that the radical critics deplored; in that regard, Taylor does directly anticipate the New Archaeologists' categorical rejection of traditional archaeology. At the same time, however, Taylor's constructive proposals show as much, if not more, affinity to positions adopted by conservative critics of the period. Although his stated objectives are anything but conciliatory, he in fact articulates a synthesis of integrationist with sequent stage proposals, albeit one achieved even more at the expense of integrationist insights than are Krieger's or Phillips and Willey's mediating positions. The result is a uniquely clear explication of principles that animate both radical and conservative responses to the critique that larger goals were not being met. Conjoined, they create an unstable amalgam in which underlying tensions are explicit; these tensions persist through the 1950s just under the surface of the typology debates and reemerge explicitly in the New Archaeology in the 1960s.

Like the radical critics of the 1930s and 1940s, Taylor stresses again and again the importance of establishing a clear problem orientation. He vilifies the archaeological establishment for failing to sort out pervasive confusions about the "nature of their objectives, their practices, and their conceptual tools"—"it is quite apparent that archaeologists have accepted the admonition to do and die without the encumbrance of reasoning why" (1967 [1948]: 5, 89)—and blames this failure for their consistent inability to produce a substantial body of knowledge about the cultural past. They had, he objects, completely lost sight of their own widely espoused historical and anthropological aims, making little attempt to move beyond the collection and systematization of data; as a result, "description seems to have been an end in itself" (46).[21] Far from providing the necessary foundation for addressing more ambitious questions at later stages of inquiry, the preoccupation with facts threatens to foreclose investigative possibilities; if not informed by anthropological questions, the resulting data would be "virtually useless for attack on cultural problems" (51).[22] When Taylor argues, in addition, that "it behooves the archaeologist not to maintain the untenable position of 'sticking to the facts.' . . . [M]ore interpretation is called for, not less!" (113), he seems to have taken over the whole of the radical critics' campaign against sequent stage approaches in which the priority granted to data collection and systematization is seen as compromising the enterprise as a whole.

But when Taylor addresses the question of how archaeologists might better serve anthropological and historical objectives, he invokes a hierarchy of methodologically distinct stages of inquiry, characterizing archaeology as "no more than a method or set of specialized techniques for the gathering of cultural information"; the archaeologist "as archaeologist" is "nothing but a technician" (1976 [1948]: 41). In this capacity, archaeology plays a foundational role in a "roughly . . . sequential" series of stages unfolding through procedures of data collection and analysis, chronological synthesis, historiographic and ethnographic construction of context, and ethnological comparison of contexts, culminating in the anthropological study of culture as such (its dynamics and statics, nature and workings). Much of *A Study of Archeology* (1967 [1948]) can be read as a systematic explication of just the sequent stage approach against which Kluckhohn and other radical critics had reacted so vehemently.

Taylor does not altogether abandon his critical arguments for problem-oriented research; he in-

sists that "procedure is intimately connected with objectives by being specifically oriented toward their attainment" (1967 [1948]: 37) and suggests that there is one stage prior to data collection, that of problem formation.[23] Nonetheless, the role that specific anthropological or historical problems may play in the initial, distinctively archaeological, stages of inquiry is sharply circumscribed. They enter the context of data recovery and analysis *only* in shaping the choice of an area or site for research and in determining, after the fact, what a researcher will choose to do with the resulting data once it has been collected (153–154). For an archaeologist—a technician responsible for data production—"there can be only one objective: to exploit fully and without abridgement the cultural or geographical record contained within the site attacked" (153).[24] Taylor is adamant that insofar as archaeological investigation inevitably destroys the record, a point to which he frequently returns (153), all influences that might distort or compromise the completeness of the data recovered must be systematically excluded.[25] In short, the archaeological mandate Taylor derives from a commitment to higher-level objectives is to produce as comprehensive and neutral a data base as possible, one capable of supporting all possible (later) problem orientations but specific to none. He enlarges the responsibilities of archaeologists (as archaeologists) beyond mere description only insofar as he requires that the recovery and systematization of archaeological data include, centrally, the identification of "conjunctures" among the constituents of the record. Rather than treating artifacts and features as isolable components of an assemblage—traits or objects that can be detached from context for purposes of description and analysis—archaeologists should record and analyze, as primary data, the relationships among artifacts and features that constitute their archaeological context.

When Taylor insists that archaeologists must more actively serve anthropological and historical objectives, he seems to mean primarily that they must be prepared to take a decisive step *beyond* archaeology. Interpretive inference becomes their responsibility when "the empirical grounds have been made explicit"—"once the empirical information has been presented"—though at that point they cease to operate as archaeologists and "become affiliated with the discipline whose concepts [they] employ and whose aims [they] serve" (1967 [1948]: 155, 153). Taylor clearly understands interpretation to be based on, *not constitutive of,* the observational data of the archaeological record. Even where interpretive hypotheses are to be tested against the record—a component of the method of successive approximations that Taylor recommends for later, nonarchaeological stages of inquiry (165)—they are tested against an existing data base, not data that have been recovered in response to problem-specific demands for particular kinds of test evidence. The antinomy between fact and theory rejected by Kluckhohn is thus reinstated, and the strong thesis of theory-ladenness central to the contextualism embraced by the radical critics is substantially restricted, if not abandoned, without comment.

Taylor's conviction that it is possible to establish a data base that, if comprehensive enough, will support all subsequent stages of analysis, interpretation, or explanation depends not only on a strong epistemic (empiricist) foundationalism but also on substantive assumptions about the cultural subject that are continuous with those that underpin conservative and mediating positions in the typology debate. In fact, one of Taylor's main contributions is a sophisticated account of the normative theory of culture according to which archaeological data, as cultural phenomena, are understood to be a manifestation of the norms and conventions that inform human behavior. Taylor makes explicit the central thesis on which this theory depends: that culture, proper, is entirely a "mental" phenomenon.[26] The behavior of human agents and the material products of this behavior are merely its "objectifications"; they may be cultural, but they are not part of culture itself. This claim has the heartening implication that so far as the study of culture is concerned archaeologists are in no worse a position than ethnographers. Neither has direct access to the contents of mind and community norms that constitute culture proper; all must infer these norms from their observable manifestations. Moreover, Taylor (like others who endorse this normative theory in various forms) evidently assumes a subsidiary thesis that grants causal priority to cultural norms; they, not functional considerations or environmental factors, are the primary source of the patterned variability evident in archaeological material.

It is thus not surprising that Taylor should derive just one general directive for the recovery and systematization of archaeological data from a consideration of higher-order (anthropological, historical) problems: archaeologists must recover information relevant for reconstructing cultural contexts, conceived as systems of norms that are objectified in human behavior and its material products. Operationalized, this directive becomes a requirement that archaeologists recover complete assemblages and pay particular attention to patterns of association among the constituents of the record; these will reveal the "conjunctives," the structure inherent in archaeological data in which, on the normative theory, the ideas and norms constitutive of cultures are manifest. In the end, Taylor can reassert the foundationalism of a sequent stage approach, despite endorsing the radical critics' demand for problem orientation, because he embraces a highly reductive conception of the cultural subject. If culture proper is uniquely a "mental" phenomenon (albeit manifest in a diversity of observable behaviors and their products), then all interpretation or explanation can be expected to deal with some aspect of culture in this sense, and the range of "how" and "why" questions archaeologists might envision answering (qua anthropologists or historians) is sharply delimited. They require information bearing on just one fundamental dimension of cultural life: the norms that structure all aspects of behavior and material culture. Given these theoretical presuppositions, it is plausible that if archaeologists working in the capacity of technicians make the recovery of data relevant for reconstructing cultural norms their first priority, they can expect to produce a comprehensive empirical foundation capable of supporting all subsequent stages of inquiry. The liability of such an approach is that in endorsing it, Taylor abandons not only the robust contextualism of Kluckhohn and other radical critics but also the insight, made explicit by Brew and Ford, that a great diversity of factors shape cultural life in general and archaeological material in particular, generating multiple dimensions of structure that may be discerned in the content and internal associations of archaeological material.

Although the normative theory of culture provides Taylor the rationale for juxtaposing a withering critique of "slightly reformed antiquarian-

ism]" (Kluckhohn's phrase, 1940: 43) with a reaffirmation that the primary business of archaeology is the collecting and systematizing of data, it is still difficult to explain why he abandons so many key features of the radical critique with which he is chiefly aligned. Taylor does grant priority to the stage of problem formation, but in a way so closely circumscribed as to constitute a repudiation of the principle that research must be problem-oriented; the problems that can enter the context of data recovery and analysis are even more generic on his conjunctive approach than in Krieger's testing procedure. And in all this, the central principle of the sequent stage approach is preserved intact: the main purpose of archaeology, qua archaeology, is to secure an empirical foundation as the necessary prerequisite for later stages in which the archaeologist, qua anthropologist or historian, will engage more ambitious goals of synthesis and interpretation. Perhaps Taylor was reacting to the implications of an unequivocal contextualism, as defended by radical critics like Kluckhohn and by the constructivists who engaged these issues in the context of the typology debates. If the facts are, through and through, problem- and theory-specific constructs, is there anything to constrain interpretation? Or is it unavoidably arbitrary (subjective or conventional), as Brew had suggested and as Ford and Thompson were later to claim? And if different problems require different data and typological constructs, so that research at all levels must be integrated around the problems of ultimate concern, does it not follow that any choice of problem also limits the scope of inquiry—perhaps irrevocably, given the destructive nature of excavation?

Whatever his motivation, Taylor comes down decisively on one side of the issue Kluckhohn had left open in 1940 when he suggested that archaeologists might legitimately define their goals either in historical or in scientific/anthropological terms and had mused inconclusively about their interdependence. Taylor sets aside Kluckhohn's residual worry that in practice, "the material collected and published by the 'historically' minded is seldom suitable for 'scientific' analysis," and embraces the suggestion that these might fruitfully be regarded not as "two distinct types of interest . . . but [rather as] two *sequent phases* of a planned research" (Kluckhohn 1940: 49; emphasis added). Given his commitment to a normative

conception of culture, Taylor can envision a sequent stage approach in which data relevant to all problems relating to a cultural subject—data relevant for reconstructing cultural norms—are collected and systematized in an initial archaeological phase of inquiry. It is a profound irony that Taylor should later be identified as the one figure in the transitional period of the late 1940s and 1950s who clearly anticipated the New Archaeology. While he was a sharp critic of atheoretical, unreflective traditional practice, his main contribution was a uniquely clear and systematic account of the dominant presuppositions of the period: the residual empiricism of a sequent stage methodology and the normative theory of culture. These were the central assumptions in opposition to which the New Archaeologists defined their program.

3

The Conceptual Core of the New Archaeology

By the late 1950s, when Meggars (1955) remarked on the "new look" evident in North American archaeology and Caldwell (1959) marked the end of an era—the maturation of the discipline beyond its "natural history" phase—internal critics (both conservative and radical) had already struggled for twenty-five years with the question of how best to institute properly scientific, anthropological forms of practice.[1] Indeed, these goals had been articulated forty years earlier, when Wissler declared the need for a "real, or new archaeology" (1917);[2] they were by no means unique to the New Archaeologists of the 1960s, who, within a few years of Caldwell's and Meggars's reviews, issued the most ambitious and influential demands for a transformation of practice that had appeared to that point. What distinguishes this most recent new archaeology from earlier initiatives is the impact it has had on the discipline, becoming "everybody's archaeology" in less than a decade (Leone 1971: 222).

The New Archaeology was initially defined in opposition to traditional forms of practice; by the 1960s, these included not only the preoccupation with fact gathering that had concerned Kluckhohn and earlier critics, but also various forms of space-time systematization that were the object of debate through the 1950s. In particular, the 1960s advocates of a New Archaeology rejected the quest

for an "ideal-complete" data base or typology and any related form of sequent stage approach in which the questions of ultimate (anthropological) concern are disconnected from data recovery or analysis and deferred to "a future time of greater leisure and fullness of data," as Strong had put it (1936: 365). Their reasons for this rejection closely parallel Kluckhohn's: they repudiate any lingering faith that facts of the record can be treated as empirical givens, invoking Kuhn's contextualist arguments for theory-ladenness, and argue that the ambitions of traditional archaeology had been compromised by implicit commitment to the presuppositions of an untenable empiricism. The constructive program of the New Archaeology is defined by a categorical requirement that archaeological research be problem-oriented: all aspects of inquiry must be explicitly designed to serve the anthropological goals of ultimate concern. The New Archaeologists thus affirm the integrationist approach advocated by radical critics in the 1930s and 1940s, decisively rejecting any suggestion that archaeology can be conducted as an autonomous (technical) fact-gathering and systematizing enterprise that is neutral with respect to (and that can be expected to support) the diverse explanatory objectives of archaeologists qua anthropologists and historians.

At the same time, however, the New Archaeol-

ogists resist any suggestion that their contextualist arguments—for example, their insistence on the theory-ladenness of facts and the problem specificity of typological constructs—entail the conventionist or subjectivist conclusions drawn by Brew and James A. Ford, and later by Thompson. In fact, they identify traditional archaeology as much with conventionalist and subjectivist positions as with the more long-standing forms of practice that make fact gathering or systematizing an end in itself; when they reject traditional archaeology as a whole, they are often as intent on challenging the putative antithesis of radical constructivism as the thesis of cautious descriptivism. Their critical analyses suggest that they consider traditional archaeology in all its forms to be predicated on empiricist assumptions about the nature and source of legitimate knowledge. Although never made explicit, the argument linking these divergent strands of traditionalism to shared empiricist premises appears to run along the following lines. If it is assumed, by way of an epistemic ideal, that legitimate knowledge claims must be strictly derived from or reducible to an autonomous foundation of empirical givens, and if it can be shown that explanatory, interpretive claims about the cultural past invariably overreach any empirical facts that can be adduced from the archaeological record (i.e., archaeological evidence underdetermines knowledge claims about the cultural past),[3] then it seems to follow that archaeologists have no option but to embrace broadly speculative (conventional or subjectivist) modes of inquiry. As Brew, Ford, and Thompson had argued, archaeologists must rely on nonempirical—indeed, noncognitive—considerations (subjective intuitions, community conventions, social or political interests) when they choose among alternative claims about the cultural past. The limitations imposed by narrow empiricism on what can count as the legitimate basis for knowledge render speculation unavoidable if archaeologists insist on doing more than simply describing and systematizing the contents of the archaeological record.

The New Archaeologists thus extend Kluckhohn's analysis of the consequences that follow from the implicit empiricism of traditional practice. Narrow empiricism does more than reinforce antiquarian tendencies, justifying forms of practice that make fact gathering or, increasingly, systematization an end in itself. It also generates a dilemma that confines archaeologists to one or another of two unpalatable options: either they can stick to the facts, with or without the conviction that doing so will support subsequent interpretive inquiry, or they can abandon the scientific ambition of producing secure, empirically grounded knowledge of the past and make their reliance on subjective, conventional (context-specific) factors explicit. Strategies for ensuring that archaeological inquiry meets its interpretive, explanatory goals will be polarized around these options so long as archaeologists remain in the grip of untenably narrow epistemic ideals, ideals that are characterized by the New Archaeologists, as much as by the radical critics of the 1930s and 1940s, as a form of empiricism.

The central and defining ambition of the New Archaeology, as articulated in the programmatic statements of the 1960s and early 1970s, was to break this cycle of dilemmic debate. The proponents of a thoroughly scientific and anthropological archaeology undertook to combat not so much an explicit commitment to neo-antiquarian modes of practice as a growing skepticism about the prospects for realizing anthropological goals in a manner consistent with scientific ideals.[4] They countered this skepticism with the (antiempiricist) argument that the limits of archaeological knowledge are not defined by the contents of the archaeological record. Given sufficiently rich theoretical resources and an appropriately rigorous scientific methodology (specifically, a problem-oriented testing methodology), archaeologists can extend their understanding beyond the sensory givens of the record without lapsing into arbitrary speculation. This optimism reflects a conviction that if archaeological data are properly (i.e., scientifically) exploited, they can be expected to impose sufficiently limiting evidential constraints on interpretive claims about the cultural past that archaeologists will have systematic, empirical grounds for adjudicating between these claims. The dilemma posed by narrow empiricism does not exhaust the options open to archaeology.

THE NEW ARCHAEOLOGY AND THE TYPOLOGY DEBATES

Nowhere are the broad outlines of the New Archaeologists' project clearer than in Hill and

Evans's response (1972) to the issues that had animated the typology debates of the 1930s through the 1950s.[5] They reject the quest for "all-purpose, standardized typologies" on the grounds that it depends on the untenable empiricist assumption that (archaeological) knowledge derives from a foundation of discrete phenomenal items, each of which has "a single meaning, or very few meanings at most" (1972: 237, 233). Although the classical empiricists they cite would never attribute inherent *meaning* to the elements of sensory experience that constitute the foundation of legitimate knowledge—they cite Hume, Mill, and Bacon in this connection (233)—the archaeologists whose positions they criticize do introduce this assumption as the basis of a normative theory of culture.[6] Hill and Evans counter both sets of presuppositions, arguing that there is no fundamental, empirically discoverable structure inherent in the archaeological record that can be treated as "the basic data, the building blocks for inference" (241) when captured by a typological scheme, and that no unique (normative) meaning can be attributed to such empirical variability as archaeologists can discern in the record.

Hill and Evans develop two lines of critical analysis to support these conclusions: an epistemic critique of the implicit empiricism and a more pragmatic challenge to the normative theory of culture. Concerning the empiricist assumptions about inherent (real, or objective) structure, they observe that actual practice makes it clear (inadvertently) that any body of archaeological data can be classified in a range of different ways, depending on the purposes at hand. If researchers are concerned with temporal, historical change, or with functional variability, as opposed to cultural (qua normative) considerations, they focus on very different dimensions of variability and produce very different typological schemes (Hill and Evans 1972: 241–245). This diversity is, in fact, largely responsible for the ongoing controversy over typological schemes. To assume that there is *an* inherent order on which an "ideal-complete" typology (as Brew describes it) could be based is to suppress the fact that "*choices* must be made"; there is a "virtually infinite number of attributes connected with any item, and it is physically impossible to take account of them all, or even more than a small percentage of them" (251, 250; emphasis in the original). Although Hill and Evans

make no reference to Kluckhohn, this line of argument is reminiscent of his objections to the enthymematic reasoning engaged in by would-be empiricist practitioners in the 1930s. Whatever their claims to comprehensiveness and objectivity, even their most deliberately neutral descriptions of archaeological material, let alone their systematizations of it, are informed by some implicit problem orientation and theoretical assumptions: "We cannot be objective and select observations or attributes in an unbiased manner. We are all biased, especially by our problems and the general theoretical paradigms to which we subscribe. And since we would be fooling ourselves to think we can escape bias in selecting attributes for our typological analyses, we argue that it is important to recognize precisely what our biases are[,] . . . [specifically,] what our types are to be used for" (Hill and Evans 1972: 252).

The suppressed bias that informs the quest for standardizable typologies is, on Hill and Evans's analysis, the commitment to a *normative* theory of culture and to systematizations that capture cultural variability (of this sort) over time and space. This, they declare, is as untenable as the empiricist methodology with which it is associated. Hill and Evans's second, albeit less fully developed, critical argument is that the proliferation of typologies in actual practice undermines any assumption that cultural reality can be narrowly identified with cultural norms or that archaeological material, as cultural, has inherent significance as an objectification of these norms (to use Taylor's term); "types are not manifestations of just one thing," Hill and Evans argue (1972: 254), invoking Lewis Binford's critique of normative theories of culture. What significance types have must be postulated as a hypothesis about the specific aspects of cultural behavior with which the material in question might have been connected (254); no one set of connections can be assumed to predominate a priori, given the dictates of an entrenched interpretive theory.

These arguments establish not only that the ideal of establishing a comprehensive, all-purpose typology is simply "unworkable" (Hill and Evans 1972: 250), but that the broader methodological strategy associated with it, the sequent stage approach, is also impracticable. Hill and Evans are clear on the point that empiricist presuppositions yield a "general methodological paradigm"

according to which "classification [and data recovery] comes most properly *before* analysis and interpretation" (234; emphasis in the original),[7] rejecting this paradigm on the grounds that it suppresses the theoretical assumptions and problem-specific choices that necessarily inform the empirical foundation-laying stages of inquiry. In the spirit of making such presuppositions explicit, and in recognition that any attribution of meaning to archaeological data is inferential, Hill and Evans insist that research must "*begin* with . . . problems, tentative inferences or hypotheses about the materials . . . observed, and then proceed to select the kinds of attributes that [the investigator] feels will lead to typologies that will be useful to [their] particular analysis" (252, 253; emphasis in the original). Here they build on Krieger's proposal that typological constructs must be tested; while Krieger treated such constructs as descriptive claims, Hill and Evans acknowledge their interpretive content, arguing that typological analysis must be integrated into a process of formulating and testing interpretive hypotheses about the meaning of the record, broadly conceived.

Despite strongly, if unintentionally, reaffirming the central arguments against various forms of empiricist practice developed by earlier radical critics,[8] Hill and Evans vigorously resist the slide into the wholesale conventionalism or subjectivism that they associate specifically with Brew and Ford. The demonstration that "ideal-complete" typologies are unattainable and that researchers inevitably exercise some theory- and problem-informed choice in the construction of their multiple typological schemes does not entail, they insist, that "the clusters of attributes we call types have no reality" or are artifacts of a "completely arbitrary" imposition of interpretive meaning (Hill and Evans 1972: 246). Types may be real and discoverable at least in the sense that they capture nonrandom associations of attributes of the record; Spaulding is credited with having properly rejected radical constructivist claims on these grounds. But it also follows from this realism about the patterns of association or distribution captured by types that whether or not nonrandom variability exists in a given body of data or on a given dimension among chosen attributes, whether it is discontinuous, and what its cultural significance is are all empirical questions that cannot be settled in advance of investigation. On this analysis, Spaulding was as misguided as Ford in making categorical assertions about the reality or unreality of types and of the empirical conjunctions of the attributes that they are meant to capture. It is the job of the archaeologist to find out what (or indeed whether) systematic patterning exists in the data and what its significance is as evidence of the cultural past, not to make "procla[mations] . . . on the basis of intuition" (Hill and Evans 1972: 267).

The vision Hill and Evans hold out as an alternative to empiricist modes of practice and their skeptical antitheses is that of a genuinely new archaeology based on the epistemological principle that even if the archaeological record does not determine a unique interpretive conclusion or systematizing scheme, it can provide an empirical basis for assessing the relative plausibility of competing hypotheses. They endorse the contextualist insight that typological systems are constructs—they are not in any sense given in the archaeological data themselves—but Hill and Evans insist, nonetheless, that the theoretical or problem-specific assumptions that inform the initial construction of a typology do not ensure that the data archaeologists recover will obligingly conform to their expectations about either its structure or its significance. Hill and Evans cite Watson, LeBlanc, and Redman in this connection: "the attributes one chooses to work with should reflect one's problems, whereas the types defined by those attributes should reflect the real world" (P. Watson, LeBlanc, and Redman 1971: 27; cited in Hill and Evans 1972: 262). Without arguing the case directly, they evidently hold that theory-ladenness and problem specificity are *not* all-encompassing or all-pervasive. Where they, and the New Archaeologists generally, stress the potential for systematic testing to secure ampliative inferences about evidential significance (i.e., inferences that go beyond description of the data to posit its cultural significance), they circumscribe the role of evaluative and theoretical commitments in much the same way as Krieger had done. Assumptions about the nature of the subject domain, interpretive conventions, and problem-specific or pragmatic considerations may play a role in the context of discovery—they may shape both problem choice and hypothesis formulation—but they can be eliminated from the context of verification. Ar-

chaeological data are presumed to be robust enough to provide a genuine test of intuitions and favored hypotheses, however they are generated.

The irony is that Hill and Evans, like other New Archaeologists, characterize their dilemma-breaking alternative as positivist and suggest that differences in view about the status and source of typological constructs arise from "fundamental epistemological differences between this ['empiricist' model] and the 'positivist' model" of practice (1972: 233). They seem unaware that positivist theories of science are a species of empiricism both in genesis and in content; if anything, it is positivists who most aggressively insist on limiting the scope of legitimate knowledge to its empirical foundations. Perhaps Hill and Evans intend to endorse something like Spaulding's liberal positivism, which makes respect for *empirical* findings the first virtue of scientific inquiry without embracing the principles of more stringently *empiricist*/positivist theories of knowledge. But considerable ambiguity about the presuppositions of that liberal positivism remains; Hill and Evans do not give a clear account of how their positivism constitutes an alternative to standard empiricist assumptions about the sources and ground of knowledge that underpin logical and classical positivism. I will argue that this ambiguity is the source of serious difficulties for the New Archaeology as a whole.

THE PROGRAMMATIC CORE OF THE NEW ARCHAEOLOGY

By the time Hill and Evans proposed their resolution to the typology debates (1972), the New Archaeology was well established, at least in programmatic terms and as a rallying point for those disaffected with traditional archaeology. It took shape initially in the polemical "fighting" articles that Lewis Binford published, beginning in 1962 with "Archaeology as Anthropology" and followed, in rapid succession, by constructive analyses of research strategies for studying cultural process (1964, 1965) and by a series of critical reviews of traditional research practice. At the same time a number of dissertation projects were taking shape that embodied the theoretical and methodological concerns championed by the New Archaeologists,[9] while established practitioners in a wide range of subfields were turning their at-

tention to the sorts of problems and projects outlined by Binford and others (e.g., P. Martin 1971). The result was a groundswell of enthusiasm for the New Archaeology, most clearly manifest in contributions to the symposium "The Social Organization of Prehistoric Societies," which was organized by Binford for the 1965 Annual Meeting of the Society for American Archaeology in Denver. When the proceedings of these meetings were published three years later as an edited volume, *New Perspectives in Archeology* (S. Binford and Binford 1968), the New Archaeology was already a strong, well-defined presence in North American archaeology.

It is interesting to note that just a year before *New Perspectives* appeared, Flannery had described North American archaeology as split three ways (1967: 119): a majority, "perhaps 60 percent of all currently ambulatory American archaeologists," were unreconstructed culture historians; a vocal minority, Flannery estimates 10 percent, were proponents of the "process school"; and the rest were unaligned critics of both culture history and processual approaches. When Leone reviewed *New Perspectives* four years later, however, he described a discipline in which the crisis of transition was past:

> The period of rapid change in American archaeology began ten years ago. The bulk of research reported in *New Perspectives* was done during the first half of the last decade. It is research that represents the thorough revitalization of anthropological archaeology. . . .
>
> . . . [Contributions to] this volume represent the first serious innovations in archaeology since the 1920's. They represent a change on so many levels of analysis that they may be pardoned while they experiment.
>
> The battles and confrontations that the work in *New Perspectives* provoked have died down and these men, their colleagues, and the problems they attend represent the undisputed frontier in archaeology. If anyone thinks a revolution did occur, these same must now think the revolution is over. Suddenly the new archaeology is everybody's archaeology. The rhetorical scene is quiet. (1971: 222)

Two years later, when Flannery (1973) published a retrospective assessment of the state of the New Archaeology, he found himself addressing an established processual archaeology whose

major challenges were coming from within—not from culture historians or those in intermediate positions "who aim their fire freely at both history and process," as he had earlier described the situation (1967: 119), but from would-be processualists who were rapidly becoming disenchanted with the New Archaeology. These internal critics were confused "about what the new archaeology is," mistrustful of "inflated evaluation[s] of what it ha[d] accomplished," (1973: 48) and, most significant, divided among themselves on many substantive issues, including the relevance and applicability of positivist models of science to archaeological problems.[10] Hill and Evan's New Archaeology analysis of the typology debates thus appeared at a point when the New Archaeology was being rapidly assimilated to the mainstream.[11]

Binford was indisputably the main architect of the New Archaeology and the catalyst for its meteoric rise.[12] Although he was by no means alone in setting the course of the New Archaeology, his formulation of its central principles is particularly interesting both because of his wide influence and because it incorporates, from the outset and in especially clear terms, the conceptual tensions later responsible, I will argue, for the widespread critical reaction against the program that dominated internal debate after the early 1980s. My aim here is to "trace the rocky path traveled by processual archaeology since Lewis R. Binford gave it national exposure in the 1960s," as Flannery has described this development (1973: 48), focusing first on the conceptual foundations that Binford laid in this period. In later chapters I consider the internal debates that arose when he and others undertook to build substantive programs of empirical research on those foundations.

BINFORD'S *NEW PERSPECTIVE:* CRITICAL ANALYSIS

Binford's campaign for a new archaeology was motivated by frustrations with traditional research practice that were, in fact, shared by many of those he identified as traditional archaeologists. As Taylor pointed out, the triad of objectives typically endorsed by North American archaeologists in the 1940s and 1950s were the study of culture history, past lifeways, and cultural process; in principle, at least, data recovery and systematization was a means to larger ends, not an end in itself. But the results of long preoccupation with establishing an orderly, comprehensive foundation of archaeological data were equivocal. Anthropological objectives remained largely unrealized and it was not at all clear that the rapidly accumulating data base could support them even if the systematizing projects of the 1950s were to succeed. Nowhere is this pessimistic assessment clearer than in Paul S. Martin's account of his conversion to New Archaeology:

> Long before my dissatisfaction and unfulfillment became articulate, a few archaeologists and anthropologists from 1930 on had concluded that our traditional methods were leading them astray, down dead ends, and up against blank walls. . . . We were in a cul-de-sac because comparing forms and systematizing our data were not leading to an elucidation of the structure of social systems any more than the ordering and taxonomy of life forms by Linnaeus explain the process of organic evolution. (1971: 3–4)

Reflecting on this situation in the late 1960s, Binford observed that "there began to appear in the literature a general dampening of enthusiasm" among many who had held out hope for "processual investigations" twenty years earlier, as they expressed increasing pessimism about the prospects of ever realizing processual aims (1968a: 7). It seemed unlikely that standard modes of practice would put traditional archaeology in a position to achieve any of its three main objectives. Binford's diagnosis of the problem (see 6–8) follows exactly the contextualist line of reasoning developed by radical critics twenty years earlier. He argues that archaeologists cannot separate the task of recovering and systematizing archaeological data from that of interpreting and explaining it and expect the resulting data base to yield (or support) credible conclusions of any kind, processual or historical. Unlike Kluckhohn, who equivocated on the question of whether a sequent stage approach might prove viable, and unlike Taylor, who endorsed a version of that strategy, Binford came down strongly on the integrationist side of the debate; he demanded thoroughgoing problem orientation at every level of the research enterprise.

There are several important respects in which Binford deepens the critique of sequent stage approaches developed by radical critics of the late 1930s and 1940s. He objects that even when traditional archaeologists attempt culture-historical interpretation, inferring "genealogical affinities" between cultural groups or historical events on the basis of formal similarities evident in their surviving assemblages, they produce little more than a gloss, in cultural language, on descriptive systematizations of the archaeological data. Their attempt to identify cultural processes of transmission across generations or cultural boundaries by analyzing patterns in the spatiotemporal distribution of clusters of formal attributes (Willey and Phillips's was the most comprehensive such synthesis; 1955, 1958) likewise fails to carry archaeological research into the realm of historical or processual inquiry. However ambitious in scope these syntheses might be, they are nothing more than a "generalized narrative of the changes in composition of the archaeological record through time" (L. Binford 1968a: 11). They might allow archaeologists to formulate questions, to specify in archaeological terms (apparent) changes in cultural tradition that require explanation, but on their own they provide no historical or processual understanding of the record or of the past that produced it (8–14). Binford was equally critical of attempts to reconstruct the lifeways distinctive of the cultural entities whose histories are traced by space-time syntheses. Typically these depend on analogical projections onto the past of ethnohistorically documented lifeways that incorporate forms of material culture like those found in the archaeological record. And, Binford insists, this strategy of interpretation is just as arbitrary and uninformative as simple redescription of the data in culture-historical terms: "fitting archaeological remains into ethnographically known patterns of life adds nothing to our knowledge of the past" (13).

The failure of traditional archaeology to move beyond description arises, Binford argues, from two root causes: traditional researchers' reliance on inductive methodologies and their commitment to a normative theory of culture. Consider, first, the critique of inductivism; I examine Binford's critique of normative theories in the next section. When Binford describes the methodologies associated with the culture-historical synthesis and with the reconstruction of lifeways as inductive, he is generally referring to the practice of starting with an assembled body of data and *then* drawing from it, or superimposing on it, interpretive and systematizing conclusions about its structure and significance. But the specifics of his analysis make it clear that he has in mind two different sorts of inductive extrapolation beyond the data.[13]

Simple induction, which generates empirical generalizations about patterns or structures of "conjunction" evident in the record, produces what Binford would later call "general facts" about the record or its similarities with material culture in the ethnographic present (L. Binford 1977b: 5; see also 1968d); the sense in which this strategy of systematizing induction fails to meet interpretive goals is obvious. By contrast, more ambitious induction (properly, ampliative inference), which does yield substantial cultural or historical conclusions, is problematic because it lacks the necessary "final link in scientific procedure" (1968a: 14): systematic testing of reconstructive or interpretive conclusions against the archaeological record. Binford's point is that the evidential import of archaeological data will never be grasped by pressing the possibilities of descriptive analysis to its limits: "Facts do not speak for themselves, and even if we had complete living floors from the beginning of the Pleistocene through the rise of urban centers, such data would tell us nothing about cultural process or past lifeways unless we asked the appropriate questions" (1968a: 13). It is crucial that the interpretive and empirical (descriptive and systematizing) dimensions of inquiry be integrated at least to the extent that claims about the significance of specific bodies of material are treated as test hypotheses in relation to this material, rather than as post hoc conclusions fit to it. If the results of testing are not the basis for accepting such hypotheses, Binford insists that we indeed have no alternative but to follow Thompson's lead and "evaluate reconstructions or interpretations by evaluating the competence of the person who is proposing the reconstruction[, which is] . . . scarcely sound scientific procedure" (L. Binford 1968d: 270).

Binford extends earlier critiques of sequent stage inductivism in another sense as well: not only is it counterproductive to defer interpretation until the facts are in, it is implausible that proces-

sual goals will be served by giving priority to historical interpretation as a necessary first step beyond descriptive systematization. He makes this point most clearly in a critical review of Sabloff and Willey's defense of the need to adopt a "first things first" approach, contra Binford's demand for the comprehensive reorientation of practice around processual problems. They had argued, with explicit reference to Binford's proposals,[14] that "only through an understanding of the historical events . . . can the larger question of process be successfully broached"; "the best way to get answers to the processual problems . . . is through the building of a proper historical framework" (Sabloff and Willey 1967: 314, 330).

While Sabloff and Willey's position seems on its face entirely reasonable, Binford objects that historical reconstruction in fact depends on "unstated processual presuppositions" (1968d: 270). Here he exploits an ambiguity in what he means by processual understanding or inquiry. When he claims that "a proper historical perspective cannot be gained without coping with processual problems" (270), he seems to say that the historical significance of the record can be grasped only when, or if, the processes responsible for its production are understood. He thus argues that the kind of historical understanding of past events sought by Sabloff and Willey is not merely descriptive but explanatory—it is "only through explanations of our observations that we gain any knowledge of the past"—and that explanatory understanding, even at this relatively particularistic level, depends on general propositions, "laws of cultural or behavioral functioning" (269–270).[15] Indeed, he declares that these culture-historical explanations are "*processual* hypotheses that permit us to link archaeological remains to events or conditions in the past which produced them" (270; emphasis added). Here "processual understanding" includes not just a broadly comparative and explanatory understanding of cultural systems, which is the ultimate aim of anthropological inquiry, but any understanding of generative (or causal) processes that operate in a cultural context, linking its constituent variables internally and with elements of its environment; it includes all those low-level (later called "middle-range") processes by which particular types of cultural context or human action produce a distinctive material record.[16] Culture-historical inquiry that

proceeds without taking up processual questions in this second sense is condemned to speculation not just because interpretive hypotheses are posed as the conclusions to research without systemic empirical testing, but because effective testing is impossible without a processual understanding of the causal connections that link archaeological material to its cultural antecedents.

Given this expansive construal of what counts as processual explanation, Binford reframes long-standing integrationist arguments against deferring the goals of processual inquiry. He does invoke the familiar argument that unless archaeologists keep clearly in mind questions about cultural process, they are unlikely to recover data relevant to these questions. But he emphasizes the further objection that unless integration is realized at all stages of inquiry, the conclusions drawn at a processual level will remain just as much speculative (inductive) extrapolations beyond the data as the culture-historical conclusions he rejects.[17] Even the most conservative (i.e., the least ampliative) forms of interpretive and descriptive reconstruction depend on processual understanding in the low-level sense that emerges in Binford's rebuttal to Sabloff and Willey; archaeological data will not stand as evidence either of particular past events and conditions or of large-scale cultural processes unless explained in light of a body of established "processual propositions" about how these data might (or must) have been produced (1968d: 270). Traditional archaeologists could not but fail to their objectives inasmuch as, following a sequent stage (inductivist) approach, they missed "a first step [which] . . . *necessarily* involves coping with problems of process" (273; emphasis in the original): centrally, the problem of formulating and testing linking hypotheses about the sets of conditions capable of producing material like that observed in the archaeological record.

NORMATIVE THEORY

Binford's second argument against traditional archaeology is that it does indeed depend on interpretive principles, despite claims of theory and problem neutrality, but on principles derived from an untenable general theory of culture. His objection to the speculative nature of culture historical interpretation arises not just from a logical

point—that the inferences by which traditional archaeologists attribute cultural significance to archaeological data are insecure—but equally from an independent set of arguments against the theoretical assumptions that underwrite those inferences. He challenges the assumption that the patterning observable in archaeological data can be treated as the outcome of one kind of generative process, namely that by which the "mental templates," or norms and regulative ideals constituting cultural traditions, are objectified in the behavior and in the material products of the human populations bearing these traditions.

As Binford describes this "aquatic view of culture," it comprises five distinct claims (1965: 205).[18] The first is that culture is reducible to a single component—ideas or norms—directly objectified in cultural behavior and material products. The second, which follows from the first, is that culture can be conceived as an aggregate phenomenon composed of an inventory of shared ideas. Thus as a growing body of material objectifications of a particular past culture are recovered (e.g., through excavation of the archaeological record), they can be expected to fill in a picture of the norms and ideas constituting that culture; these norms are identical with (or are directly manifest in) central tendencies in the characteristic material objects produced by the bearers of the culture. These first two tenets of the normative theory provide a general characterization of culture as an assemblage of norms and ideas; three additional claims concern the model of cultural dynamics that accounts, in normative terms, for the transmission and diffusion of culture and for its material (and archaeological) manifestations. The third component of the normative theory of concern to Binford is that culture, as a mentalistic phenomenon, is assumed to be transmitted either by learning in the process of socialization (whose details and biological basis were specified by Taylor 1967 [1948]: 95–116) or by contact between contemporaneous populations; the distribution of material culture traits in space is attributed to the movement of culture-bearing populations. Given this conception of the content or form and dynamics of cultural phenomena, it follows, fourth, that similarities and differences in the formal traits characterizing spatially and temporally distinct assemblages can be considered a measure of the degree to which individuals or populations

share the same culture or "genealogical affinity" (L. Binford 1968a: 8). Finally, where "degrees of similarity . . . are a measure of cultural affinity" (L. Binford 1972a: 331), the fifth principle is that discernible breaks in the distribution of associated sets of traits can be assumed to represent boundaries between distinct cultural entities, analogous to and indicative of the existence of distinct ethnic groups that display an integrity through time and space. In a range of polemical publications that appeared in the 1960s and early 1970s Binford challenges each of these components of normative theory.

The central problems with normative theory, on Binford's account, are that it had long proven untenable in archaeological application and is manifestly implausible as a general theory of cultural phenomena. The more specific argument turns on the observation that archaeologists regularly encounter patterning in the record that violates the expectations of the normative model —particularly as articulated in the last two principles outlined above, which most directly mediate archaeological inference. It is most fully developed in Binford's critique of Bordes's attempts (1961, 1968) to make sense of the variability evident in Mousterian assemblages in normative terms, where the patterning within spatially and temporally associated bodies of material proved incongruent across dimensions of variability; that is, the "patterning in one characteristic . . . varie[d] independently of patterning in other characteristics" (L. Binford 1972b: 259; see also 1968c). This incongruence violates the expectation, articulated in the second and fourth assumptions, that cultural traditions are integrated wholes and will produce stable associations of covarying traits by which, on the fifth principle, they can be identified and distinguished from one another. In the Hope Fountain–Acheulian test case, variability in the associations of the tool types making up site-specific assemblages did not correspond with morphological variability within these tool types (1972b: 260–263); traits presumed distinctive of discrete ethnic groups frequently co-occurred within a given stratum on a single site, or alternated in interleaved strata. Binford objects that in order to maintain the normativist view that this variability reflects distinct cultural identities, Bordes had arbitrarily shifted the level of analysis from individual traits to whole assemblages, min-

imizing the anomalous variability that was emerging at finer levels of resolution. That ad hoc move simply forestalled recognition that this case, and others like it, challenges normative assumptions about the nature and sources of variability evident in the archaeological record. Binford concludes that given this empirical violation of some key assumptions about "the nature of the archaeological record," it must be accepted that "little if any of the variability thus far demonstrated in the archaeological record prior to the upper paleolithic is referable to 'ethnic' units of hominid populations which were 'culturally' bounded" (1972b: 291).

These difficulties are not unique to Bordes's treatment of the variability in Mousterian assemblages; they are symptoms of a deeper problem with the normative theory of culture that Binford had begun to articulate as early as 1962. Like Ford a decade earlier, he had long argued that it is simply implausible to treat culture as a "univariate phenomenon" or to presume that its form and dynamics might be "explicable by reduction to a single component—ideas" (1965: 205).[19] As they stand, however, Binford's assertions are unsubstantiated counterintuitions about the nature and dynamics of cultural phenomena. His finding that archaeological material frequently violates normative expectations concerning the structure of variability in the objectification of cultural norms is significant because it provides him with empirical, and pragmatic, grounds for challenging the "ethnic unit" conception of culture deployed in culture-historical analysis. He could argue, in explaining Bordes's difficulties, that interpretive models based on a normative theory of culture cannot deal with the complex structures of variability encountered in the record because patterning in material culture is not (only) an objectification of norms or ideas; in fact, such patterning cannot be attributed to any single variable and its associated mechanisms of transmission and transformation. Working back from these empirical (archaeological) difficulties to the final two constituent claims of normative theory, Binford calls into question the assumption that culture can be defined in reductive mentalistic terms (the first two assumptions of the theory), and then challenges the third component of the theory, the claims concerning cultural dynamics: "in no way can ideational innovations or communication of knowledge or ideas be cited as a sufficient cause

for change, variability or stability." Binford continues, "We must first understand the forces operating on a socio-cultural system as a whole, then we may understand the causal nature of changes which we might observe within one of its component parts" (1971: 23).

If these arguments against the normative theory of culture are accepted, then traditional inductive (ampliative) claims about the culture-historical significance of archaeological data do either reduce to descriptions of the data or to unsecured speculation—but not simply as a consequence of the logical structure of traditional interpretation, as Binford sometimes suggests. If normative theory were understood to offer an approximately true account of the nature of cultural phenomena and the way in which material culture (and, ultimately, the archaeological record) is produced, it would provide traditional archaeologists with strong grounds for inferring that the archaeological record reflects (or objectifies) the cultural norms that governed past human (cultural) behavior. Taken together, the five components of normative theory function as interpretive principles that, if accepted, establish systematic linkages between archaeological material and antecedent cultural ideas that support normative attributions of meaning to archaeological material. Structurally, inferences based on these assumptions are no more inductive (qua ampliative) than what Binford proposes for processual archaeology in urging that conceptual links be established to secure claims about the evidential import of the record.

What Binford objects to, in his extended critique of normative theory, is not the lack of linking principles, or the role that they play in culture-historical interpretation, but the inadequacy of the principles on which traditional archaeologists depend; their arguments are not so much invalid as unsound. By raising general questions about the plausibility of normative theory, Binford shows that the interpretive conclusions drawn by traditional archaeologists are speculative in the sense that they could be true, but the grounds cited for accepting them provide them little support. If, in addition, he were able to show that the linking principles supplied by normative theory are (probably) false on empirical grounds, then he would be in a position to make the stronger claim that the interpretive hypotheses accepted by tradi-

tional theorists are not only speculative but are (probably) false. This is the significance of his argument against Bordes: that the structure of patterning in the archaeological record subverts the expectations of a normative theory provides indirect empirical grounds for suspecting that the normative theory is problematic. His objection to culture-historical interpretation more generally is not just that it is inductive (ampliative) and therefore speculative in form, or that it lacks the theoretical backing of interpretive principles that connect archaeological data to antecedent cultural behavior, but that the theoretical principles on which it depends are at best unsubstantiated and at worst empirically and conceptually unsustainable.

CONSTRUCTIVE PROPOSALS

Despite the centrality of the foregoing critical arguments to all Binford's proposals for a new archaeology—he defines his program largely as an antithesis to the forms of practice he rejects—from the outset his frustration with traditional archaeology is informed by the vision of a constructive alternative to it. It is from within the perspective of this alternative that the sources of difficulty in traditional practice are diagnosed. His objections to inductivist methodology presuppose a conception of deductive modes of practice that he associates with a properly scientific testing methodology and with an ambitious specification of the explanatory goals appropriate to anthropological archaeology. And his criticism of normative theories of culture is implicitly comparative; they are less plausible in general, and less fruitful in archaeological application, than the comprehensively materialist and systemic alternative he favors. Conceptually the ecosystem theory Binford proposes as an alternative to normative theories of culture is the point of departure, indeed the linchpin, for his articulation of the main constructive tenets of his new archaeology. Only given prior commitment to a systemic—specifically, an ecosystemic—conception of culture could Binford specify the explanatory goals of the discipline as he does and insist on the viability of a scientific (deductive) testing methodology for archaeological pursuit of those goals. I consider each component of the program in turn: the underlying theory of culture, its explanatory goals, and the associated testing methodology. In connection with the latter two components I focus on ways in which appeals to external, philosophical models of scientific practice have introduced tensions that would, in the end, seriously compromise the program's integrity.

THE ECOSYSTEMIC MODEL OF CULTURE

The central lesson Binford draws from his critique of normative theories is that cultures must be recognized as complex if the variability evident in archaeological material is to be explained; everything we know about "the structure and functional characteristics of cultural systems" (1962: 218) calls into question any simple reduction of such variability to mentalistic norms and conventions. These critical arguments anticipate the main components of Binford's constructive thesis, the claim that cultures are best conceived in systemic and (eco-)materialist terms. Less cryptically, Binford argues that cultures should be conceived as systems composed of a number of closely interrelated, mutually conditioning "operational subsystems"—they integrate a number of highly diverse components—whose form and dynamics are functionally determined by the exigencies of adaptation to their material (ecological) environment.[20]

In early publications (e.g., 1962) Binford's point of departure was the observation that as ordinary experience demonstrates, material culture may function in and be shaped by a number of contexts or dimensions of cultural life that can endow a given element of material culture with several very different cultural "meanings"; this insight is best captured by his famous distinction between "technomic," "sociotechnic," and "ideotechnic" contexts (1962). The central failing of the model of culture and cultural dynamics invoked by traditional theorists, a failing that Binford identifies most clearly in his critique of Mousterian interpretations, is that it involves a simple, ethnocentric projection of contemporary cultural experience onto the past; it is "rooted in the main on causal or 'obvious' features of the contemporary human experience: 'Frenchmen have different things than Japanese'" (1972b: 288). At the same time, it denies key features of contemporary cultural life, distancing the contemporary "us" from the prehistoric "them"; in important respects prehistoric agents, the people "behind the artifacts" (to paraphrase Flannery 1967), are treated as pas-

sive bearers of cultural, ethnic traditions. Binford rejects both elements of this conception of Paleolithic culture. Regarding assumptions about agency, he argues that early modern humans of the Paleolithic may have been more like us than the traditional model allows. The complexity of their material remains suggests that they were not passive receptacles of tradition but were capable of drawing selectively on an inventory of traditional wisdom or norms; like contemporary people, perhaps they treated "transmitted knowledge and belief [as] . . . a reservoir of accumulated knowledge to be used differently when appropriate" (1972b: 259). But unlike contemporary cultural agents, they seem not to have "passed a threshold, a 'cultural rubicon'" that would make their behavior reflect a partitioning into "culturally maintained distinctive populations, ethnic groups" (290).

It is ironic, given Binford's categorical rejection of analogical reasoning of all kinds, that the basis for his argument is a systematic reworking of the analogy between contemporary and Paleolithic humans that underpins normative theories of culture. Far from arguing that no analogy can be presumed, he urges that it be realigned: similarities can reasonably be assumed in individuals' general capacity for rational (means-end) action, but differences must be recognized in cultural traditions. Moreover, his grounds for preferring *his* analogical construction and the ecosystemic theory it presupposes are not empirical. Rather, he cites its greater plausibility and its promise in opening up new lines of inquiry, pointing to its explanatory power (relative to that of normative theory) and its capacity for "generat[ing] fruitful explanatory hypotheses" (1965: 213).[21] These arguments may be compelling, but they are unavoidably ampliative.

To fill in this skeletal characterization, Binford, following White's and Steward's materialist theories, proposes that culture is properly conceived in *eco*systemic terms as the "extrasomatic means of adaptation for the human organism" (1962: 218); it is a "material-based organization of behavior," not a "mental phenomenon" (1972a: 9). This core definition directly counters the first two principles of normative theory, which specify the reductively ideational nature of culture. The associated model of cultural dynamics (the third principle cited above) is likewise rejected in favor of the thesis that continuity and change in cultural systems are a consequence not of the internal dynamics of belief transmission but of adaptive responses to the conditioning environment in which these systems operate; "changes in the ecological setting of any given system are the prime causative situations activating processes of cultural change" (L. Binford 1964: 439). It follows from this ecological formulation of the systemic model that, contra the fourth and fifth principles of normative theory, formal similarities in archaeological assemblages cannot be treated as a measure of cultural affinity, nor can we expect variability in the record to converge in spatial and temporal distribution as if it were a manifestation of coherent and distinct cultural traditions.

Here again the primary reasons Binford gives for adopting a systemic conception of culture that privileges the ecological dimensions are not its substantial independent empirical support, though its endorsement by Steward and White is taken as evidence of its credibility. He emphasizes instead its potential fruitfulness when applied to archaeological problems; while such explanatory success provides indirect empirical support, more often the case Binford makes is pragmatic. In two key reinterpretations of problems that had resisted solution in traditional (normative) terms, Binford applies the ecosystem model to good effect; in the first—the Old Copper Complex case discussed in "Archaeology as Anthropology" (1962) —he draws on its resources as a systemic theory, and in his later treatment of the Mousterian case he exploits its potential as a source of ecological interpretation (1972b).

Binford's interpretation of the Mousterian case is that the intra-assemblage variability Bordes could not explain reflects not distinct ethnic groups but diversity in the subsistence strategies adopted by prehistoric communities that were, for the most part, ethnically undifferentiated; their activities at different sites varied with the opportunities offered by their environment, not as an expression of cultural identity. In the earlier reanalysis of normative theories about the devolution of the Old Copper Complex (1962), Binford argues that the decline in production of copper tools, which was counterintuitive on normative principles, could well be explained by recognizing that these tools functioned in a number of contexts other than the strictly utilitarian or technical. He suggests, specifically, that if they are regarded

as "sociotechnic" items marking status, then the decline in their production might be explained by complex changes in the organization of society—in particular, its development toward nonegalitarian forms—and their emerging role as elite status markers. Although this hypothesis is presented as very preliminary, it is sufficiently promising, Binford maintains, to demonstrate the explanatory potential of systemic theory: "only with a systemic frame of reference could such an inclusive explanation be offered" (1962: 224). The Mousterian case demonstrates the explanatory potential of a specifically *eco*systemic explanatory theory.

In addition to these case-specific arguments for the explanatory power of ecosystemic as opposed to normative models, Binford gives two more general pragmatic and methodological reasons for embracing his preferred alternative. The first is, again, a comparative argument in which the ecosystem approach is said to significantly broaden the scope of archaeological inquiry. Binford objects that in failing to provide explanatorily powerful (or empirically adequate) models of the cultural conditions responsible for the record, normative theories also severely limit the kinds of questions that can be raised about the cultural past. Those who adopt a strict normative (or idealist) approach treat cultural traditions as if they were self-generating; they assume not only that all the variability encountered in the archaeological record can be explained by reconstructing the "transmitted ideas and knowledge" and the "patterns of information flow" (1971: 25) that constitute the ideational dimension of a culture, but that no further explanation is required to account for the presence and transmission or diffusion of these ideas. The normativist thus "ignores the possibility that there are processes selectively operating on a body of ideas or knowledge" (25), foreclosing inquiry into nonideational conditions that shape the content, diffusion, and transmission of the cultural norms themselves or that may directly affect the production of material culture (and of an archaeological record) independent of these norms.[22] The ecosystem paradigm is to be preferred not only because it suggests a wider range of conditions and processes that might be responsible for the record than those cited by normative theorists (thereby enhancing explanatory power) but also because it opens up a further, distinctively anthropological level of inquiry,

foregrounding questions about the conditions and processes responsible for the specific forms of cultural life—the cultural norms, behaviors, and events—that produced the archaeological record.[23]

The final case Binford makes for materialist, ecosystem alternatives again concerns their relative fruitfulness, this time at an epistemological level. When culture, on a strict normative theory (e.g., that advocated in Taylor 1967 [1948]), is viewed as entirely distinct from its tangible objectifications, the generative processes and causal connections presumed responsible for material culture and the archaeological record are, by definition, unanalyzable; they link ontologically distinct categories of phenomena. Consistently maintained, this theory rules out the possibility of reconstructing culture per se by any means but speculative projection of our cultural experience (the sorts of norms and conventions that inform our behavior) onto the past. Taylor acknowledges this feature of normative theory in making a virtue of the fact that, on a normative theory, ethnographers and ethnologists are on no firmer footing than archaeologists; insofar as archaeologists concern themselves with culture proper, they too must engage in inferential reconstruction of the norms and ideas that are objectified in the observable behavior of their subjects. While this line of argument may ensure that archaeology is at no special disadvantage in studying cultural phenomena, by no means does it provide grounds for optimism. In fact, as Binford points out, many saw in these leveling arguments reason for varying degrees of skepticism; while inference to cultural antecedents is always uncertain, its reliability "varies directly with the degree to which the subject is removed from discussions of artifacts themselves" (1968a: 21). This intuition is explicit in the metaphor of a "ladder of inference" introduced by British archaeologists C. F. C. Hawkes (1954: 161–162) and Piggott (1959: 7–12) in the decade before Binford's call to action.

Hawkes and Piggott specify a hierarchy of levels of reconstructive security that begins with inferences concerning the technologies necessary to produce artifacts; at the next level of security are inferences concerning those aspects of cultural life that are most directly shaped by the material conditions (subsistence practices); there follow increasingly insecure forms of inference

about social organization, ranging, again, from those aspects of social life that are most directly shaped by ecological, material, or technological constraints to those that reflect the contingencies of cultural tradition. The most tenuous forms of inference, at the furthest remove from the supporting evidence, concern the "ideational," symbolic dimensions of cultural life. The paradox in such a scheme is clear, though Binford does not explicitly point it out. It establishes reasons for particularly mistrusting inferences about the normative (mentalistic) aspects of cultural systems—especially when these are understood to be independent (cultural) variables, constraining of but unconstrained by the material conditions of life that Hawkes and Piggott assume to be most directly and reliably accessible to archaeologists. Here the ground is cut from under the enterprise of traditional archaeology; on the "ladder of inference" model, the cultural subject is conceived in precisely the terms that render it most inaccessible to archaeological inference.

The strongest claim Binford makes for the fruitfulness of his alternative theory of culture is, then, that *it alone sustains the possibility of inquiry into the cultural past;* it postulates a cultural subject that is archaeologically knowable. This argument is presupposed by Binford's earliest programmatic statements in which he insists that "data relevant to most, if not all, the components of past sociocultural systems are preserved in the archaeological record" (1962: 218–219). If culture is conceived as a complex system in which each element interacts with and is responsive to all others, then cultural norms are firmly reconnected with the behavioral, material, and organizational dimensions of cultural life. More to the point, material culture can be expected to bear the marks of its implication in all the constituent subsystems of cultural life; it is one mutually conditioning component of cultural life among others, not an objectification of underlying (autonomous and self-moving) cultural givens.

Binford makes even stronger claims for epistemic optimism when he later shifts the emphasis from a systemic to an *eco*systemic model. On an ecosystem account, the dynamics of adaptive response to ecological conditions are understood to be the primary determinant of cultural systems, responsible for their overall form and developmental trajectory. In its most reductive form,

which Binford defends in later polemical response to his critics,[24] this theory implies that all particulars of cultural life, not only its tangibly material dimensions (its technology and subsistence practices) but also its mentalistic aspects, are to be explained functionally, in terms of the role they play in supporting the adaptive fitness of the system as a whole. Although it represents a significant compromise of his original systemic model—in many respects it simply inverts the constraints of normative theory that Binford had so vehemently criticized in the early 1960s[25]—a strict ecosystem model has the virtue that the material, ecological factors and processes to which it attributes primary causal significance can be assumed to be law-governed and therefore eminently reconstructable in terms consistent with his scientific ideals. When Binford responds to the "paradigmatic bias" of "posturers" (1989: 4; 1982b: 125, 134) who insist that internal (mentalistic and ethnographic) variables play a substantial, independent role in shaping cultural systems, he makes explicit the pragmatic grounds for embracing a reductive ecosystemic paradigm. Their models, like the normative theory of culture associated with traditional archaeology, accord a central causal and explanatory role to radically contingent factors—beliefs and ideals that result from the evolution of cultural tradition or the exercise of human agency—factors that are not law-governed and cannot be reliably (scientifically) reconstructed from empirical evidence of the material conditions and consequences of cultural life. Ultimately Binford rejects all such alternatives because they cannot sustain a scientifically respectable program of archaeological research; on his account, a key to breaking the grip of the skepticism associated with traditional archaeology is to embrace a thoroughly materialist and systemic conception of the cultural subject.

EXPLANATORY GOALS

With an epistemological argument against the sequent stage strategies of traditional research and an alternative to the associated normative conception of culture in hand, Binford then sets out the scientific, anthropological goals of the discipline around which all aspects of the New Archaeology should be integrated. The central and defining goal of the New Archaeology is to move decisively

beyond the descriptive, historical modes of practice associated with traditional archaeology and to take up distinctively *explanatory* problems. What distinguishes Binford's formulation of this principle, which is by no means unique to him, is his further specification of what explanatory understanding requires. To be properly scientific, he argues, drawing on logical positivist models developed by Hempel,[26] archaeological explanation must be law-governed and, unlike the alleged inductivism of traditional research, it must be *deductive* in logical structure. Binford takes those requirements to mean that archaeologists must set their sights on understanding "the total range of physical and cultural similarities and differences characteristic of the entire spatial-temporal span of man's existence" (1962: 218); they must move beyond investigation of the particular and focus on questions about generalizable, prospectively law-governed (structural and processual) features of cultural systems and their adaptive responses to their environments. As Flannery put it, in a passage alluded to earlier, "the process theorist is not ultimately concerned with 'the Indian behind the artifact' but rather with the system behind both the Indian and the artifact" (1967: 120).

These proposals have obvious programmatic appeal, particularly when viewed against the background of sequent stage approaches in which explanatory problems are identified as the (exclusive) domain of the final stages of inquiry, sharply differentiated from the descriptive concerns of historical and ethnographic investigations (as, for example, in Sabloff and Willey 1967). They amount to a decisive choice in favor of the option Kluckhohn had described as a "scientific attack," with its focus on large-scale anthropological questions about "trends toward uniformity in the responses of human beings to types of conditions," and against (descriptive) historical analysis dedicated to the delineation of antecedent events and conditions "in all their particularity" (1940: 41). But on closer examination, the details of Binford's account of the explanatory goals that a scientific archaeology is to serve reveal a number of important ambiguities.[27]

For one thing, the sharp distinction between descriptive and explanatory modes of inquiry breaks down when pressed. In many contexts Binford's critique of traditional archaeology suggests that culture-historical reconstructions, and even

systematizing schemes, are in fact forms of explanation; their failings are those of explanations that are arbitrarily limited in scope or that lack adequate theoretical underpinning. For example, when Binford makes the case for giving immediate priority to processual questions, he objects that the culture-historical reconstructions that Sabloff and Willey propose are really low-level (first-order) explanations that presuppose processual understanding. They consist of a set of hypotheses about the specific conditions and events responsible for the material found in the archaeological record that depend, covertly or overtly, on a general, lawlike understanding of the processes that link material culture of this kind to other aspects of the cultural systems that produce it. John Fritz and Plog (1970) extend this point to typological schemes, arguing that the key classificatory concepts used in descriptive systematizations of archaeological material (especially those that designate cultural units or functional classes of artifacts) encapsulate complex, largely unsubstantiated explanatory arguments; they depend on general (nomological) causal beliefs about the conditions responsible for the formal attributes of artifacts that stand in for the laws that, on a Hempelian model, underwrite explanatory claims (1970: 407). Although system-level explanation is the ultimate goal of processual inquiry, on Binford's and Fritz and Plog's analysis even the most modest reconstructive and interpretive claims depend implicitly on processual knowledge and should conform, in structure, to the covering law model. When the laws in question are left implicit or unsubstantiated, archaeologists deal in what Hempel (1942) calls explanation sketches.

Perhaps most telling, when Binford indicates more specifically what it is that distinguishes genuine processual explanation from description (or from low-level culture-historical explanation of the contents of the archaeological record), he often invokes a difference in descriptive content rather than the distinctive logical features of the Hempelian model. In criticizing Sabloff and Willey, for example, he insists that it is not enough simply to identify the conditions that preceded the cultural events requiring explanation (e.g., the collapse or transformation of a cultural system); indeed, at some junctures he makes it clear that it is not enough to cite a pattern of co-occurrence between the types of antecedent condition and outcome in

question. On its own, he suggests, describing patterns of succession or correlation does not explain *how* and *why* the cited effects were produced by the postulated cause, the primary objective of a processual explanation. Such explanation requires, in addition, an account of the underlying causal relations, the generative mechanisms or processes, that link co-occurrent phenomena—an account that is itself descriptive. There is, then, considerable artificiality in the sharp distinction between explanatory and descriptive goals, and in the parallel distinction between scientific and historical levels of inquiry, that Binford takes over from the sequent stage schemes he rejects.

This last ambiguity about the relationship between explanatory and descriptive accounts is particularly important because it reveals an underlying tension in Binford's conception of scientific goals: it reflects the fact that he draws on two quite distinct models of explanation. When he characterizes his own position as involving "a shift to a consciously deductive philosophy" (1968a: 18), he appeals to Hempel's covering law model of explanation—specifically, its deductive-nomological variant—according to which the force of an explanation derives from its demonstration that the phenomenon to be explained is an instance of an established lawlike regularity that is presumed to be universal and invariant (nomological) for such phenomena. In other contexts, however, he draws on what he characterizes as a modeling conception of explanation: "At the time I wrote 'Archaeology as Anthropology' (1962), I had not explored the implications of the epistemological problems associated with the task of explanation. At that time, explanation was intuitively conceived as building models for the functioning of material items of past systems" (1972a: 17). In fact, the covering law model is clearly present in "Archaeology as Anthropology," while the alternative modeling conception emerges most explicitly in Binford's later discussions of explanatory goals, introducing conceptual tensions that have serious consequences for the practical viability of Binford's program.

When Binford first specifies what explanation in a "scientific frame of reference" requires, he argues that it is "simply the demonstration of a constant articulation of variables within the system" such that, in an archaeological context, "processual change in one variable can be shown to relate in a predictable and quantifiable way to change in other variables" (1962: 217). In short, consistent with Hempel's model, explanation is accomplished when an observed event (e.g., an event described in terms of the value, or change in value, of a particular variable) is shown to fit an established empirical regularity covering such events, such that it could have been expected to occur as it did (thereby meeting the requirement that explanations should establish grounds for rational expectation). Sabloff and Willey failed to explain the Mayan collapse because, on Binford's diagnosis, they did not establish a causal connection between the collapse and the invasion that they cite as its cause; they fail to provide "a set of general laws which connects the 'causes' with their 'effects' in such a way that if we knew that the earlier events have taken place, we would be able to predict the event we wish to explain" (1968d: 268). Binford refers to Hempel in this connection: "The assertion that a set of events . . . have caused the event to be explained, amounts to the statement that, according to certain general laws, a set of events of the kinds mentioned is regularly accompanied by an event of the kind [for which explanation is sought]" (Hempel 1965: 232, quoted by L. Binford 1968d: 267–268). What Binford fails to recognize, although it is clearly stated in the passage he cites, is that Hempel's formal model of explanation presupposes an explicitly reductive regularity theory of causality, according to which causal connections are *no more than* empirically established constant conjunctions among observables. This is a classically empiricist (Humean) treatment of causality. It follows directly from the injunction to avoid speculation about unobservables, including not only claims about first and final causes (the primary target of Comte's positivist critique) but also explanatory appeals to underlying generative (causal) mechanisms and processes, or to a natural necessity of connection between observable events and entities that are consistently associated (the conception of causality challenged by Hume).

By contrast with this stark Hempelian positivism, Binford routinely insists that processual explanations must be based on what amounts to a nonreductive, implicitly realist understanding of causal connections. These should reflect "our current knowledge of the structural and functional characteristics of cultural systems"; to be com-

pelling they must invoke "laws of cultural or behavioral functioning" (1962: 218) that provide an understanding of the "conditions and mechanisms *by which cultural changes are brought about*" (1964: 425; emphasis added). When Binford considers the specifics of the Mayan collapse, he suggests that Sabloff and Willey should have provided a fuller account of the nature of the interacting agents or entities and should have described those features of their interaction "which might have been crucial to *bringing about* the collapse of the Classic Lowland Maya" (1968d: 268; emphasis added). An appeal to invariant patterns of conjunction between invasion and collapse—the basis for explanatory understanding on the simple deductive form of the covering law model invoked by Binford—could not satisfy this requirement for an account of constitutive factors and causal processes. To show *how* the effects are brought about is to go beyond the demonstration that they fit a pattern of occurrences and therefore could have been expected: it is to explain the pattern itself. The irony is that if Binford were able to implement a program of archaeological research that conformed strictly to the requirements of Hempel's covering law model, he would revert to precisely the kind of empirical description of observables—in this case, observable regularities—that he and New Archaeologists generally were most intent on transcending. More of this shortly.

TESTING METHODOLOGIES

In retrospective discussions Binford identifies his attack on "unscientific conventionalist strategies of interpretation" (1972a: 330) as the defining feature of his new perspective. On his analysis, the subjectivism and conventionalism endorsed by Thompson were unavoidable; traditional archaeologists had no option but to accept interpretive conclusions on the basis of faith and convention because they lacked "any rigorous means of testing, and thereby gaining confidence in, propositions about the past" (1968a: 16). This immanent skepticism could be avoided altogether, however, if archaeological data were used as a body of evidence against which interpretive conclusions might be systematically tested rather than as a basis for inductive inference. Despite invoking Kuhn and embracing the central contextualist arguments of radical critics like Kluckhohn,

Binford clearly shared the view of such comparatively conservative critics as Strong who retained a strong faith in the robustness and autonomy of archaeological evidence. His programmatic vision depended fundamentally on the conviction that the archaeological record is capable of imposing significant empirical constraints on claims about the past if approached for the purpose of testing them, rather than with the expectation that it will eventually yield a comprehensive picture of the past inductively, as knowledge of its contents accumulates. Thus, he recommended that the New Archaeologists must invert the traditional relationship between hypotheses and evidence: "The generation of inferences regarding the past should not be the end-product of the archaeologist's work[;] . . . independent means of testing propositions about the past must be developed. Such means must be considerably more rigorous than evaluating an author's presuppositions by judging his professional competence or intellectual honesty" (1968a: 17).

To give these general recommendations more specific content, Binford again turned to Hempelian positivism. He invoked Hempel's "hypothetico-deductive" model of scientific confirmation, characterizing this "final link in scientific inquiry" (1968a: 14)—the reconnection of interpretive hypotheses with archaeological evidence through systematic testing—as a deductive alternative to the insecure inductive practices he attributed to traditional archaeology. The general outlines are reminiscent of integrationist arguments that have surfaced repeatedly since Dixon (1913) and Wissler (1917) first canvassed the alternatives to neo-antiquarian approaches. Binford recommends that archaeologists proceed by first deriving archaeological test implications from the interpretive and explanatory hypotheses they mean to test, then designing a strategy for data recovery or analysis that will establish whether or not these implications are borne out by archaeological data. Such an approach is appropriate to a scientific, anthropological archaeology because it avoids any reliance on intuitive, subjective, and conventional judgments of plausibility: "the accuracy of our knowledge of the past can be measured[,]. . . . [and] the yardstick of measurement is the degree to which propositions about the past can be confirmed or refuted through hypothesis testing . . . [against] independent empirical data"

(L. Binford 1968a: 17). The process of inference by which hypotheses are formulated is firmly relegated to the context of discovery; questions about credibility are to be addressed exclusively in terms of the (deductive) logic of the context of verification. The integrated testing methodology envisioned by Krieger is thus extended to all levels of inquiry, not just those associated with the construction of typological schemes.

When the philosophical model lying behind these recommendations is considered, however, the manner of its application to archaeology is not at all clear. Hempel's hypothetico-deductive model of confirmation provides an account of the formal relations that hold between evidence and a lawlike generalization of the sort that, once established, could figure as the major premise in a covering law explanation. When a putative law of this sort makes a universal or a statistical claim about patterns of conjunction that hold between categories of phenomena—for example, that all entities describable as swans are white, or that metal of a particular sort always breaks when subject to a specified level and kind of stress, to cite some standard examples—hypothetico-deductive confirmation requires a procedure of checking, empirically, to determine that particular examples of the phenomena included in the domain covered by the law (swans or stressed metal) conform to its expectations (about color or breaking points). By (methodological) extension of these principles, testing is a matter of deriving test implications from a hypothetical law concerning its instantiations—the particular instances of the phenomena the law is meant to cover (swans, specified metals)—and then checking its empirical adequacy by inspecting these cases. For example, if Sabloff and Willey had undertaken to explain the Mayan collapse by showing that it conformed to, and could be subsumed under, a general law specifying that invasions of the sort in question are always followed by collapse, the credibility of their explanation would depend not only on showing that invasion did, indeed, precede the Mayan collapse but, most important, on establishing the law itself. In this case, testing might proceed by checking the implication that all events properly described as an invasion of a specified kind are followed by cultural collapse.

Two difficulties are immediately evident that were identified, in the mid-1970s, by Merrilee Salmon (1975, 1976) and by internal critics such as Sabloff, Beale, and Kurland (1973).[28] The first is a problem inherent in the hypothetico-deductive model that has been much commented on in philosophical contexts: unless the postulated law is limited in scope—unless it covers a finite set of cases—it can never be *deductively* confirmed. The ideal of hypothetico-*deductive* confirmation can be achieved only if it is possible to inspect all instances in the domain covered by the law and show that they fit the general, systematizing claims it makes about them. In this case, the conjunction of the descriptions of all subsumed cases does entail the test hypothesis, precisely because it contains no more information than that supplied by premises that describe all its instances. In cases in which laws are universal or the test evidence for other reasons represents only a subset of all instances that constitute the domain covered by the laws in question—the latter being the usual case in hypothesis testing—the relationship between hypothesis and confirming evidence is inductive, in the sense that the hypothesis makes claims that go beyond (it amplifies on) the information about all available instantiating cases that could be cited in its support. In short, testing procedures conforming to the "hypothetico-deductive" model are rarely deductive unless they concern closely circumscribed (usually relatively trivial) test hypotheses, although the model does capture a pattern of reasoning about the import of test evidence that is widely held to provide hypotheses some degree of inductive support.[29]

The second difficulty is that this model seems largely inapplicable to the sort of archaeological testing Binford advocates. He is, after all, specifically concerned that archaeologists not remain at the level of systematizing the observable contents of the record; and yet it is generalizations about observables to which the model most obviously applies. For example, the procedure of testing typological schemes recommended by Krieger fits a hypothetico-deductive model unproblematically; when typological concepts embody claims about patterns in the association of attributes in specific sorts of archaeological assemblage (often defined by spatiotemporal context), they are most obviously tested by drawing out test implications for unexamined assemblages and determining whether the expected patterns of association hold in them as well as in the assemblages on

which the schemes were originally based. But Binford takes the primary object of archaeological testing (in a scientific, anthropological program of research) to be processual and historical hypotheses about the particular past conditions and events that produced the contents of the archaeological record and, ultimately, the system-level dynamics that explain these particular antecedents. The design of an archaeological test for hypotheses of these kinds is essentially particularistic. First tested is the hypothesis that particular conditions obtained in the past (whether locally or at a system level). That hypothesis, if confirmed, may provide support for (or, if disconfirmed, may falsify) a general lawlike proposition concerning the regularities governing cultural phenomena of the sort instantiated by the archaeological case.

To use archaeological evidence as the basis for testing prospective laws of cultural dynamics, the processual archaeologist must use an extensive body of lower-level lawlike propositions to establish whether or not a given assemblage of archaeological data supports or refutes the expectations of a test hypothesis about cultural systems (or about more localized cultural conditions). The hypothetico-deductive model is most directly applied to the testing not of explanatory hypotheses about the cultural past per se but of the interpretive principles (qua hypothetical laws) that establish the significance, or meaning (as Binford often puts it), of archaeological data as evidence of antecedent cultural conditions and events. In the latter case the object of testing is a hypothesis that postulates a reliable (ideally, invariant and universal) association between a particular type of archaeological trace and specific antecedent conditions or events; testing proceeds by checking to see whether the material and behavioral variables linked by such a hypothesis are, in fact, instantiated in the range of contexts in which it is possible to inspect both elements of the conditional. In principle, if it were possible to conclusively confirm linking hypotheses of this kind, they could serve as the major premise in a deductive argument to the effect that, given a particular kind of archaeological record, specific events or conditions *must* have occurred or obtained in the cultural past; the form of such retrodictive arguments is structurally symmetrical to that of a covering law explanation. The difficulty remains, however, that even if it were possible to establish laws of

this sort for all contemporary or ethnohistoric contexts, their projective application to past contexts for which only the material elements are accessible remains inductive (qua ampliative). The first difficulty with hypothetico-deductive confirmation reasserts itself, now at the level of testing the interpretive principles that underwrite the use of archaeological data to test hypotheses about the cultural past.

Given the difficulties inherent in archaeological applications of the hypothetico-deductive model, it is not surprising that as Binford elaborated his original programmatic recommendations, he emphasized, with increasing urgency, the need for an expansive program of actualistic research capable of securing the linking hypotheses presupposed by the interpretive arguments that establish the evidential import of test evidence. He observes, in this connection, that because "explanation begins for the archaeologist when observations made on the archaeological record are linked through laws of cultural or behavioral functioning to past conditions or events," it is essential that "hypotheses about cause and effect . . . be explicitly formulated and tested" (1968d: 269–270). Nonetheless, explanatory, reconstructive hypotheses about the cultural past formulated at various levels of generality—as hypotheses about localized cultural conditions and historical events, or about the structure and dynamics of cultural systems as a whole—are the primary focus of his arguments for deductive testing procedures; it is these that require systematic evaluation against the surviving material record of this past if the impasse created by the inductivism of traditional archaeology is to be avoided. When Binford considers archaeological testing of this sort, the confirmation procedure he envisions is typically a matter of empirically evaluating explanatory *models* in the sense associated with his modeling conception of explanation. He argues that "the archaeologist should be continuously engaged in the development of 'models' of the past, specifying the conditions which, if true, would accommodate our observations in the present" (1972a: 334). His analyses of concrete examples all exemplify this approach to testing, bearing very little resemblance to the type of testing strategy suggested by a hypothetico-deductive model of confirmation.

Consider, for example, Binford's treatment of

what he takes to be a classic case of the skepticism entailed by traditional archaeology: Allchin's concern (1966) that a physical gap in the distribution of a distinctive artistic tradition across southern and Saharan Africa might be a consequence of differential conditions of preservation rather than of two distinct but strikingly similar cultural traditions evolving independently. Allchin suggests that a common artistic tradition obtained throughout the region, but between the areas where it has been documented archaeologically more perishable materials were used. Given that hidden or missing data are always possible, she objects that it is virtually impossible to prove or disprove either candidate hypothesis; generalizing this worry, she concludes that it is impossible to conclusively prove or disprove *any* given hypothesis about the past.[30] Binford counters this skeptical conclusion with the argument that Allchin's difficulty arises only because she treats claims about the past as the conclusions of inductive inference from an existing body of data and presumes, as a matter of interpretive convention (a corollary to the fifth principle of the normative model described above), that "an interrupted distribution signifies a cultural boundary and independence for the two traditions represented" (1968a: 18). If she were to formulate these claims as interpretive hypotheses rather than conclusions, and in this spirit undertook to evaluate them by seeking evidence of a break in continuity in other dimensions of the archaeological record—for example, in "the stylistic attributes of other items . . . bead forms, decoration on bone implements, projectile point forms, etc." (19)—then the grounds for accepting the hypothesis would not be limited to the evidence and interpretive assumptions that gave rise to it. The methodological principle at work here is that the archaeological record would serve as a resource not for establishing further instantiations of a descriptive hypothesis per se but for providing evidence that should exist, or could (only) exist, if the model of independent cultural systems was in fact accurate and the artistic traditions in question evolved in distinct cultural contexts.

Although Binford's testing methodology counters "Allchin's dilemma" by proposing deductive testing practices as a promising alternative to the inductivism of traditional practice, contrary to his claims on their behalf these are manifestly nondeductive. Clear-cut archaeological test implications are rarely, in any strict sense, deductively entailed by the complex sorts of causal hypotheses that Binford would have archaeologists test, and the archaeological evidence used to test such ampliative hypotheses cannot establish that they were instantiated in the cultural past without interpretive, and typically ampliative, reconstruction of the conditions (cultural and natural) responsible for the production and preservation of this evidence.[31] Binford's response to the Allchin case illustrates the point made in general terms above: the use of archaeological data as test evidence depends on a wide range of auxiliary hypotheses concerning the archaeological implications of cultural discontinuity in different dimensions, most of which will not be constituents of the hypothesis being tested.[32] It is always possible that one or another of these auxiliaries is false or inapplicable to the case in question, and under these conditions the test implications may not be borne out even if the hypothesis is correct; alternatively, they may be falsely confirmed. With a full suite of incontrovertible and biconditional auxiliaries (the ideal core of "middle-range" theory as Binford conceives it), archaeological testing might approximate deductive security. But in virtually all cases in which ambitious explanatory (processual) hypotheses are the object of archaeological testing, the inference required to bring evidence to bear on them will be structurally inductive (qua ampliative); however compelling it may be, the outcome of testing will remain to some degree insecure.

In short, when the specifics of Binford's constructive program are considered in any detail, his sharp dissociation of traditional inductive methodologies from new deductive procedures, like his opposition of descriptive to explanatory goals, proves to be an unsustainable gloss on his central methodological insights. To characterize his alternative strategy of inquiry as deductive is to obscure many of its most important and subtle features; as a form of research practice it is almost always inductive (in the broad sense that it relies on ampliative inference), but it is, nonetheless, systematically and rigorously empirical. In many respects Binford's insights about the potential of a research program of model building and testing are compelling. He does make a strong case for proceeding on the assumption that in many contexts, the archaeological record can support a highly rigorous (if not deductive) empirical test-

ing program if archaeologists formulate their interpretive conclusions as explanatory models of particular past cultural conditions and undertake an active program of actualistic research designed to establish the linking hypotheses necessary to bring archaeological data to bear on these models.[33] But Binford's appeal to Hempelian models of confirmation and explanation not only adds little of substance to these proposals, it obscures precisely the considerations that make them attractive; in particular, it is incompatible with his growing recognition of the complex role that background knowledge (auxiliaries and linking hypotheses) plays in establishing comparative claims about degrees of confirmation.

As I indicated in the introduction, the demise of positivism (Suppe 1977b) was well under way in philosophical contexts by the time Binford invoked it as an authoritative model of scientific practice in archaeology. By the late 1960s contextualist critics had decisively challenged the empiricist presuppositions of logical positivist theories of science; critics like Kuhn, who was also widely influential among New Archaeologists, added external challenges (from the history of science and the psychology of perception) to the internal critiques that had already begun to undermine assumptions about the foundational status of empirical (observational) evidence. As a late exponent of logical positivism, Hempel was then at the center of intense debate about the viability of his deductivist models of explanation and confirmation and was in the process of modifying his original position in many respects (e.g., adding inductive and statistical variants to his covering law model of explanation, and exploring the puzzles generated by a hypothetico-deductive model of confirmation).

What the philosophical critics of logical positivism drew attention to, and what Hempel himself grappled with, were the implications of recognizing the constructed, interpreted (auxiliary-dependent) nature of evidential claims, as well as the uncertainty of the inferences by which they are brought to bear on a specific test hypothesis; these are precisely the jointly methodological and epistemological issues that have been a persistent concern for archaeologists. To guard against a tendency to construct idealizations that misrepresent the nature of the scientific enterprise, philosophical postpositivists insisted that the analysis of science must be grounded in a detailed understanding of how scientists actually negotiate the uncertainties inherent in their enterprise. It was thus to be expected that the positivist models Binford invokes would be at odds with his practice-grounded insights about how archaeologists might proceed; in many respects this disjunction reproduces opposition that was then emerging between the defenders of "received view" philosophy of science and their contextualist critics. Rather than focus on the philosophical dimensions of this incongruity,[34] however, I turn in the next chapter to consider several lines of tension that emerged within the New Archaeology itself when its advocates undertook to implement Binford's positivist ideals.

Emergent Tensions in the New Archaeology

THE LEGACY OF POSITIVISM/EMPIRICISM

Archaeology was by no means alone in its struggle to redefine entrenched goals and modes of practice in the 1960s. In fact, Gibbon argues that the New Archaeologists' enthusiasm for positivist/empiricist ideals is best understood as an extension of a "concerted effort to 'harden' the social sciences" that took root across North American social science in the 1960s (1989: 139–140). He argues that this move to scientize social research, far from representing a decisive break with past practice, was a defensive reassertion of traditional naturalist ambitions[1] fueled by an anxious concern to shore up the credibility of social research. The associated emphasis on quantitative methodologies and the rhetoric of logical empiricism served to affirm the scientific maturity of these disciplines,[2] legitimating and protecting positivist research programs that had become entrenched in North American social science from the 1930s through the 1960s (Gibbon 1989: 126) and that were themselves a key component of wide-ranging attempts to defend Enlightenment ideals of civilization (e.g., Kolakowski 1968: 174–206).[3]

At the same time, the threats to which these scientizing moves were a response included internal critiques of the positivist tradition in social science, articulated with increasing urgency and in-

fluence through the 1960s and 1970s. Vocal minorities opposed, with varying degrees of success, a myopic preoccupation with the facts at the expense of theoretical development—"butterfly collecting," as Leach had described it with reference to anthropological practice (1961: 2)—and challenged underlying philosophical assumptions about the stability and neutrality of observation. Some also objected to inductivist strategies of building up systems of empirical generalizations by a process of amassing and summarizing observations; they argued, in a realist spirit, for programs of inquiry designed to get at the underlying causal or quasi-causal factors responsible for observable behavior, either at an individual or systemic level. Many roundly castigated a compulsion among traditional researchers to preserve empirical rigor above all else—"methodolatry" in new and old forms—on the grounds that it reinforced a pervasive superficiality of analysis and sharply limited the scope of inquiry. In short, North American social science of the 1960s incorporated both dominant naturalizing trends, identified by Gibbon as the broader movement of which the positivist New Archaeology was just one example, and a growing contingent of antipositivist, and sometimes explicitly humanistic, countertrends.

This turn against positivist ideals, even as they continued to be a force, is evident in Harvey's re-

versal (1973) of his own earlier endorsement of positivism in geography, as well as in a century-long pattern of vacillation between objectivist and antiobjectivist commitments in history that has been documented with particular clarity by Novick (1988). Indeed, Novick argues that professional historians in North America have moved back and forth between dilemmic options very like those that have defined the terms of archaeological debate since the turn of the century. At key junctures historians committed to ideals of objectivity have been eager to set their enterprise on a firm empirical foundation, sometimes invoking the tenets of positivism or empiricism directly; yet time and again, they confront the limitations of any program of research that requires strict fidelity to its empirical foundations.[4] Although, on Novick's account, philosophical concerns had little direct influence on historians,[5] their recurrent internal struggle with ideals of objectivity reflects the epistemological anxiety that arises from implicit empiricist commitments: if you assume that the source and content of legitimate knowledge claims must derive from (or be reducible to) observational evidence, then you cannot avoid speculation even in identifying archival material as a historical record, let alone in inferring its significance as evidence of the past. The 1960s marked a turning point in North American history, as objectivist ideals that had been reasserted with particular vigor through the 1940s and 1950s were systematically undermined—not so much by direct philosophical challenges as by the emergence of perspectively divergent programs of inquiry, including various forms of social history, labor history, women's history, and black history. It was this manifest plurality (and plasticity) of historical interpretation that gave rise to explicitly relativist and deconstructive critiques within history, as in other social sciences (see, e.g., "Objectivity in Crisis," Novick 1988: 415–629).

Although positivist and empiricist ideals were a more persistent and dominant influence in social sciences less equivocal about their naturalistic ambitions than was history (see Gulbenkian Commission 1996: 33–69), these fields were also shaped through the 1960s by internal challenges to their defining scientism. Sociological critics of the 1960s and 1970s routinely describe their discipline as having been in the grip of a positivist paradigm that began when the influence of Émile

Durkheim on such central figures as Robert K. Merton and Talcott Parsons displaced an earlier pragmatist orientation (Horowitz 1968: 198–202; Rousseas and Farganis 1965: 273).[6] In a critique that echoes archaeological challenges to empiricism, Rousseas and Farganis condemn as futile and counterproductive "the hope or belief that the end of the ideological cast of mind will permit us to view the world uncolored by value judgments": "facts are themselves the product of viewing 'reality' through theoretical preconceptions . . . which are, in turn, conditioned by the problems confronting us"; there is always "a selectivity of facts in the analysis of social problems" (1965: 273–274). To assume otherwise is "nothing but the delusion of an unsophisticated positivism" (273), whose cost is rigidity in the unexamined assumptions that do inevitably inform practice.[7] Three years later, Horowitz notes "a rising tide of discontent and self-criticism" in North American sociology (1968: 212) directed against the narrowness of extant positivist approaches, repudiating what Berger (one highly visible champion of this discontent) had earlier described as the appeal of a highly reductive "one-dimensional [logic] . . . closed in on itself" (1963: 168).

Parallel lines of argument appeared in psychology as well, where a positivist orientation was associated with behaviorism in its various forms; "extreme positivists chose to affiliate with such developments in psychology as behaviorism, association theory, and learning theory" (L. Thompson 1961: 40). The rationale for the methodological behaviorism of Skinner, for example, is quite explicitly a positivist/empiricist proscription against speculation after unobservables, which he identifies as the contents of mind, motivations and beliefs, and cognitive mechanisms (Skinner 1974: 14–17). A properly scientific psychology, on this account, focuses exclusively on correlations between observable stimuli and behavioral responses, explaining behavior in terms of cause-and-effect relationships conceived on a strict (Humean) regularity theory of causality. In critical response to this tradition of research as transposed to social psychology, Harré and Secord (a philosopher and a social psychologist) argue that the cost of embracing such empiricist presuppositions is an "overemphasis [of] fact at the expense of ideas"; behavioral scientists tended to proceed "as if observation and experiment *by them-*

selves can create a science" (1972: 36, emphasis in the original; see also MacCorquodale and Meehl 1948). This they find unacceptable for the same reasons that had impressed the New Archaeologists and their precursors, the radical critics of the late 1930s and 1940s. Only if social psychologists made it their aim to understand the underlying processes and mechanisms—here, the beliefs, intentions, and conceptual schemas of human agents—could they expect to explain observable behavior; the complexity of behavioral responses is just too great to be understood in terms of their correlations with external stimuli. In short, Harré and Secord recommend that social psychologists focus on precisely the dimensions of the social, psychological subject domain that behaviorists had set aside as an unsuitable subject for properly scientific study. Moreover, they insist that a commitment to theorize the unobservable—in this case intentional states and social conventions—is by no means a departure from mainstream scientific practice; as scientific realists, they understand such theorization to be the central objective of the most successful sciences.

The similarities between these diverse critiques of naturalist research programs in the social sciences and the antiempiricist arguments of the New Archaeology are striking. For all, the central argument against "imitative scientism" (as Radnitzky calls it, 1968a: 145) was that a preoccupation with "saving the phenomena" had enforced an implausibly reductive conception of social phenomena that both misrepresents the standards governing (real) scientific practice and ensures that social scientists could never do justice to the explanatory complexity of their subject domains, whether these were large-scale historical processes and social systems or cognitive, psychological mechanisms. And all sought ways of making effective use of observational data as evidence of the underlying conditions—structures, processes, mechanisms—responsible for the manifest forms and dynamics of social life. The difference is that the critics demanding revolution in other fields identified the traditional forms of practice they challenged as positivist—they clearly recognized the empiricist commitments that animate positivist research programs—while in archaeology the critical antiempiricist vanguard identified *itself* as positivist.

For several reasons, then, the positivism of the

New Archaeology cannot be understood simply as the delayed counterpart to a "last gasp" of conservative scientism in the social sciences, as Gibbon suggests (1989: 140). For one thing, positivist ideals continued to be influential in most social sciences despite the gathering strength of critiques of various stripes; what emerged across the social sciences in the 1960s was not a decisive rout of scientism but the articulation of an increasingly polarized opposition between naturalist research programs and a range of antiempiricist and humanistic alternatives, many of which were by no means anti-scientific.[8] And for another, what distinguished the New Archaeology as a movement for disciplinary reform in this period was that its advocates embraced key elements of the positions articulated on *both* sides of the wider debate about the scientific status of the social sciences. They promoted an explicitly positivist approach to inquiry as a strategy for overcoming the myopia of "narrow empiricism," a myopia they condemned because it had enforced an untenable preoccupation with the observables of the archaeological record. The central attraction of the New Archaeology was its paradoxical promise that if empiricist presuppositions were abandoned and a positivist testing methodology implemented, it might be possible to escape skepticism about the possibility of ever using archaeological data to understand the cultural past without resorting to arbitrary speculation. As one external commentator observed, "Possibly the radicalism or novelty of their propositions is somewhat exaggerated, but it is easy to understand the enthusiasm of pioneers carried away by new perspectives; and their efforts actually do signify an important forward stride. 'The past is knowable,' declares L. Binford . . . in a brilliant refutation of the arguments of the archaeological skeptics and agnostics of the modern English school" (Klejn 1973: 73).[9] In a similar vein, Renfrew attributed the liberating effect of the New Archaeology to the conceptual reorientation effected by its critique of empiricism: "Its chief contribution . . . is to enlarge our horizon by insisting that the basic limitation on the archaeologist in dealing primarily with artifacts does not restrict them to thinking in terms of artifacts alone. . . . [Contributors to *New Perspectives in Archaeology* show] how high we can set our sights—considerably beyond space-time and subsistence—without losing empirical validity" (1969: 243).

It is a profound irony that the commitment to conceptual analysis that inspired such transformative analyses of the empiricism implicit in traditional archaeology should also have given rise to an endorsement of positivism, a prescriptively stringent form of empiricism. By reintroducing, at the conceptual core of the New Archaeology, the very empiricist presuppositions that its advocates had so vehemently rejected, the program was compromised from the outset by a number of fundamental contradictions. These emerge both in relatively abstract debates about the defining goals of the New Archaeology—in particular, the commitment to deductivist ideals (as articulated both in terms of explanatory goals and guidelines for a problem-oriented testing methodology)— and also, with increasing clarity and urgency, in tensions between these ideals and the concrete forms of practice by which the New Archaeologists hoped to realize them. To the extent that New Archaeologists adhered to positivist ideals, they tended to revert to a variant of precisely the narrowly descriptive, essentially presentist mode of inquiry from which they had hoped to escape. And to the extent that they succeeded in making newly effective use of their data as a basis for building and evaluating ambitious hypotheses about the cultural past, their practice diverged sharply from the deductivist models of explanation and confirmation associated with such latter-day exponents of logical positivism/empiricism as Hempel.[10] Indeed, these models could not but have failed to lead New Archaeologists out of the (empiricist) impasse they identified at the core of traditional archaeology. In philosophical contexts, positivist/ empiricist theories of science had proven to be incapable of accounting for precisely those expansive aspects of successful scientific inquiry— the persistent impulse to use observables as a resource for (cautiously, systematically) extending our knowledge *beyond* the realm of observables— that the New Archaeologists most wanted to emulate in their own practice. As a rhetorical scaffolding for the New Archaeology, positivist models of explanation and confirmation obscure what is most interesting about this program epistemologically and methodologically. Not surprisingly, the New Archaeologists' endorsement of positivist ideals quickly became the target of a skeptical reaction, regenerating—in the dispute between processual and post- or antiprocessual archaeologists—the long-running dynamic of opposition between narrow empiricism and speculative constructivism that New Archaeologists had hoped to escape.

The incongruity of invoking positivism as an alternative to empiricism was by no means lost on those who resisted the skeptical impasse implicit in traditional archaeology. Sympathetic internal critics insisted that the critical self-consciousness of the New Archaeology should be applied reflexively, to its own conceptual foundations; they challenged the wisdom of invoking Hempelian models and began to articulate nonpositivist alternatives that better capture the distinctive insights of the New Archaeology.[11] In what follows I consider an extended internal debate about the explanatory goals of the New Archaeologists that erupted as soon as their first major publications appeared. The dimensions of conceptual contradiction were clearly apparent in this dispute. But more important, as successive rounds of this debate gave rise to increasingly pointed appraisals of the inadequacies of the covering law model, the realist and causalist intuitions implicit in Lewis Binford's modeling approach began to receive explicit formulation. This sustained examination of the explanatory, anthropological goals of the New Archaeology served, in turn, to reframe the question of what it means to structure archaeological research as a problem-oriented program of hypothesis testing; I discuss these methodological issues in chapter 7.

THE RELATIONSHIP BETWEEN EXPLANATORY AND ANTHROPOLOGICAL GOALS

When Renfrew and Klejn applauded the expansion of horizons associated with the New Archaeologists' "rebuttal to skepticism," they were reviewing *New Perspectives in Archaeology* (S. Binford and Binford 1968), a collection of substantive and theoretical papers that "declared the entry of a warlike cohort of young [North] Americans into the area" (Klejn 1977: 11). For this collection Sally and Lewis Binford assembled examples of the first systematic efforts to implement the programmatic goals of the New Archaeology, written by what Lewis Binford later referred to as the "second generation" of New Archaeologists. The intense debate that it generated as soon as it appeared was

already evident in a series of commentaries included in the concluding section of *New Perspectives*. Early assessments drew attention to a number of ways in which the projects represented in *New Perspectives* fell short of realizing the explanatory ambitions of the New Archaeology; they raised questions both about the goals of the program and their implementation that later became the focus of internal debate between New Archaeologists who remained loyal to the covering law model of explanation and those who advocated a systems model approach to explanation.

Several anthropologists who contributed commentaries to *New Perspectives,* in particular Lee (1968) and Aberle (1968), pointed to a disjunction between the crudeness of the background cultural theory and explanatory hypotheses that framed the projects reported by the archaeological contributors—most especially hypotheses about the social structure of prehistoric pueblo societies in the U.S. Southwest (e.g., Hill 1968; Longacre 1966, 1968)—and the sophistication of the testing methodology they brought to bear on these hypotheses. Aberle observes that if archaeologists are to be effective in using their data as a basis for testing hypotheses about social dynamics, they will have to "keep very much abreast of current theory and concepts"; many of the early projects undertaken by New Archaeologists were compromised by unsophisticated use of ethnographic sources and by reliance on theoretical assumptions about social organization that were, by the late 1960s, implausibly simplistic and "outworn" (Aberle 1968: 354).[12] The general tenor of this critique is that in their programmatic concern to demonstrate the potential of a thoroughly integrative, problem-oriented investigation of the archaeological record, the New Archaeologists who took to the field in the 1960s had failed to develop (or borrow) cultural theory of sufficient sophistication: the theoretical resources on which they draw could neither support the formulation of plausible explanatory models of the cultural processes they hoped to investigate nor carry the weight of the reconstructive inferences they relied on to interpret the archaeological data as evidence of these processes.

But even if the theoretical credentials of these early projects had been impeccable, it was by no means clear that they would have served the larger anthropological goals of the New Archaeology, at least as initially set out. Most were reconstructions of the social organization of particular past cultures, often narrowly focused on patterns of descent and residence. Aberle notes that these aspects of cultural systems "are not always the most interesting," especially given the avowed purpose of the New Archaeology (1968: 358): to establish an explanatory understanding of cultural process within the framework of an eco-materialist conception of culture. Aberle invokes a range of ethnographic cases to demonstrate that matrilineal/matrilocal patterns of the kind reconstructed by Hill and Longacre for prehistoric southwestern pueblos occur in societies that differ widely in degree of hierarchical structure, in forms of associated kinship structures, and in patterns of resource distribution. They do not necessarily indicate much else about the broader sociopolitical organization of these societies; more to the point, they may not be especially salient for understanding the dynamics of pueblo communities responsible for their history of aggregation and dispersal. Aberle argues that ranking and hierarchy, among other variables that cut across residential, descent, and kinship systems, are factors "of equal or greater importance, especially in the context of ecological and evolutionary considerations" (1968: 358). These misgivings are echoed by several archaeological contributors, including Deetz, whose pioneering work with the Arikara inspired many of the early field projects associated with the New Archaeology. He warns that it is "at least potentially dangerous [to concentrate on the aspects of descent and residence in a social system] in that it can lead to an undesirable narrowing of perspective" (1968: 45).[13] More generally,

> It is perhaps legitimate to ask why we are so concerned with the reconstruction of prehistoric social systems at all. There is always the danger of a certain method or area of inquiry becoming an end in itself. The true value of such inferences would seem to lie in the direction of the ultimate benefit to general anthropological theory; the elucidation of system and orderly process in culture, past and present. Until this type of inquiry is joined in a systematic fashion to the main body of ethnological theory, the danger is always present of such reconstructions entering the realm of ultimately sterile methodological virtuosity. (Deetz 1968: 48)

The concern Deetz articulates here is that although a commitment to reconstruct social organization may represent a break with traditional archaeology—certainly it involves a decisive move beyond description of the record itself, in the sense that archaeological data are put to work as evidence of a distinctively *cultural* past—it does not necessarily contribute to a more general explanation of the evolution and functioning of cultural systems. Deetz's critique depends on a distinction between levels of explanation that was implicit in Binford's earliest treatment of processual goals and was made explicit, a few years later, when John Fritz (1972), Fritz and Plog (1970), and Patty Jo Watson, LeBlanc, and Redman (1971) developed in more detail the argument for characterizing the explanatory goals of the New Archaeology in terms of the covering law model. On this account, the general requirements for explanation set out by Hempel—that the phenomenon requiring explanation be shown to fit the expectations of a general law—were to be applied to archaeological research at several interlocking levels.

At the most basic levels of this system of embedded explanations lie what Fritz describes as arguments of relevance that make possible "indirect observation of the past": "At the first level, arguments link attributes of the archaeological record to attributes of past events which are believed to have produced them. . . . At the second level, arguments link attributes of past phenomena that are believed to have produced them" (1972: 140). Arguments of relevance at the first level explain the content and variability of the archaeological record in terms of specific antecedent activities, events, and conditions—the efficient causes of this record; at the second level, they explain these particularities of the cultural past in terms of larger patterns of interaction that link them together as constituent elements of a cultural system. Presumably it is at the expansive end of a continuum of second-level linking arguments—where localized subsystems of attributes are themselves explained in terms of systemwide processes—that genuinely processual explanations are to be found. Deetz objected to the research reported in *New Perspectives* because, despite an orienting commitment to anthropological goals (conceived in ecosystem terms), New Archaeologists of the "second generation" had addressed themselves primarily to the task of realizing first-level and, at

most, modest second-level explanations; they were intent on testing reconstructive hypotheses about the social organization and practices responsible, for example, for patterns of association among distinctive pueblo room types and in the intrasite distribution of ceramic design elements (to take the southwestern examples). Unless broader objectives are kept firmly in view (e.g., the explanation of pueblo aggregation and collapse), the New Archaeology ran the risk of reverting to a new form of particularism—what Lewis Binford later condemned, with reference to the same examples of "second-generation" research, as a "trivial endeavor" (1977b: 4)—with no guarantee that the culture-historical events and conditions they were intent on reconstructing would prove to be relevant for understanding underlying (long-term, large-scale) cultural dynamics.

Although this point about priorities seems clear enough, there remains considerable uncertainty, prefigured by ambiguities inherent in Binford's original account, about what exactly a focus on explanatory goals entails for archaeological practice at any level, and how anthropological goals will be served by a commitment to realize an explanatory (rather than merely descriptive or reconstructive) understanding of the cultural past. To take the second problem first (I consider the first problem in the section that follows): if the objective is ultimately to test anthropological hypotheses about large-scale, long-term cultural processes in the "laboratory" of prehistory, then, on a covering law model, the enterprise must be to test general laws that specify patterns of correlation between key system-level variables. For such testing, it is critically important to determine the nature of the cultural systems and trajectories of development that obtained in prehistory, first establishing what kinds of instances prospective general laws of cultural process must be able to subsume if they are to be deemed credible. Archaeologists will be effective in addressing processual questions only if they can develop credible first- and second-order explanation at a number of levels of generality; these range from the attribution of function to particular artifacts (the narrowly focused example of first-level explanation that concerns Fritz) to inferences about various aspects of cultural life and social organization based on assemblages of artifacts and sites (localized second-level explanations of the kind that Deetz found want-

ing). Considered in this light, both first- and second-level explanations are internally complex. Together they constitute a system of interlocking microhypotheses, each of which is simultaneously an explanation and a (descriptive) reconstruction: at the first level, an explanation of how elements of the archaeological record were produced also reconstructs and describes originating events and conditions in the past; likewise, at the second level, an explanation of how these particulars relate to one another is also a descriptive hypothesis (a reconstruction) of the culture history of an archaeological subject. As I argued earlier in connection with Binford's account of explanatory goals, this implies that it is not at all obvious at what point descriptive reconstruction gives way to genuinely *processual* explanation.

Certainly, on the covering law model the distinction between reconstruction and explanation cannot be drawn in terms of logical structure. Fritz and Plog (1970), and later Fritz (1972) and Watson, LeBlanc, and Redman (1971), all embrace Hempel's symmetry principle. They hold that explanations and predictions (or, in the case of the archaeological subject, retrodictions)[14] are both arguments in which the connection between initial conditions (the antecedent or cause) and outcomes (the consequent or effect) is established by appeal to a general law, a statement of constant conjunction between these variables that is cited in the major premise of the argument. This general connection can be used (symmetrically) either to explain a particular outcome or to predict it, given evidence (cited in the minor premise) that the correlated variable obtains in the case in question. At every level, the advocates of the covering law model argue, archaeological explanations will be credible only if well-established lawlike principles can be invoked that "cover" the inference, either linking elements of the surviving record to cultural antecedents or linking those antecedents to one another and to larger cultural processes. Therefore processual explanation must be distinguished from the descriptive reconstructions of culture history not by the logic of subsumption of instances under laws, but rather by the scope of the laws that underpin second-level explanation. Reconstructive accounts make use of laws governing limited aspects of cultural systems to establish claims about localized (system-specific) events and configurations of attributes; by contrast, processual explanations account for these particulars in terms of laws that capture systemwide regularities. Note, however, that in this case the judgment of whether a second-level explanatory account serves "nontrivial" anthropological ends or reverts to particularism presupposes the theoretical (metaphysical) commitments of an ecosystem conception of cultural phenomena. It makes sense to identify genuinely anthropological explanation with the subsumption of instances under laws of cultural process only if it can be assumed that localized events and conditions are, in fact, integrated into orderly systems whose form and dynamics are a function of system-level adaptive responses to an external environment. In short, this crucial distinction is unavoidably paradigm- and problem-specific. Reconstructions of social organization might well serve anthropological goals if these were not defined in terms of a strict eco-materialism.

Further problems arise when the practical implications of reorienting archaeological practice around explanatory goals conceived on a covering law model are considered. For example, even the strongest advocates of processual archaeology acknowledge that in the main, archaeology is a *law-consuming* rather than a *law-generating* enterprise, a concession that would seem to undermine any very stringent requirement that the primary goal of archaeological research should be to establish processual laws. Although Watson, LeBlanc, and Redman staunchly advocate the view that "archaeologists are uniquely situated to formulate and test evolutionary laws about human behavior" (1971: 26)—this defines, for them, the distinctive contribution that archaeologists can make to anthropological understanding—they recognize that archaeologists are rarely in a position to test these laws. As a rule, "it is the explanation that is tested and confirmed or not" (27). At issue in archaeological testing is not the adequacy of the law invoked to establish an explanatory linkage between putative causes and effects, but whether this law is an appropriate basis for explanation "in the given case": "the laws themselves are usually neither formulated nor explicitly tested by the archaeologist" (27).[15] Watson, LeBlanc, and Redman seem to regard this limitation as merely practical. They are confident that there are determinate laws of cultural process to be discovered, which they conceptualize in Hempelian terms as statements of con-

stant conjunction. In principle the archaeological "laboratory" could provide a (deductive) test of such laws by, for example, supplying evidence of cultural forms or dynamics that clearly violate the expectations of a given law of cultural process. The limitation would seem to be that such a disconfirming instance is telling only if there can be no question that the fault lies with the law (or theory) rather than with the assumptions that underlie the reconstruction of the case. In archaeological contexts the variables that a putative law of cultural process links together (in regularities of interaction or interdependence) must all be reconstructed, so archaeological evidence for or against the law is indirect; the testing of laws of cultural process depends on a complex system of first- and (modest) second-level explanations. And despite their "strongly positive" attitude about the prospects for secure reconstruction (21), Watson, LeBlanc, and Redman seem to acknowledge that their ideal of deductive certainty remains elusive where these mediating explanations are concerned; the retrodiction of past events and conditions from their surviving material record is inevitably ampliative, unless the law that covers the inference is biconditional.[16]

The constraint on ensuring that archaeology serves anthropological goals seems, then, to lie at the level of the reconstructive inferences by which archaeological remains are linked to cultural antecedents. And here another problem arises: on a covering law model, these explanatory/reconstructive arguments are only as credible as the laws they invoke, and in practice these laws are often sketchy and ill-supported. This weakness was a second recurrent theme in early commentaries on *New Perspectives;* not only were the explanatory hypotheses tested by the "second generation" theoretically naive, but their interpretations of archaeological data as evidence depended on implausible assumptions about the cultural antecedents that could have produced specific elements of the archaeological record. Recognizing this objection, Fritz and Plog particularly emphasize the need to disembed and test the "proto-laws," the "ideas or beliefs which function as laws" (1970: 408), that underpin first-order arguments of relevance. On this analysis, the prerequisite for effective archaeological testing becomes a program of nonarchaeological (actualistic) testing designed to establish laws linking cultural antecedents to material con-

sequents in actualistic contexts where both cause and effect can be directly observed. As Hole put it, "I have come to the somewhat reluctant conclusion that the frontiers of anthropological theory are in studies of modern situations that serve to elucidate the relationships between what we may find archaeologically and the cultural or other processes that explain or . . . relate to them" (1973: 32).[17]

In the decade that followed their initial publications, a great many New Archaeologists drew the same conclusion and turned to ethnoarchaeology and experimental research (see chapters 7 and 9).[18] To cite just a few examples: Lewis Binford undertook ethnographic fieldwork in Alaska with the aim of better understanding the subsistence practices of hunters in a subarctic environment (1978, 1981b); Yellen did complementary work with the !Kung on mobility, settlement, and butchering practices (1977); Hole did ethnographic work in Luristan, Iran (1979); and Gould worked with Aboriginal groups in the Western Desert of Australia (1971). Longacre initiated a long-term study of ceramic production, use, and deposition in the Philippines (1974), and, under the aegis of behavioral archaeology, Reid, Rathje, and Schiffer (1974; Reid, Schiffer, and Rathje 1975) developed a conceptual framework for actualistic research that translated into a wide-ranging interest in modern material culture (e.g., Schiffer 1992; Schiffer, Butts, and Grimm 1994; Rathje and Murphy 1992).[19]

Reflecting on the need for, and the implications of, a serious commitment to actualistic research, Deetz (1970) proposed "a novel experiment for archaeologists," as Leone later described it (1972a: 91). He recommended that archaeologists consider redefining their discipline as the study of "the material aspects of culture in their behavioral context, regardless of provenience"—a suggestion that, he hastened to add, has quite profound implications: "in one sense (i.e., with this broadened definition of subject matter), I have just now abolished the field of archaeology as we know it" (Deetz 1970: 122). Deetz seems an unlikely person to make such a proposal, given his early critique of the research initiatives of second-generation New Archaeologists. Actualistic research stands at one remove from archaeology itself, at least as conventionally defined, and yet it is here that archaeologists seem most likely to generate rather than consume laws of cultural process. Indeed, in

embracing a covering law model of explanation, New Archaeologists risk a double deferral of their processal goals: an understanding of cultural process at the system-level depends on first reconstructing the instances—the particulars of past cultural systems—that processal laws must cover; and that reconstruction depends, in turn, on establishing the laws necessary to secure first-level arguments of relevance—the linking principles that connect archaeological material to its cultural antecedents. There are thus several senses in which descriptive reconstruction and explanation are interdependent, and several respects in which the line that divides genuinely processal, anthropological understanding from other forms of explanation is eroded when the advocates of deductivist ideals undertake to specify precisely what the goals of inquiry must be, given their commitment to a covering law model of explanation.

EXPLANATION: FORM VERSUS CONTENT

Whatever the prospects for contributing to the store of general anthropological knowledge of cultural process, second-generation New Archaeologists were unequivocal in their commitment to explanatory goals; it was critical to move decisively beyond descriptive space-time systematics and make effective use of archaeological data as evidence of the cultural past, however broad or narrow the scope of the questions framing their inquiry. But even these more generic explanatory objectives proved complicated, raising a number of residual questions about what constitutes a compelling explanation, and here the constraints of commitment to a covering law model come into clear focus.

With respect to first-level explanations, Fritz and Plog explicitly declare that the laws required to establish compelling (retrodictive) arguments of relevance are "deterministic" and "causal."[20] A generalization that captures regularities among variables might meet the formal requirements for a covering law on the Hempelian model—it might be "true, universal and conditional" (1970: 407)—but if it is an "accidental generalization" it will not provide grounds for drawing conclusions about the behavioral antecedents that were responsible for particular archaeological traces. As Binford had argued, something more than a claim of constant conjunction is required to secure ret-

rodictive inference; the laws establishing the connection between antecedent and consequent must demonstrate that "one set of phenomena (past behavior) was *sufficient* to produce a second set (the characteristics of the artifact or feature)" (J. Fritz and Plog 1970: 407; emphasis added).[21] When Fritz and Plog elaborate this requirement, they initially follow a Hempelian line of argument; even the ascription of functions to tools must be understood as a retrodictive inference that depends implicitly on covering laws. At the same time they insist that such inference depends not so much on knowledge of the kind provided by Hempelian covering laws—knowledge *that* certain attributes are regularly correlated—but on an understanding of *why* they are correlated: how particular kinds of material trace can be produced and the conditions that must obtain for them to be produced. They thus imply that the ethnoarchaeological and experimental research required to secure arguments of relevance must be designed not to document regularities of material:behavioral correlation but to provide an understanding of the nexus of causal and quasi-causal conditions that would have generated those regularities, linking behavioral antecedents to material outcomes.[22]

Of second-generation New Archaeologists, Schiffer (1975) and Gould (1978a) are among the most uncompromising advocates of positivist, deductivist ideals in the design of actualistic research.[23] They insist that the central objective of this work must be to "discover consistent relationships that exist between different kinds of material remains and human behavior" (Gould 1978a: 816), which can be formulated as "atemporal, aspatial statement(s) relating two or more operationally defined variables" (Schiffer 1975: 4; see also 1978a: 232). But when Gould turns to a more detailed account of the kind of understanding ethnoarchaeology can provide, he likens it to grasping the rules governing language use: ethnographic observation provides an insight into the cognitive principles, the cultural "rationale" and world-structuring normative systems, that constitute the underlying mechanisms responsible for "human residue behavior in particular societies" (1978a: 816). Although Schiffer is more consistent in characterizing actualistic research as a positivist law-testing enterprise, he makes a strong case against the simplistic view that the archaeological record directly reflects the location

and form of past activities (1972a: 156, 1972b: 163);[24] retrodictive inference cannot be secured by simple correlational laws that link surviving material remains to the activity structures of a living culture. If archaeologists are to grasp the cultural significance of their data, they must also understand the operation of a wide range of intervening processes that contribute to the formation of the archaeological record; they must therefore have a robust understanding of *causal* mechanisms and processes that goes well beyond identifying simple correlational principles.

This tension between the constraints of a positivist conception of covering laws and causalist intuitions is also evident in analyses of second-level explanations of various degrees of complexity and generality. When Watson, LeBlanc, and Redman (1971) consider what "scientific" explanation requires in terms of specific examples, they expand on Binford's analysis of the explanation offered by Sabloff and Willey (1967) for the Mayan collapse. Like Binford, their diagnosis of why this account falls short diverges sharply from the covering law model they otherwise endorse. In the spirit of disembedding the covering laws implicit in an "explanation sketch" (as Hempel 1942 refers to incomplete covering law explanations), they argue that an adequate account of such a complex cultural event requires not a single general law linking invasion and collapse but a system of laws linking various factors that constitute these aggregate events; in this connection they identify a number of assumptions that Sabloff and Willey would have to make explicit and substantiate if their account was to explain the Mayan collapse. But in fact, the content that Watson, LeBlanc, and Redman require of these subsidiary principles is much richer than the statements of constant conjunction that make up Hempelian covering laws. They include, for example, assumptions about the intentions and power of the invaders and the vulnerability of the society invaded, as well as the means by which invasion was carried out such that it could (or did) bring about the large-scale collapse of the Mayan social and political system. In elaborating these suggestions, Watson, LeBlanc, and Redman consistently focus on the nature of the social groups in question—their constitution and internal dynamics, and the powers and liabilities they have as a consequence—as well as on causal mechanisms that might have medi-

ated the conjunction linking invasion and collapse: "decimation of the native population, disruption of the economy and communication systems, widespread destruction of property, forcible removal of the native power structure and substitution of a new power structure administered by the alien invaders, and so on" (1971: 28). If these assumptions were refined and tested, the grounds for accepting the claim that an invasion explains the Mayan collapse would be the plausibility of a closely specified model of how, to paraphrase Binford (1968d: 268), invasion actually brought about collapse in this particular case, not an independently established law linking constituent variables in patterns of constant conjunction.

These decidedly nonpositivist (realist, causalist) intuitions about the content required for explanatory understanding are, if anything, even more explicit in the debate about second-order, processual explanation that arose in the early 1970s, when internal critics of the covering law model proposed a "systems" approach to explanation. As Flannery outlines this alternative, it is inspired by a reaction against deterministic explanations of complex systems:

> The law-and-order archaeologists' version of Hempel—or, at least, the way they apply it—is precisely the physical science approach that [Ludwig] von Bertalanffy rejected in the 1920s as being inadequate in dealing with biological phenomena. In fact, von Bertalanffy originally developed systems theory because the laws of physics and chemistry had failed to adequately describe or explain life processes and living systems, under which heading prehistoric populations must certainly fall. (1973: 51)

Broadly conceived, systems explanation is to be achieved not by setting particular events or attributes of a cultural system into a generalized pattern of conjunction among such variables that it holds regardless of context but rather by building a detailed model of the particular system in which the *explanandum* occurs, then testing to determine whether, in fact, the entities and forces posited by the model do actually exist and interact—and produce the observed outcome—as hypothesized.

In this spirit Tuggle, Townsend, and Riley object that the covering law model of explanation (particularly its deductive-nomological version) is "mono-causal" and therefore incapable of dealing with the complexity and particularity of cultural

phenomena at a systemic level (1972: 7). Like Flannery, they advocate a "systems paradigm," but they draw on different sources—Meehan's systems model of explanation in the social sciences (1968)—and argue for a highly formal approach to modeling the internal structure of cultural systems. The goal of a systems approach, on their account, is to build a formal calculus that is capable of predicting (or simulating) "all the possible outcomes of variable interaction"—all the possible states or behaviors a system may exhibit—given a catalogue of the variables that constitute the system and a set of abstract rules that govern their interaction (Tuggle, Townsend, and Riley 1972: 8). Explanation is then a matter of showing that particular events or system states are "to be expected," given the configuration and dynamic of the system, not because they fit a regular (system-independent) pattern of conjunction—"Meehan's explanations do not deal with general laws of any sort" (1972: 9)—but because, at a highly abstract level, the model captures the underlying structure and dynamic of the particular system in question. It is irrelevant, on this account, whether any comparable system or outcome has been realized before.[25] Flannery objects that even if this variant of the systems approach were applicable to archaeological problems, which he doubts, the formal "rules" that underpin Tuggle, Townsend, and Riley's systems explanations suffer from the same limitations as Hempelian covering laws: as formulated, they "reveal nothing about causality" (Flannery 1973: 52). And indeed, Tuggle, Townsend, and Riley are themselves quite explicit that these rules—which (statistically) capture patterns of interaction between system variables— "say nothing about the causal relations operant among the data" (1972: 9): the model is intended to be an abstract calculus that establishes purely formal relations between system states and their outcomes.

Within a year LeBlanc, an early advocate of the covering law model, responded to Tuggle, Townsend, and Riley's proposals with two counterarguments (LeBlanc 1973). First, LeBlanc insists that the covering law model is by no means "monocausal" in any sense that precludes its application to complex phenomena. There is nothing in the Hempelian model itself that requires complex phenomena to be explained in terms of a single

covering law, indeed, any number of laws may be invoked that specify regularities between subsets of the variables that constitute a complex system.[26] As Watson, LeBlanc, and Redman suggested in their analysis of the Mayan collapse, the explanation of large-scale events in such a system will typically require a concatenation of smaller-scale explanations that cite laws establishing linkages not between comprehensive system states or events, such as invasion and collapse, but between the constituent properties of these systems and the conditions that affect their more localized form and patterns of interaction.[27]

Second, LeBlanc argues that on close inspection, the Meehan systems model proves to be a variant of the covering law model. "Mere description," he objects, does not on its own provide "sufficient explanation"; "describing the relationship (R) among several variables does not explain the final state of the system" (1973: 206). There must be a stronger link between the variables that make up particular antecedent and consequent states if they are to be explained by subsumption under a description of the system as a whole. In line with his commitment to a covering law model of explanation, LeBlanc concludes that the "rules" defining the relationships that hold among variables must be covert covering laws: they must summarize correlations that reliably hold between these types of variable regardless of context; otherwise, there is no reason to believe that they are anything but accidental generalizations. It is no stumbling block for the covering law model that, on Tuggle, Townsend, and Riley's account, these rules are typically statistical in form rather than nomological (i.e., they capture probabilistic rather than invariant or universal relations); by the mid-1960s Hempel had himself expanded his account of this model to include deductive-statistical and inductive-statistical variants (e.g., in his essay "Aspects of Scientific Explanation"; 1965: 331–496), as well as the original deductive-nomological formulation that had become influential in archaeology. LeBlanc concludes that "Meehan's systemic explanation is a model compounded of a set of laws" that, although it "allows for more powerful explanation than any single law-like generalization encompassed by it," is not structurally different from standard covering law models (1973: 212). In short, because Tuggle, Townsend, and

Riley treat the rules central to their system models as purely formal constructs, LeBlanc has little difficulty assimilating their alternative to a logical positivist account of explanation.

Although persuasive, once the terms of debate set by logical positivist/empiricist conceptions of evidence and explanation are accepted, LeBlanc's analysis misses the potential force of the critique implicit in arguments for a systems approach; moreover, it does nothing to address a number of widely recognized difficulties fundamental to the covering law model. Merrilee Salmon (1978), for example, points to problems of relevance that had been the focus of philosophical attention for over a decade (e.g., as summarized in W. Salmon 1971): how do covering law theorists determine which generalizations (which statements of correlation), out of all those that might cover the phenomena in question and meet the requirements of a covering law model, are explanatorily relevant? She has in mind such infamous counterexamples as a hypothetical case in which the explanation given for why a particular man never gets pregnant is his practice of taking birth control pills (W. Salmon 1971: 11–12, 34). The implicit law—that men who take birth control pills never get pregnant—is true, universal, and conditional, but clearly irrelevant; and yet, as John Fritz and Plog had observed (1970), on the covering law model nothing in the form of the argument or its covering law disqualifies it as an explanation. Although, Salmon argues, the systems theorists' critique of the covering law model was motivated by concern with such problems of relevance, the alternative proposed by Tuggle, Townsend, and Riley hardly resolves them: "one feature of Meehan's account is that the mathematical model may come from any source at all—if it fits, then it explains" (M. Salmon 1978: 22); and surely computational adequacy is no more adequate for explanatory understanding than deductive (or inductive) subsumption. The fact that the declining birthrate in the United States "fit[s] an equation . . . developed from studies of fruitfly populations" does nothing to explain the human birthrate curve (M. Salmon 1978: 22). Or, to take a familiar example from the physical sciences that Merrilee and Wesley Salmon (1979) use to illustrate this point more generally, a mathematical model of the sort required by Meehan could be designed to simulate the interaction between the variables of temperature, pressure, and volume in a system consisting of a closed container of gas; but as useful as this model might be in predicting how each variable will change in relation to the others, we would not be inclined to say it had explained any of the states it simulates.

After a decade of controversy, LeBlanc reversed his position and, in collaboration with Read, agreed that the problems of relevance raised by Salmon and by some of the advocates of systems approaches are insurmountable. The covering law model is "inadequate as paradigm for what can be termed an intuitively satisfying explanation"; it cannot be expected that "a specific case [will be] explained by showing it to be a particular instance of a general law" (Read and LeBlanc 1978: 308, 309). For "useful scientific explanation," it is crucial to establish a necessary, not merely a regular (and possibly accidental), relationship between antecedent and consequent. To illustrate this point, Read and LeBlanc consider another well-worn philosophical example: the hypothetical explanation of whiteness in swans.[28] To account for the whiteness of a particular swan, they argue, something more is required than the subsumption of this instance under a generalization that all swans are white. It is crucial to show why the generalization holds and this is typically accomplished, according to Read and LeBlanc, by embedding it in an encompassing theory. Regarding the coloration of swans, a "somewhat more satisfactory" account might cite the color of its parents and invoke Mendelian-style laws of genetic inheritance. To account for these patterns of inheritance it becomes necessary, in turn, to consider the genetic mechanisms that are responsible for the characteristic whiteness of swans; and to explain why swans, as a species, have this genetic endowment it might be appropriate to appeal to "the laws of evolution plus the specific environmental constraints to which swans have adapted." Beyond this point, questions arise about "what causes natural selection," and so on (309). In principle this explanatory regress is infinite. What determines whether a particular explanation is "satisfactory" or "useful," on Read and LeBlanc's account, is the comprehensiveness of its theoretical grounding; the regress stops when the phenomenon that requires explanation has been embedded in con-

firmed theory that does not require explanation itself because it "include[s] all other phenomena perceived to be of the same character as the instance in question" (310). They suggest that the color of swans is adequately explained by an appeal to the adaptive conditions that selected for their whiteness because at that point in the regress, the explanation is comprehensive enough to account for species coloration generally: it includes the blackness of ravens as well as the whiteness of swans. In this sense the "substantive content" of explanatory accounts determines their adequacy (308); an account is explanatory if embedded in a higher-level theory of appropriate scope and content.

When Read and LeBlanc characterize this crucial requirement of theoretical embedding, however, they rely on an expanded repertoire of empiricist concepts, drawn from Ernest Nagel (1961), that undercuts the realist, causalist character of their critique of the covering law model and their rationale for seeking successively more fundamental explanations for the whiteness of swans. They describe the necessary higher-order theory as an abstract calculus that provides a formal synthesis of experimental laws (laws that directly systematize observable data) by demonstrating that the regularities they capture exhibit a deeper structure common to all instances of the phenomena in question and, at their most abstract, common to neighboring subject domains. On this account, the theories themselves have no empirical content; they make no claims about the microconstituents of the subject domain or about underlying causal mechanisms and processes. They acquire content derivatively when applied to an empirical domain, and they explain not by showing how disparate phenomena are produced by common causal mechanisms but by establishing that "quasi-regularities at a concrete level" all exhibit the same "underlying general principles" of organizational structure (Read and LeBlanc 9178: 312).

Consistent with this view, Read and LeBlanc characterize the theory required for *archaeological* explanation as a purely formal, mathematical calculus that captures, by means of progressive curve smoothing and abstraction from descriptions of empirical regularities, "an underlying orderliness or patterning in the data" (1978: 311). To illustrate their point Read outlines, in an appendix, a "formal theory of population size and area of habita-

tion" (Read and LeBlanc 1978: 312–317). In nine definitions and three axioms, he specifies the assumptions of a comprehensive theory of settlement systems designed to capture general properties of the relationships that hold between area and population size "when certain structural arrangements are held constant" (313), then uses these principles to develop three models of hunter-gatherer campsites based on data gathered by Weissner (1974) from a sample of Bushman campsites. The mathematical model itself is entirely abstract, incorporating no substantive content beyond the assumption that human campsites (will) reveal generalizable, if not universal, structural properties (Read and LeBlanc 1978: 317); instead, it provides formal tools for representing the empirical regularities that hold between specified variables (site area and population size, in the case of the models of hunting-gathering campsites). In application to particular instances of small-scale human settlement, Read's formalism substitutes for more cumbersome, literal descriptions of the phenomena, establishing *that* a particular "principle of organization" holds in a specified range of cases. But as one commentator observes, "the model appears to tell us little about WHY these 16 Bushman camps are so ordered" (Bayard 1978: 318).

As a prospective basis for explanation, it is by no means clear how problems of relevance are to be resolved by embedding lower-level "quasi-regularities" in theoretical models of this kind. The approach to explanation that Read and LeBlanc propose is, in essence, a hybrid of the covering law and formal systems models that they themselves found inadequate; explanation remains a matter of subsuming instances under regularities that hold among variables, and then subsuming these regularities under still more general, theoretical statements about the underlying orderliness of complex systems. Even if the formal model developed by Read and LeBlanc was a reliable basis, in archaeological applications, for inferring population size from site area in hunter-gatherer campsites (i.e., even if it was shown to capture robust regularities in the relationship between these variables), it no more explains the size of a particular camp than the appeal to a generalization about the whiteness of swans explains the color of a particular swan. Moreover, by their own critical analysis of the systems theorists' pro-

posals, the explanatory power of this model would not be substantially improved by demonstrating that it describes a localized pattern that fits a more general algorithm—for example, the structural properties of a wide range of settlements. Such a model provides no understanding of *why* a given "principle of [spatial] organization" obtains, or what mechanisms and processes generate it in particular instances, comparable to the genetic mechanisms and selective pressures that Read and LeBlanc cited in their example of an improved explanation for the whiteness of swans.

The irony here is in the explanatory regress that Read and LeBlanc invoke as a source of enriched content, theoretical models are actually divested of content; at each level of theoretical embedding, explanation is reduced to progressively more schematic systematizations of observables. Questions of relevance simply reassert themselves at a new level: how are spurious examples of pattern fitting, like Salmon's fruit fly algorithm, to be excluded? Following Hesse's early analysis (1959, 1966), and later realist arguments of Boyd (1973) and Bhaskar (1978), it is not clear how underlying structures can even be identified, much less selected (from among all those that might capture the correlational patterns manifest in a complex system) as the salient basis for an explanation, unless pattern detection and selection are informed by some understanding, however hypothetical, of the causal properties of the systems' constituent parts and processes (see chapter 5 for more discussion of this line of argument).[29] To explain falling birthrates, Salmon argues, it would be necessary to consider not just the structure of an emerging statistical pattern but a range of factors that might be responsible for generating that pattern: for example, "the availability of various contraceptives and the widespread publicity about the dangers of overpopulation, or the existence of family planning agencies" (M. Salmon 1978: 22). Likewise, on Salmon and Salmon's generalized account, the gas laws are only explanatory given an understanding of the constituents of gases and their dynamics, not assimilation to a more abstract model of the regularities they capture. Although, as they observe, "it is surprisingly difficult . . . to produce an adequate [general, philosophical] treatment of causal relations," in most contexts it is attention to explicitly *causal* factors that grounds explanatory understanding; "the time has come to put 'cause'

back into 'because'" (M. Salmon and Salmon 1979: 72).[30] The neo-positivist account of scientific theory that Read and LeBlanc invoke is inimical to these insights, predicated as it is on the empiricist commitments that gave rise to Hempel's intractable theoretician's dilemma (1958). The result is an account of explanation in archaeology in which the tensions inherent in Binford's early treatment of explanatory goals—between the commitments implicit in a model-building approach and the empiricist presuppositions reintroduced with the covering law model—come to a head in a number of starkly drawn internal contradictions.

BEYOND "SAVING THE PHENOMENA"

Through the 1970s, as the debate over the merits of the covering law model unfolded, a great many North American New Archaeologists and their British counterparts sidestepped the programmatic issues it raised and focused their attention on developing and implementing a modeling approach to various aspects of archaeological research. For some this was a means of operationalizing a systemic view of culture, but it also appealed to those with a more generic commitment to establish a systematically scientific, "analytic" (Clarke 1968) research program in archaeology.[31] An enormously rich and diverse tradition of modeling practice has since grown up in Anglo-American archaeology, in many respects embodying the most promising substantive (nonpositivist) insights of the New Archaeology (see, e.g., Aldenderfer 1991).

From the outset, however, it was clear that a commitment to model building can take very different forms. As early as 1972 Clarke advocated a "pluralist view" of archaeological modeling (1972a: 4), and he outlined the merits of models that vary widely in purpose and, therefore, in scale, content, and form, as well as in such qualities as precision, efficiency, and predictive power. They range in scale and breadth from "controlling models" that function as paradigms (e.g., ecomaterialist or normative models that define in general terms the nature of the subject domain) to case-specific "operational" models of episodes of cultural change (e.g., those proposed by contributors to Renfrew 1973b) or, more narrowly, the concatenation of conditions responsible for particular

aspects of the archaeological record (Clarke 1972a: 5–6, 10). Some are phenomenological models designed to capture the variability, in form and in spatial and temporal distribution, of archaeological material (e.g., the campsite model outlined in the appendix to Read and LeBlanc 1978: 312–317; the models of site distribution and seriation patterns cited by Clarke 1972a: 18, 25, 36). Others go decisively beyond the systematization of observables: they simulate the long-term operation of subsistence systems (e.g., Thomas's simulation of Steward's desert culture model of the Shoshone seasonal round, 1969, 1972, 1973; Steward 1938); the social structure of complex societies (Clarke's example of a system model of a Danish medieval parish, 1972a: 32); the cultural conventions evident, for example, in site structure or architectural design (e.g., the architectural grammar identified by Glassie 1975, or the structural analysis of site layout at Glastonbury in Clarke 1973b, both discussed in chapter 8); or the complex of demographic, environmental, and technological factors responsible for large-scale cultural transformation (e.g., the demic-diffusion model of the Neolithic Revolution proposed by Ammerman and Cavalli-Sforza 1973, 1979, and endorsed by Renfrew 1987, discussed in chapter 16). And along both continua of scale and content, models may take radically different forms ranging from informal, qualitative and narrative models to highly formal mathematical models and computer-automated simulations (e.g., Clarke 1972a: 54).

Despite the considerable enthusiasm for formal modeling techniques evident in Clarke's 1972 review (1972a; see also contributions to Clarke 1972b), in 1975 Doran and Hodson offered a number of reasons for treating their application to archaeological problems with caution; these have been reiterated in most subsequent assessments (e.g., Aldenderfer 1991). In particular, they warn that mathematically tractable models are "too simple for most archaeological problems"; and even when computer simulation makes it possible to cope more realistically with the complexity of archaeological subjects, it requires a level of understanding of the conditions and processes involved that is "only rarely met in archaeological work" (Doran and Hodson 1975: 315).[32] The most successful subsequent modeling practice takes seriously both concerns. As Flannery describes the model of the evolution of foraging strategies into incipient agriculture practice presented in *Gila Naquitz,* it was critical both to build in sophisticated patterns of interaction between variables and to avoid reliance on "made up" values for inputs; "we have done everything we could think of to make the model realistic" (1986: 436). When the challenge of realistic modeling is not a priority or cannot be met, the imminent danger to which Doran and Hodson draw attention is that the appeal of spurious formalism will overwhelm the demand for explanatory content; archaeological modeling will revert to the purely formal or, alternatively, to the particularistic and phenomenal (descriptive) end of the continua of scale and content along which archaeological models lie. Lewis Binford makes this point directly in a retrospective assessment of his earliest programmatic statements:

> Modeling is sometimes a deceiving business. I have had the experience of generating what I thought of as a model, only to realize that what I had generated was a cognitive map, a set of descriptive categories in terms of which I could talk about data but which did not have the properties of dimensions that could be operationalized beyond the empirical cases they subsumed. Any science must, of course, develop a "metalanguage" as it advances in the recognition of relevant phenomena and becomes more sophisticated in the development of models. However, there is a big difference between this and what some persons accept as explanation. (1972a: 335–336)

The conceptual resources introduced with a new metalanguage—a cognitive map (or "controlling model") that offers new categories for description or analysis—can be instrumental in bringing into view "previously unrecognized forms of patterning," and it may be tempting to "feel that some kind of 'explanation' has been achieved when such a relationship can be accommodated to a familiar cognitive unit, a term or phrase" (L. Binford 1972a: 336). But in the end, Binford insists, "mathematical or cognitive techniques for *describing* recognized forms of patterning and distributional phenomena are just that—*descriptive*" (336; emphasis in the original). In themselves they provide no understanding of the cultural processes or causal dynamics responsible for the patterns that can be systematized at an observational level. The tensions inherent in Tuggle, Townsend, and Riley's proposals for systems explanation, as well

as in Read and LeBlanc's theoretical embedding approach, bear witness to these pitfalls; their formal models marginalize the very content that, by their own critical diagnosis, they find necessary for explanation.

Binford's critique presupposes a distinction between types of models that was, at the time (in the early 1970s), a matter of some interest to realist philosophers of science. As Bunge describes it, in "From the Black Box to the Mechanism" (1973: 101–105), there is a fundamental difference between models formulated with the objective of disclosing "what is inside the box," by way of inner structure and behavior-generating mechanisms, and models that characterize systems strictly in behavioral terms. The latter—opaque or, to use Bunge's term, "black box" models—establish correlations between inputs and outputs, patterns of stimulus and response, and they characteristically ignore internal mechanisms or processes (108); they conform to positivist/empiricist constraints on cognitive content inasmuch as they are restricted to the descriptive systematization of observables (192). In an archaeological context these include the range of operational models described by Clarke that are developed by "building up" a direct representation of archaeological or ethnographic data.

At one step beyond behavioral, opaque box modeling lie "gray" or "translucid box" models, positing the internal states of the system that link input and output without specifying any internal structure: "no mechanism has been conjectured. . . . [T]he model includes endogenous variables but [these] . . . are just intervening variables with a computational rather than representational value" (Bunge 1973: 10). In a parallel analysis of scientific modeling, Harré characterizes this class of models as specifying the (causal) powers of a system—its capacities to act or to be acted on under various conditions—but attributing those powers to an unspecified "intrinsic nature" (1970: 83). The references to mediating variables are placeholders; they provide a "schematic explanation" of system behavior that invites further investigation of actual but unknown internal states that enable the system to behave as it does (Harré 1970: 83). The critiques of space-time systematics (e.g., McKern-style typological systems) offered by radical critics of the 1930s and 1940s, and later by the New Archaeologists, suggest that such systematizing schemes are models of this kind. They simply redescribe the formal variability of archaeological assemblages in cultural terms; typological categories—cultural traditions, horizons, affinities, lines of cultural diffusion and evolution—stand in for a more detailed specification of the cultural forms of life and interactions among the human communities who made and used this material. So long as models of this kind primarily serve an instrumental or heuristic function, they also satisfy empiricist requirements: "without going much beyond the data it enables one to condense the latter and even to predict the evolution of the system. But no model of this kind, be it a black box or a grey one, will *explain* the behavior both external and internal of the system. Moreover, it will remain isolated from the rest of our knowledge of things or at least will make no use of it" (Bunge 1973: 103; emphasis in the original).

By contrast, a final class of models, transparent box models, embodies a commitment to "throw further light into every box" (Bunge 1973: 104); they posit the "inner workings"—the internal structure and mechanisms—of the represented system, building on collateral knowledge of more familiar (or accessible) systems that suggest how its manifest patterns of behavior may be generated. These models incorporate theoretical content, in the form of claims about the (unobservable) constituents of a system and their causal properties, that are not reducible to descriptions of observable properties and behaviors. Indeed, on a realist construal, the analysis and manipulation of observables become not ends in themselves but the basis for building and testing prospective transparent box models of internal (or underlying) causal mechanisms and processes.

These broad classes of model are themselves internally complex. Models all along the opaqueness spectrum may be *sentential* or *formal/mathematical* in form; they are conventional, symbolic representations of the target objects they are designed to represent (Harré 1970: 33, 36). At the opaque end of the spectrum, phenomenal and behavioral models incorporate no content beyond the observations they systematize and are typically *homeomorphic:* the source on which they are modeled is the same as the subject or target they are meant to represent. The mathematical models of settlement patterns described by Read in the appendix to Read and LeBlanc (1978: 312–317) are

formal, homeomorphic models based on ethnographic data that are formulated at various levels of abstraction.

By contrast, *iconic,* or "picturing," models are not strictly formal and conventional; subject to "projective conventions" they exploit some dimensions of physical similarity (between model object and target) in the representation of their target (Harré 1970: 33, 38, 52–54). If an iconic model is based directly on its subject, it is homeomorphic; archaeological examples include scale models of an archaeological site or the replication of prehistoric tools, which Clarke describes as the most familiar of operational models in archaeology (1972a: 13). But if an iconic model is based on sources other than its subject, then it is *paramorphic;* if it draws on a number of different sources, it is a *multiply connected* paramorphic model. Whether sentential/formal or iconic, any transparent box representation of the "inner workings" of an enigmatic system will be a paramorphic model. Virtually all archaeological models of the cultural past (descriptive, reconstructive, or explanatory) are paramorphic,[33] and most are multiply connected. Such models make empirical, existential claims that go well beyond the systematization of observables; they "stand in for the real mechanisms of nature of which we are ignorant," enabling researchers to "picture possible mechanisms for producing phenomena" (Harré 1970: 54). On a realist theory of science, the central aim of scientific inquiry is to build and test such models; it is their extraobservational content that grounds explanatory understanding.[34] When Binford distinguishes between descriptive and explanatory models, and when archaeological critics of the covering law model and formal systems theory insist that genuine explanation requires more than fitting instances to regularities, it is this kind of model content that they call for.

Although the distinction between description and explanation is quite sharply drawn in these early postpositivist accounts of scientific modeling, a consistent realist must regard it as inherently unstable. In the biophysical sciences, technological developments may quite literally bring underlying mechanisms into view, thereby making accessible to direct phenomenal modeling that which could be pictured only hypothetically with a paramorphic iconic model. But even in cases in which the target is, in principle, inaccessible and homeomorphic modeling is not an option, an explanatory regress is always imminent. When the behaviors or properties of a subject of inquiry have been explained at one level by successfully modeling mechanisms that produce them at another, the question of how these mechanisms are themselves generated and sustained can always be raised, as illustrated by the explanation for the color of swans considered by Read and LeBlanc. But the considerations that stop this regress are pragmatic as much as evidential or theoretical: they have to do with what we want to know and why, and with the resources we have for building and testing iconic models in the field in question. At any level of resolution an account will be explanatory insofar as there are good empirical grounds for accepting its claims about the existence and powers or liabilities of the mechanisms it postulates, and insofar as those mechanisms account for the behaviors or properties (instances or regularities) we find puzzling. What counts as a compelling explanatory claim is therefore unavoidably domain-specific. The degree to which it is compelling will depend on what we understand about the constitution and the operation of a particular subject domain: what explanatory questions we are in a position to pose and what hypothetical answers we have the resources to formulate and test. Such domain specificity seems especially to hold in archaeological contexts, and it reinforces Clarke's brief for explanatory pluralism; there are inevitably "many competing models for each archaeological situation, where none may be finally picked out as uniquely and comprehensively 'true'" (1972a: 4; he credits Hesse 1966 with this idea). Indeed, as our understanding is refined and expanded, this plurality of models should be expected to proliferate.

Finally, if one central objective of scientific inquiry is to develop and test models that render opaque and translucid systems as close to transparent as possible, then the empirical practices of the enterprise must be understood in rather different terms than those afforded by a positivist/empiricist model of confirmation. Most broadly conceived, the process of systematic empirical investigation is a matter of exploiting what we know of familiar systems to build hypothetical models of puzzling, poorly understood systems, usually

by means of analogical reasoning (see chapter 9), then simulating their operation and searching for evidence that should be present, or that can only be present, if the system is constituted and operates as postulated. In archaeological contexts this practice is perhaps best characterized as a "bootstrap operation," as Mellor described it in an early philosophical assessment of initiatives associated with scientific archaeology (1973: 479; see also Mellor 1974). It involves continuously tacking back and forth between the archaeological evidence of the subject of inquiry and the source contexts that supply the resources necessary both to build hypothetical models of the subject and to test them (see chapter 11, and the essays in part IV).

Although not formulated with reference to model testing, Bruce D. Smith's proposal of a hypothetico-analog method of confirmation (as an alternative to a Hempelian hypothetico-deductive method) captures many of the salient features of this process.[35] Of critical importance is his emphasis on the role played by considerations of plausibility in initially formulating and evaluating hypotheses and on the dependence of archaeological testing on the auxiliary hypotheses that underpin (analogical) arguments of relevance (1977: 604, 612). As difficult and uncertain—as irreducibly inductive (qua ampliative)—as it is, Smith argues that this process of building and evaluating hypotheses about the cultural past holds great promise: if archaeologists can become "good enough puzzle solvers," they will be able to "see beyond the patterns of cultural debris to the behavior patterns of prehistoric human populations" (114).

Of all the philosophical theories of science that were actively debated in the 1960s and 1970s, those most congenial to the substantive insights of the New Archaeology are the various forms of scientific realism advocated by philosophical critics of logical positivism/empiricism. In an archaeological context it is Gibbon who most forcefully argued the case for considering realist alternatives to Hempelian positivism (1989).[36] In particular, a realist account of the goals of scientific inquiry captures the defining commitment of the New Archaeology: to effect a foreground-background shift in which systematic recovery and analysis of observables (the contents of the archaeological record) are not ends in themselves but a means of building and testing explanatory models of the unobservables of the cultural past—the cultural events and conditions, structures and processes to which the archaeological record bears witness. Moreover, such realism underwrites a strongly positive (if not positivist) appraisal of the archaeological enterprise; the alternative to deductive certainty, which is unattainable in any case, is not undifferentiated speculation but painstaking conceptual and empirical evaluation of (iconic, paramorphic) models that purport to represent conditions and events that actually obtained in the cultural past and are (in part) responsible for the contents of the archaeological record.

Recast in realist terms, the processual (anthropological) goals of the New Archaeology reflect not so much commitment to a different form of understanding (explanation as opposed to description) as an insistence that archaeological modeling can and should be pushed beyond the particularities of cultural lifeways and culture histories; the object of archaeological understanding should be long-term, systemwide cultural processes for which, it is assumed, the most salient explanatory factors will be material (ecological) conditions and mechanisms of adaptive response. I urge that this be understood as a jointly pragmatic and conceptual commitment, not as an empirical or theoretical imperative. It presupposes a particular conception of the cultural subject that is itself an ambitious empirical postulate. It is an open question whether cultures can all (and in all respects) be explained in ecosystemic terms; moreover, there are any number of other explanatory questions that can meaningfully be addressed in archaeological terms. The widely cited wisdom of Chamberlin's "method of multiple working hypotheses" seems especially relevant here (Chamberlin 1890; see also Platt 1964). The most compelling tests of any hypothesis are those that are comparative: tests that pit it not just against its negation (the null hypothesis) but against substantive rivals. Where ecosystem models are concerned, this means that problem-oriented archaeological research cannot be designed strictly as a search for supportive evidence. It will be crucial to systematically develop and test lower-level, context-specific models that bring into view not only those aspects of cultural

life that are most directly implicated in adaptive responses to the material (ecological) conditions of life, but also those that an ecosystem model suggests should be explanatorily irrelevant. In short, a strategic pluralism of explanatory goals is called for, both for the reasons cited by Clarke—as a concession to the enigmatic complexity of cultural phenomena—and as a necessary feature of the rigorous, self-consciously scientific approach to archaeological research advocated by the New Archaeologists.

Arguments for Scientific Realism

In 1980 van Fraassen published a widely influential defense of "constructive empiricism," The Scientific Image. *It was a rebuttal to the various forms of scientific realism that had been formulated, in the 1960s and 1970s, as an alternative to logical positivism. Realists — such as Harré, Bhaskar, Smart, the early Putnam, Boyd, and Glymour, among others — offered the diagnosis that positivist/empiricist theories of science had run aground on intractable internal difficulties because scientific inquiry could not be understood in terms consistent with logical positivist/empiricist commitments. In particular, they argued, the strategies of inquiry and successes typical of many of the sciences belie the thesis that their central goal is to "save the phenomena" (Duhem 1969). Rather than redescribing theoretical claims about unobservables as heuristic devices or theoretical "detours" that ultimately serve the purpose of systematizing observables, we should construe these claims literally, as referring to entities and events that are presumed to exist and to have the properties postulated. Although philosophical realism was never widely influential within archaeology, Gibbon (1989) made a persuasive case for its relevance and many New Archaeologists (and their antecedents) explicitly endorsed a strong commonsense realism.*

This essay was originally written in 1984 in response to the debate generated by van Fraassen's reformulation and defense of the central tenets of an empiricist theory of science. I make no direct link here to developments within archaeology but hope the analysis of the central dynamic of this debate indicates something of what a realist orientation offers a field like archaeology.

Although I have little sympathy for Ernest Nagel's instrumentalism, his "dictum" on the debates over scientific realism (as Boyd calls it, 1981: 644) is disconcertingly accurate; it does seem as if "the already long controversy . . . can be prolonged indefinitely" (E. Nagel 1961: 145). The reason for its continuance, however, is not that realists and instrumentalists are divided by merely terminological differences in their "preferred mode[s] of speech" (141); indeed, that analysis appeals only to those who are already convinced that realism of any robust sort is mistaken. Instead, the debates persist because the most sophisticated positions on either side presuppose fundamentally different conceptions of the aim of philosophy and of the standards of adequacy appropriate for judging philosophical theories of science. Realism and antirealism thus confront one another as preferred and largely incommensurable modes of philosophical practice; it is in this sense that they have "dialectical resources for maintaining [their positions] in face of [virtually any] criticism" (145).

In what follows I first give an analysis of re-

current strategies of arguments in the debate between realists and antirealists that shows how the locus of debate has shifted to metaphilosophical issues. On that basis, I characterize and assess the forms of philosophical practice that have emerged in this debate. My thesis is that this debate cannot be resolved on principled, philosophical grounds, but there may be pragmatic reasons for preferring a realist orientation in many contexts if the competing positions are judged *as* comprehensive research programs.[1]

FORMS OF REALISM

Scientific realism is at once attractive and controversial because it purports to preserve a good many cherished intuitions about the scientific enterprise. Most crudely, realists defend the commonsense view that science is in the business of investigating an independently existing reality and that its best-established claims about this reality (both observational and theoretical) are approximately true of it.[2] There are two ways of defending this theory to which its proponents return again and again. The first strategy is to eliminate rivals that challenge us to revise our realist intuitions, by showing that they are just wrong in what they claim about science or that their claims lead to absurdity if consistently developed. This approach yields various forms of default argument for realism:[3] realism is endorsed as the only alternative that survives criticism. The second strategy is to draw out and substantiate the intuitions that support realism so that it emerges as an especially plausible (not just the only remaining) philosophical account of science as actually practiced. This result is typically achieved either by indispensability arguments, according to which the acceptance of realist tenets is essential to scientific practice, or by miracle arguments, which are intended to establish the stronger conclusion that the truth of these tenets is a necessary condition for scientific practice or its success.

DEFAULT ARGUMENTS FOR REALISM

Postpositivists who advocate scientific realism generally assume the plausibility of commonsense realist intuitions and concentrate on the development of default arguments; their point of departure is a critique of positivism, particularly its criteria for distinguishing theoretical from observational claims. Faced with well-developed internal critiques of all standard criteria of demarcation, realists conclude that there is no basis for adopting categorically different epistemic attitudes toward theoretical as opposed to observational claims. They urge that, in principle, a realist construal may be appropriate for all classes of knowledge claims, including the theoretical, because the alternative of extending antirealist suspicions about theory to observational claims yields an untenable skepticism. As Churchland puts it, "we cannot adopt an instrumentalist or other non-realist attitude toward the doctrines and ontologies of novel theoretical frameworks unless we are willing to give up truth, falsity and real existence across the board" (1979: 2). Realism thus prevails by default so long as antirealists are unprepared to embrace such skeptical consequences or are unable to formulate a defensible criterion for setting claims about observables sharply apart from those that concern unobservables. The criticisms brought against van Fraassen's object-based criterion (1980: 16–18, 56–58) suggest that there may be no distinction to draw between theoretical and observational claims that is sufficiently hard-and-fast to do the work required of it by antirealists.[4] Nevertheless, the possibility does remain that a viable alternative might be forthcoming; consequently, this family of default arguments must be considered inconclusive. As the debate has unfolded, attention has turned to arguments that offer a constructive, rather than purely critical, defense of realist claims about science.

INDISPENSABILITY ARGUMENTS FOR REALISM

The simplest of the constructive arguments for realism turns on the observation that researchers in the advanced sciences are largely (and increasingly) preoccupied with what Hellman describes as "esoteric experiments" and "esoteric searches" (1983: 232); their objective is to find out about entities that exist beyond the range of our unaided senses (unobservables), either as an end in itself or as a means of explaining observable phenomena.[5] Putnam (in his realist phase, 1971) goes so far as to declare, on this basis, that it is disingenuous—"incoherent" and "intellectually dishon-

est"—to follow the antirealist in denying what we must believe in practice; it is simply necessary to "take a methodological stand" concerning the existence and accessibility of *some* such theoretical entities in order to get the research enterprise off the ground. There is an obvious difficulty with this line of argument, however; the indispensability of a presupposition does not guarantee its truth. It is conceivable that we may suffer systematic delusion about the necessary conditions for our own practice—indeed, such delusion may even confer such practical advantage that *it* becomes indispensable (as ideology) to the form of life it supports.[6] In the end, we must admit that indispensability arguments do little more than affirm that realism is intuitively compelling; it is in the miracle arguments that a strong constructive case is made for realism and it is in connection with them that metaphilosophical considerations come into play.

MIRACLE ARGUMENTS FOR REALISM

Those who advance miracle arguments take claims about the indispensability of realist assumptions as their point of departure, but they make the success of science rather than the nature of scientific practice itself their *explanandum.* The proponents observe that mature research is informed at all levels by theoretical presuppositions about the "inner structure and constitution of things" (Harré 1970: 15), and argue that its dramatic success is inexplicable—it would be a miracle—unless these presuppositions are, in fact, approximately true.

To secure this argument, realists must respond to various forms of "methodological Darwinism" that either deny the role of theory in research practice or deny it realist significance. For example, van Fraassen objects that there is no need to invoke realist conditions to account for the development of scientific knowledge; the constraints imposed by a demand for empirical adequacy are sufficient to account for success (1980: 40–41). Theories are, he says, "born into a life of fierce competition, a jungle red in tooth and claw"; those theories that survive are just the ones that "latch onto actual regularities in nature" (40). Realists such as Boyd and Rescher have countered that this selectionist argument rests on a mistaken

analogy. Methodological Darwinism is weakest at precisely the points at which biological theories of evolution have proved strongest; unlike its biological counterpart, it is falsified in ways that justify an appeal to some analog of the teleological forces rejected by biological Darwinists. The extent of scientific success is so great, and the rate and directedness of its development so rapid, that it cannot have been achieved by means of an "inductively blind" process of trial and error (Rescher 1977, 1978: 51–63, in response to Popper 1972: 242–247), or by means of a process of selection for hypotheses that are (merely) empirically adequate, as van Fraassen suggests (Boyd 1985: 23–30). An increasingly accurate theoretical understanding of an independently existing reality, especially of its underlying (often unobservable) causal dynamics, must be recognized as playing an essential role in the selection of hypotheses to test and in the design and evaluation of the tests to which they are subjected.[7]

Rescher's rebuttal to this Darwinian account of scientific success turns on a critique of how antirealists conceptualize the initial selection process by which hypotheses are identified that warrant testing. He proposes a "palatable" version of the Peircean thesis that we must have an inborn cognitive instinct for narrowing the field of all conceivable hypotheses to a few especially plausible candidates (Peirce 1934: 105–107); it is to be understood in purely methodological terms as a set of "heuristic principles of method . . . [that have] emerged from a process of trial-and-error in inquiry" (Rescher 1978: 61). In this case, however, it is on the strategies for selecting theses, rather than on individual theses, that the selective pressures of the cognitive enterprise operate.[8] This proposal simply pushes the miracle of success back one level and obscures the fact that scientific guessing is systematic—the key to its success on Rescher's account—precisely because it is *informed* guessing. A number of analyses of research practice, many developed without any direct reference to the debates over realism, make it clear that researchers proceed whenever possible by building on existing theories about the subject domain and related or analogous phenomena.[9] The implication realists draw from such examples is that the pattern of exponential increase in understanding characteristic of mature sciences must be

attributed to progressive improvement in the accuracy (realistically construed) of the background and collateral theory that informs the development of new theories; Peirce's "faculty of divining the Ways of Nature" (Peirce 1934: 107) is best conceived as the use of accumulated theoretical knowledge to determine what sorts of entities or causal mechanisms a candidate hypothesis may plausibly postulate to account for given sorts of phenomena.

On a realist analysis, in even the most cautiously empirical of research programs theoretical presuppositions play an important role in determining not only what hypotheses will be considered initially plausible but also how they will be tested and how the resulting evidence will be evaluated. Realists frequently point out that constant conjunctions of events—generalizable regularities—are rarely manifest in the observed world; they must be deliberately produced by experimentally closing down natural systems, limiting the range of variables that interact and manipulating the conditions under which they interact. This feature of research practice brings into view two levels of realist presupposition. Unless it can be assumed that the patterns of events observed under experimental conditions are the effects of independently existing causal structures, they cannot be expected to "persist and operate outside the context of [experimental] closure"—implying, in turn, that they cannot support the kind of discerning projection of regularities that makes possible the prediction and manipulation of phenomena, the central instrumental value of scientific knowledge (Bhaskar 1978: 64). In addition, even when the primary aim of inquiry is to systematize observables, researchers depend heavily on background knowledge and theoretical hunches about underlying causes to determine which variables they should manipulate and which empirical regularities, of all those that might be produced by experimental means, they will consider genuine, projectable regularities rather than mere experimental artifacts. Experimental practice is not a random search for regularities; [10] experiments allow for the isolation and manipulation of a suspected (theoretically postulated) causal mechanism, and regularities are documented as the (undisturbed, projectable) effects of its operation. In short, scientific success in establishing reliable empirical generalizations often depends directly on the accuracy of orienting theoretical assumptions about unobservable dimensions of the subject reality.

Boyd (1973, 1981, 1983) makes a parallel case for the theory dependence of esoteric research in experimental biology, where theoretical hunches are often the direct object of inquiry. This is a paradigm case in which researchers must be highly selective in the design and evaluation of experiments, given the complexity of the systems they investigate. They must control for alternative mechanisms and complicating factors that are known, on background theory, to be capable of replicating or masking the operation of the mechanisms postulated by the test theory, and they assess the import of experimental results in light of these same considerations; the plausibility of their test results depends on the likelihood that the experimental setup controls effectively for all theory-anticipated artifacts. Given these conditions, Boyd argues that if the theories informing research were replaced by empirically equivalent but theoretically divergent theories, "quite different methodological practices would be identified as appropriate," and quite different judgments would be made regarding the degree to which the test evidence can be said to confirm both claims about presumptive entities and "generalizations about observables" (1985: 9, 10). On this account, the process of selecting for "fit" hypotheses among those deemed plausible is not, and could not be, a matter of selecting only for empirical adequacy. It would indeed be a miracle that research so directly informed by theoretical presuppositions about "the Ways of Nature" should consistently pay off as it does, even at a purely instrumental level, if these presuppositions were not in fact approximately, and increasingly, true of an independently existing reality.

ANTIREALIST SKIRMISHING

Miracle arguments of the kind developed by Bhaskar, Boyd, and Rescher (among others) pose a new challenge to antirealists; they must show that the features of science that seem to require realist explanation—its success, its concern with theoretical understanding, and the theory dependence of experimental practice—can perfectly well be accounted for in nonrealist terms. To this end van Fraassen asks what purpose esoteric research serves if not ultimately to increase, by however

circuitous a route, empirical adequacy and predictive, manipulative power with regard to observable phenomena. He insists that all apparent reference to unobservables can be reformulated in sanitized empiricist terms as indirect talk about empirical consequences and the formal, structural resources of alternative theories (van Fraassen 1980: 77–80).[11] Larry Laudan argues the same point in historical terms. The junk heap of science is replete, he declares, with theories that once enjoyed considerable empirical success even though, in retrospect, it is clear that the claims they made about unobservables were just false. Approximately true knowledge of the (unobservable) constituents and deep structure of reality cannot, therefore, be a necessary or sufficient condition for the instrumental success of science (L. Laudan 1981: 33).

The cases Laudan cites have been reanalyzed in realist terms, revealing that they are by no means as unambiguously antirealist in import as he claims (C. Hardin and Rosenberg 1982),[12] and Boyd has reinforced his original analysis with examples of research practice in which the principles guiding inquiry must be considered irreducibly theoretical because they lack established empirical consequences.[13] These responses deflect the antirealist offensive that provoked them —they do provide compelling reasons for continuing to take realist accounts seriously—but their success is limited. The historical and methodological case for or against realism will always remain inconclusive; an antirealist of sufficient ingenuity will always be able to reformulate apparently realist references to or assumptions about unobservables as covert references to observables. What realists have shown is, however, that nonrealist accounts are often somewhat strained *reformulations* of a primary realist understanding of scientific inquiry and the knowledge it produces. They may serve certain philosophical ends but they leave the point and form of empirical research practice a mystery. Like the Craig and Ramsey theory-demolition strategies of the 1950s, it is technically possible to reconstrue theoretical claims in respectably empiricist terms; but this requires significant distortion of practice, which, on Craig's own account, does not afford any obvious clarification either of the reduced expressions or of scientific reasoning and methodology (Craig 1953: 54; see also Ramsey 1931).[14]

THE ESCALATION OF CONFLICT

At this point, the question arises of why one would seek an antirealist reconstrual of research practice: why go to such lengths to establish that the scientific enterprise does not, in fact, investigate and inform us about an independently existing reality in both its observable and unobservable dimensions? The answer seems to be that in the eyes of antirealists, the use of abductive inference to infer realist conditions of practice involves unsupportable epistemic risk.[15] On Laudan's account, "critics of epistemic realism have [ever since antiquity] based their skepticism upon a deep-rooted conviction that the fallacy of affirming the consequent is indeed a fallacy" (1981: 45). The principle underlying this objection, captured by van Fraassen's frequent comparisons of scientific realism to theism (see n. 6), is that insofar as we lack any context- or theory-transcendent standpoint from which to judge our theoretical claims it is unavoidable that we may be globally in error. The history of science teaches that by "pessimistic induction" (Newton-Smith 1981: 14), even the most seemingly indispensable and secure theoretical claims are likely to require revision (if not wholesale rejection) within the lifetime of a working scientist; by implication, the antirealist argues, it is the better part of wisdom to treat all theoretical claims with epistemic caution.[16]

As a methodological directive for philosophers, this principle of doubt counsels against adopting a principle of charity in the analysis of science:[17] "making sense of a subject [philosophically] need not consist in portraying it as telling a true story" (van Fraassen 1981: 665). Van Fraassen advises that philosophers should be "disinterested in the right way"; they must be impartial not only with regard to the competing theories that practitioners hold about their subject but also with regard to the reflective (metalevel) theories that they hold about the aims and achievements of the research enterprise. Where scientific realism is concerned, the moral is that philosophers should cultivate enough distance from practice that they can question the ontological and epistemic convictions (or presuppositions) that researchers take for granted. Affirming internal assumptions about the ontological conditions of practice and the epistemic status of our best theories is "not the only option we [philosophers] have" (van Fraassen 1981: 666);

a better option is to adopt the critical, uninvested stance of observers who have the freedom to question realist convictions and explore alternative, nonrealist ways of conceptualizing the research enterprise.

With these arguments the locus of debate about realism shifts from differences over the details of rival theories about science to a more comprehensive metaphilosophical disagreement about the principles that should inform the formulation and evaluation of these theories. If, as an antirealist like van Fraassen recommends, philosophical analysis should be governed by a commitment to minimize epistemic risk, then a nonrealist reconstrual of practice will always be justified, however strained or derivative it might be. Realist alternatives require a leap of faith that is, on this view, both epistemologically unjustifiable (it will always involve an insecure meta-abduction) and methodologically suspect (it embodies a lapse in the stance of disinterestedness appropriate to philosophical inquiry).

Realists respond that this shifting of the burden of proof onto their shoulders, this demand that they give special justification for going so far as to construe the central epistemic and theoretical presuppositions of successful practice in realist terms, requires its own justification. It is in fact illegitimate, judged in realist terms. The realist analysis of science is informed by a metaphilosophical rationale, as are antirealist analyses. Theirs is that philosophy (specifically, philosophy of science) is best conducted as an extension of science itself; its central task should be to provide a naturalistic, a posteriori explication of the theoretical knowledge and methodological principles embodied in actual research practice.[18] Thus philosophy can do no better than to adopt the strategies of inquiry that have proven successful in science, the paradigm of empirical, a posteriori inquiry. And in this case, abductive forms of inference—inference from the successes of science to a realist "best explanation" for that success—are especially to be recommended because they replicate in a philosophical context what are, on a realist account, long-established scientific forms of reasoning and explanation (Newton-Smith 1981). This argument also provides a justification for taking seriously precisely the entrenched beliefs and presuppositions that van Fraassen insists a philosopher of science must bracket. If scientific ab-

duction follows a principle of building on received knowledge, then philosophical abduction may, indeed should, take the internal understandings of its scientific subjects as its point of departure.[19]

No doubt antirealists like van Fraassen would be inclined to reject this approach out of hand, because in taking the standpoint of the practitioner as its point of departure, it aims at a kind of analysis of science that an antirealist would consider properly a part of the subject of inquiry, not of its *philosophical* explication. But from the point of view of a realist, who does not share this conception of the philosophical enterprise and who embraces a principle of charity in the philosophical explication of scientific practice, it is antirealists who must justify their radical questioning of practice. They are the ones who must give special reasons for adopting a comprehensive principle of doubt according to which we are not justified in accepting the theoretical presuppositions and conclusions of our most successful scientific practice as anything more than heuristic fictions. Realism and antirealism are thus formulated as self-sufficient research programs whose orienting metaphilosophical rationale ensures that each holds what is, in their own terms, a best or most defensible orienting conception of science.

COMPETING PHILOSOPHICAL PROGRAMS

On this assessment of the postpositivist debate between scientific realists and antirealists, the critical question to be addressed is that of what, exactly, these opposed positions have to offer as comprehensive philosophical research programs. When this issue is made explicit, the realist does have one strong objection to bring against the antirealist, a metaversion of the default argument. If antirealists are consistent in their commitment to epistemic caution—in particular, if they believe that the use of abductive inference must be called into question across the board—then their challenge undermines not only realist claims about science and many of the theoretical and epistemic commitments that are constitutive of science, but also virtually all empirical generalizations, including their own neo-empiricist claim that science is instrumentally reliable. To identify specific scientific practices as successful—even if success is construed in purely instrumental terms—re-

quires a theory-mediated inference that specific forms of success are projectable. In this respect, the claims that antirealists make about the "fitness" of scientific theory have just the same status as generalizations based on patterns of conjunction among observables: they depend on the very forms of theoretically rich inference they are meant to displace. To maintain the epistemically modest view that science aims at (and often succeeds in) "saving the phenomena," antirealists must accept that robust talk of "truth, falsity and real existence" (Churchland 1979: 2) can be preserved only at the level of empirical knowledge claims about observables and instrumental success (if it can be preserved at all), and this does not afford them the resources necessary either to explain or to sustain the scientific enterprise as they conceive it. If, instead, antirealists are fully consistent in their rejection of realist leaps of faith, they must call into question even the claims about empirical regularities and instrumental success that are central to their own account; they are then led directly to a wholesale skepticism that most are unwilling to accept. In this case, realism is vindicated by default of the inherent inconsistency of antirealism (Boyd 1981: 658).

If antirealists were prepared to carry through their commitment to a principle of doubt and embrace the consequences of its consistent application—if they were prepared to abandon their own residual realism—they could defuse the challenge of this new default argument. However rewarding research practice is and however fruitful its supporting theoretical tradition, no realist analysis of history or practice can rule out the possibility that the success of science is just a lucky miracle, albeit a miracle on which we depend in virtually all our day-to-day and scientific activities; scientific realism is unavoidably (and unashamedly) vulnerable to skeptical challenge. Antirealists could make a consistent case for rejecting the abductive inferences on which the strongest constructive arguments for realism, the miracle arguments, inevitably depend. But in the process they would abandon the apparent success of science as a possible *explanandum* for philosophically responsible analysis. The debate thus reaches a stalemate; the considerations that could settle the case in favor of one party will never be satisfactory to the other.

What remain, I submit, are pragmatic reasons for embracing a realist stance, and with them a limited version of the indispensability argument resurfaces. As Putnam argued in the early rounds of postpositivist debate about scientific realism, "there are many aims of many scientists and it is just not the case that all scientists are primarily interested in description" (1971: 72). Some researchers in some fields do regard the theories they develop as nothing more than instrumentally useful systematizing devices; despite Einstein's formative commitment to "construct a model of an observer-independent reality" (Fine 1986: 2), it has proven notoriously difficult to formulate a viable physical interpretation of quantum theory. Einstein's commitment to realism was not, Fine argues, a "cognitive doctrine" so much as a "motivational stance toward one's scientific life, an attitude that makes science seem worth the effort," and by all accounts, many contemporary physicists have not found this commitment necessary for the "meaningful pursuit" of understanding in their field (1986: 7, 9). But in a great many fields a realist stance is constitutive of research practice, not just psychologically but conceptually as well. It makes no sense to search diligently for evidence that one mechanism rather than another operates in a given biological system if the notion of an underlying mechanism is understood to be no more than an elaborate fiction; it becomes impossible to conduct this search by experimentally manipulating elements of the system if you cannot assume that the background knowledge you have of its (other) microconstituents is approximately true. As Hacking put it—referring to a series of experiments that involved spraying a niobium ball with electrons to decrease its charge, or positrons to increase the charge—"so far as I'm concerned, if you can spray them then they are real" (1983: 23).

In contexts in which realist commitments constitute the actual (albeit tentative and evolving) foundation of a research enterprise, I submit that they warrant charitable interpretation, but by no means does this entail their uncritical acceptance. How much useful explication can be expected of the theory-dependent judgments that actually inform research practice if antirealist commitments determine in advance that they must be eliminable? What ground is there for analysis of the subtle distinctions researchers make between the kinds of epistemic attitude appropriate to different theoretical propositions if you are already committed, philosophically, to the view that these

propositions are to be treated as a class (defined by contrast to observational claims) to which a single, limited epistemic attitude is appropriate? Given a defining stance of professional disengagement, antirealism is a self-consciously limited program of philosophical inquiry: its purposes are served when it has exposed the epistemic insecurities of its realist competitors in philosophy and demonstrated that the realist tendencies of practitioners are risky and dispensable.

When practitioners understand their enterprise in realist terms there is much to be gained, philosophically, by taking this stance seriously and approaching the analysis of specific research programs with the assumption that they do, in fact, seek (and sometimes deliver) well-grounded understanding of a mind-independent reality.[20] This approach does not determine in advance that philosophical analysis will vindicate these presuppositions,[21] but it does entail that philosophical analysts have a commitment, from the outset, to understand in detail and to critically assess the ontological and epistemic assumptions that inform research practice. In particular, a pragmatic realism focuses attention on the abductive inferences by which researchers draw on their own and collateral theory to build models of unfamiliar aspects of their subject domain; central questions, from this perspective, are how practitioners make discerning judgments about the plausibility of claims about theoretical entities and how they design probative empirical tests for them.[22] More broadly, the challenge a pragmatic realist faces is to give a systematic account of the conditions under which the postulates central to particular theoretical traditions warrant existential commitment as the basis for designing experiments and developing new theoretical models.[23]

Taken as a whole, such a program of philosophical analysis stands to contribute to the research enterprise itself; indeed, philosophical analysis in the spirit of a pragmatic realism is very often an extension of the self-reflective turn taken by practitioners who recognize the need for explicit, systematic, and critical appraisal of past practice as a source of guidelines for building on its successes and avoiding its failures. It promises not only to contribute to the "hammering out" of presuppositions that frame inquiry (Kim's phrase, 1980; see chapter 6, below) but also to broaden the range of experience on which reflective practitioners can draw when they assess options for inquiry within their own fields; as philosophers expand the range of research programs they study, they develop the basis for systematic comparison of the methods and theoretical initiatives that have paid off over time and in different disciplinary contexts.

When the pragmatic advantages of an antirealist stance are considered, it is clear that a program of radical questioning can serve the important creative function of countering the inherent conservatism of established research traditions, opening up the exploration of alternatives to entrenched framework assumptions. There is some suggestion in van Fraassen's metaphilosophical comments and in Hesse's advocacy of intellectual pluralism that this is a line of defense they might offer in response to the argument for realism I outline here (van Fraassen 1981; "Science and Religion" in Hesse 1980: 235–256). But a pragmatic realist can certainly raise antirealist questions about particular scientific theories;[24] the assumption that the animating goal of (most) scientific research is to establish as accurate an understanding as possible of an independent reality makes it more rather than less important to ask whether it is appropriate to take a realist attitude toward the entities or mechanisms posited by particular theories. Realists acknowledge that the theoretical presuppositions of scientific practice can never be secured with certainty against the possibility of global error, but they respond to this threat in the manner of a localized and mitigated rather than a wholesale skepticism.

In short, the difference between realists and antirealists is not that the former are limited to complacent acceptance while the latter can initiate criticism of the scientific tradition. Although an antirealist would no doubt reject pragmatic realism as unacceptably partisan and object that it reinforces unnecessary and unjustifiable epistemic commitments, it should be obvious that realists are by no means constrained to provide an uncritical apologia for the existing scientific status quo. The interests of pragmatic realists converge on those of reflective practitioners in their shared concern to enhance the scope, efficiency, and precision of scientific inquiry, a concern that reinforces rather than diminishes their commitment to ongoing, rigorously critical analysis of the presuppositions that frame research practice and its presuppositions.

Consistent antirealism about science has a good deal to offer in making clear the tenuousness of our epistemic situation. What is more, it is a critical stance that constitutes a self-sufficient, and self-warranting, philosophical program. But if philosophers are to comprehend the practice of actual science—if we are to explain both its successes and its failures, and establish the basis for a more nuanced appraisal of its claims (both theoretical and observational)—then a pragmatic realism such as that sketched here is indispensable. It is one component of the process, which is exemplified by but not limited to scientific inquiry, of refining our Peircean instincts so that we need not rely on blind trial and error in inquiry and in action. At its best, it is one way to draw lessons from experience that can underwrite informed judgments about how to proceed with inquiry and how or what to believe on the basis of its results.

Between Philosophy and Archaeology

THE END(S) OF PHILOSOPHY

The Society for American Archaeology and the Philosophy of Science Association both launched their society journals in 1934: *American Antiquity* and *Philosophy of Science*.[1] In the ensuing fifty years these societies witnessed substantial change in the identity of the disciplines they represent, and in both cases this has involved internal debates that have turned, in part, on questions about how philosophical inquiry relates to the practice and results of empirical research.

On one line of metaphilosophical debate that unfolded in the 1970s and 1980s, philosophers found themselves embroiled in internal dispute over the question of what, if anything, justifies the continued existence of their discipline as an autonomous field of inquiry. It is often observed that in the course of its history, philosophy has been displaced from one after another of its traditional subject areas by newly emerging sciences: natural philosophy gave way to a succession of physical and life sciences in the seventeenth through the nineteenth centuries, social philosophy gave rise to the social sciences in the late nineteenth century, and philosophy of mind had to contend with the coming-of-age of psychology and cognitive neuroscience in the twentieth century. The pessimists conclude, on these grounds, that philoso-

phy now holds little more than historical interest. Rorty adds the charge that professional philosophy has survived thus far by cultivating the pretension that it comprehends, in general terms, what constitutes legitimate knowledge, thereby setting itself up as a "tribunal of pure reason," a "cultural overseer . . . who knows what everyone else is really doing whether they know it or not because he knows the ultimate context [the 'neutral ground' of common epistemic standards] . . . within which they are doing it" (1979: 4, 317). This is empty posturing, Rorty insists, because there are no stable, universal standards for judging knowledge claims; all are paradigm- or context-specific and all are subject to (continual) revision. Philosophers stay in business only by reifying whatever conventional standards happen to dominate in a given cultural or disciplinary context. He concludes that in the end, small *p* philosophy (shorn of its pretensions) can do little more than mediate a conversation between disciplines that are conceptually self-sufficient; it has nothing to add of substance to this conversation, and little beyond the philosophical canon to count as its own subject domain.

This debunking of professional philosophy will no doubt find sympathy among disaffected critics of the philosophizing that has gone on in archaeology since the advent of the New Archaeology.

But before it is appropriated as justification for a general return to unreflective dirt archaeology, we should note that even if Rorty is right to condemn the myth that philosophy is "queen of the sciences," it does not follow that the end of philosophy is at hand (Baynes, Bohman, and McCarthy 1987). One of Rorty's sharpest critics argues persuasively that philosophy has an important role to play as metaphorical "handmaiden to the sciences" (Kim 1980).[2] Although, as Rorty insists, foundational assumptions can never be secured beyond doubt, they are nonetheless indispensable to all forms of practice, inquiry, and knowledge. Given this indispensability, "when a paradigm turns self-reflective, as any sufficiently mature and comprehensive paradigm should, it becomes important for the self-knowledge of its practitioners to undertake the kind of intra-paradigmatic inquiry I have indicated[,] . . . [namely] inquiry concerning the conceptual, foundational, and regulative aspects of a given paradigm" (Kim 1980: 595). There is, then, much philosophical work to be done in hammering out the working (if not the absolute) conceptual foundations of practice, even though there is no prospect of thereby delineating stable, transcontextual foundations for knowledge. Even Rorty allows that philosophers can sometimes provide "useful kibitzing" on topics to do with the nature of human knowledge; if nothing else, they know "by heart the pros and cons" of "stale philosophical clichés" that all too often punctuate intrascientific debate about the goals and limits of empirical inquiry (1979: 393).

But beyond clearing up confusions generated by (mis)appropriation of their own concepts and theories, philosophers have rich resources on which to draw in refining the conceptual framework of empirical research programs. The tools of philosophical analysis are indispensable in disembedding the presuppositions of inquiry and subjecting them to critical scrutiny. And, by extension, these tools can play a central role in helping researchers envision alternatives to conventional ways of thinking about the prospects (or limits) of inquiry. Systematic conceptual analysis gives form to emerging conventions of practice, in the process delineating the space of possibilties and opening up new directions for the development of that practice. In this regard, philosophy (the practice) may be essential to planning, even to innovation, at a strategic level. In addition, some philosophical theories offer special insights about knowledge, or about the objects of inquiry that may find direct application to practice. Such practical utility is especially likely in areas in which philosophy has a history of close interaction with science, a tradition recently reclaimed by those intent on grounding philosophy of science in the sciences in the two senses I outlined in the introduction. At its best, philosophical thinking in this engaged spirit crystallizes the accumulated wisdom of diverse research fields that have experimented with alternative frameworks for inquiry. Philosophy so conceived is, without question, an extension of what reflective researchers already do; there is no brief here for the kind of autonomy Rorty repudiates. But this loss in self-sufficiency is more than made up for by what philosophy gains, prospectively, in scope and creativity. My thesis, then, is that philosophical analysis can be an important locus of change in how empirical subjects are investigated and understood; it is one way in which research disciplines learn from their investigative experience.

THE SELF-REFLECTIVE TURN IN ARCHAEOLOGY

One persistently controversial aspect of American archaeology has been the self-consciousness of many practitioners about philosophical problems and the use they have made of philosophical literature in responding to them. Kluckhohn articulated the rationale for cultivating this connection between philosophy and practice in the critique of empiricist anthropology and archaeology that he published in *Philosophy of Science* in 1939; archaeologists could expect to escape the confines of a preoccupation with fact gathering only if they came to grips with the constraints imposed by untenably narrow assumptions about the role of theory in empirical inquiry (see chapters 1 and 2 above). Fitting later made this case retrospectively: "After World War II there was certainly a technological revolution in archaeology. . . . Archaeology put its plumbing in order, and although at that time its theory must still be found in its technique, its techniques were in the process of elaboration. . . . .But plumbing does not exist by itself. To paraphrase John Gardner, the archaeology that supports its plumbers and neglects its philosophers will have neither good plumbing

nor good philosophy. And neither will hold water" (1973: 287–288). In a parallel set of arguments, Clarke argued that the only way archaeologists stand to exercise effective control over the "direction and destiny" of their discipline is to engage in "explicit scrutiny of the philosophical assumptions [that] underpin and constrain every aspect of archaeological reasoning, knowledge and concepts" (1973a: 7, 11–12). In spite of differences on many other dimensions, these advocates of a reflective turn in archaeology shared the conviction that philosophical analysis can serve as a source not only of critical insight about the limitations of the (empiricist) assumptions underlying traditional practice, but also of constructive alternatives to these assumptions. It was in this spirit that the New Archaeologists turned to philosophy of science for general models of properly scientific practice.

As it happened, however, this appeal to ready-made philosophical solutions proved to be so divisive that many came to question the wisdom of philosophical engagement of any description. With the "derailment of the deductive-nomological (D-N) bandwagon," Renfrew observes, philosophical discussion in archaeology degenerated into a faddish and anarchic flirtation with "a rapid succession of mutually contradictory 'paradigms'": "the 'isms' of our time" (1982a: 8).[3] Not only do these "isms" hold little prospect for solving problems of practical and empirical import, they generate a polemical rhetoric that has largely diverted energy and attention from them. The sharpest archaeological critics of second-order philosophical reflection conclude that it is so inherently disputatious and inconclusive that it could never have played a constructive role in the development of a genuinely new archaeology; whatever the perils of unreflective, "innocent" forms of practice, it is preferable to empty—or, more accurately, counterproductive—philosophizing.

Published statements of disaffection with philosophical discourse in archaeology offer three distinct but interdependent analyses of the problem. The first and most fundamental objection is that the philosophical theories imported by New Archaeologists were inapplicable to concrete problems of archaeological practice or reinforced the worst tendencies of existing forms of practice, failing to broaden archaeological horizons. The second and third lines of objection suggest a di-

agnosis of this failure. The second cites limitations inherent in the models imported from philosophy: they were inadequate in and of themselves, as models of science. The third focuses attention on how these models were imported and exploited: philosophical models and modes of analysis might have been useful to archaeology but were misappropriated in ways that could not but prove counterproductive.

The first worry was raised early on by archaeological reviewers of *Explanation in Archeology* (P. Watson, LeBlanc, and Redman 1971). For example, Clarke warned against the dangers of attempting to fit archaeology to a model of science that was originally intended to make sense of physics (1972c: 238), reaffirming his commitment to internal philosophical analysis (1973a). Daniel Miller later reviewed a range of difficulties that could be seen, with hindsight, to have arisen "because archaeology is a social science and not a natural science" (1982: 85). Even such a prominent advocate of positivist models of scientific practice as Lewis Binford acknowledged their tendency, in application, to generate "trivial" results (1978: 3), while Flannery published his first scathing commentary on the philosophical pretensions of the New Archaeology—his indictment of "Mickey Mouse laws" (1973: 51; see discussion of these reactions in the introduction and in chapter 3)—just five years after an initially enthusiastic endorsement of its aims (1967). Renfrew seems to speak for a broad cross section of the discipline when he observes that the models imported were "difficult to refute but impossible to use" (1982a: 7).

Objections of the second sort were raised by a number of internal critics and philosophical commentators who felt that insufficient attention had been paid to controversy about the models themselves. Clarke's full objection, in the review cited above, is that "there is little reason to suppose that the positivist philosophy of physics is especially appropriate for archaeology—not least if, for example, it appears only weakly applicable even for biology" (1972c: 238). Indeed, there was good reason to worry that these models might be at best "only weakly applicable" even to physics. Philosophers were quick to point out that the descriptive inadequacy of logical positivist/empiricist models was a primary reason for their widely touted demise in philosophical contexts (e.g., Levin 1973;

Morgan 1973, 1974), while a number of archaeologists (e.g., Tuggle 1972; Tuggle, Townsend, and Riley 1972) made the case that other philosophical accounts of scientific explanation and confirmation might have much more to offer archaeology than the Hempelian models originally advocated by New Archaeologists.

Critics who raised the third kind of objection suggested that the New Archaeology ran aground in the transition from critical analysis to the proposal of constructive alternatives in part because of the way philosophical models were put to work (or not) in developing a conceptual framework for the new program. At best they served a heuristic and symbolic function; they provided a rallying point for the New Archaeologists, offering them inspiration and legitimating their programmatic ambitions. At worst they were a vehicle for empty rhetoric, inherently polemical and divisive. But in neither case were the resources of Hempelian positivism (or, for that matter, Kuhnian contextualism) effectively applied to the problem of articulating a viable alternative to the orienting presuppositions of traditional research. As often as not, the critics who raised these objections engaged in exactly the kind of polemical exchange they deplored. The philosopher Morgan was uncompromising in his condemnation of the "revivalist" tone of the New Archaeologists' appeal to positivism and what he took to be their general lack of philosophical sophistication (1973, 1978; see also Levin 1973). It was not lost on archaeological respondents that such self-styled philosophical authorities had generally failed to learn enough about archaeology to have anything constructive to contribute (e.g., P. Watson, LeBlanc, and Redman 1974: 125).[4] Two retrospective reviews appeared in the early 1980s—Flannery's "Golden Marshalltown" (1982) and Schiffer's "Some Issues in the Philosophy of Archaeology" (1981)—that supplement this third line of critique with a more fine-grained (if, in the case of Flannery, no less polemical) diagnosis of why the exchange between philosophy and archaeology had stalled so dramatically.

Elsewhere I have argued for a variant of the first and second critique: the particular models imported to archaeology—those associated with Hempelian positivism—were both internally flawed and, given their empiricist presuppositions, fundamentally inconsistent with the aims and insights of the New Archaeology. As such, they could not but have failed to provide useful, applicable directives for building a new archaeology (Wylie 1982c, 1989b; see in the present volume chapters 3, 4, and 7). The question remains, however, of how these inconsistencies could have arisen and persisted; as inflammatory as it has been, the third line of critique raises issues about the way philosophical theories were put to work in archaeological contexts that bear closer examination. In what follows, my aim is to come to grips with the general question of what philosophy has to offer, and what it stands to learn from, an empirical discipline like archaeology. I first reexamine philosophical critiques of the exchange between archaeology and philosophy initiated by the New Archaeologists, and then turn to Flannery's and Schiffer's retrospective assessments of archaeological philosophizing. Far from establishing that any such exchange is doomed to failure, these critiques suggest that the devil is in the details—specifically, the details of philosophical practice.

PHILOSOPHICAL IMPORTS

One potentially useful feature of Morgan's unrelentingly negative review of *Explanation in Archeology* (P. Watson, LeBlanc, and Redman 1971) is the general thesis he advances about the hazards of interdisciplinary borrowing. It is inevitably risky, he insists, to "tak[e] a technique, method, analysis from an area outside one's own specialty and attempt . . . to apply it to one's area of interest"; the least hazardous strategy of borrowing is to apply familiar aspects of "one's [own] specialty" to problems in other fields (1973: 259). If Morgan means that the degree of hazard is a function of whether the transaction is, in effect, an import or an export, his objection is surely questionable. Any import is also an export, and lack of knowledge of the subject field can be just as crippling as lack of knowledge of the source field, as Patty Jo Watson, LeBlanc, and Redman pointed out in rebuttal to his review (1974: 131). Morgan's analysis does, however, suggest another distinction that may be more useful: a difference between imports (or exports) that involve "technique[s], method[s], analys[es]" (1973: 259) and those that involve the *results* of these research techniques or types of analysis. When he stresses the unsettled, disputatious nature of philosophical debate, he makes

it clear that philosophical theories cannot be detached from the presuppositions and arguments that lend them (context-specific) credibility. Contextualist theories of science suggest that the same holds for scientific theories.[5] If this point is accepted, it follows that transferring tools or strategies of inquiry to new contexts and applying them to the problems that arise there may be less hazardous than attempting to import to new contexts the solutions they have helped to realize elsewhere as ready-made answers.

This distinction between the importability (as it were) of methods and that of results throws into relief a recurrent theme in objections of the third kind, objections to the *manner* in which philosophical models were imported and used in archaeological contexts. The loss of innocence heralded by Clarke and advocated, in other terms, by the New Archaeologists turned on a recognition that archaeologists must take responsibility for the presuppositions that underpin and in some cases constrain their practice; that necessity motivated their arguments for bringing philosophical *methods* of analysis to bear on conceptual problems that had arisen within archaeology. But the critical analysis of traditional practice could carry New Archaeologists only so far, and it was in the shift to importing philosophical results that the application of philosophical techniques faltered; New Archaeologists did not subject the models they imported to the kind of critical analysis that had given them conceptual and polemical advantage in challenging traditional archaeology. Indeed, they were quite explicit that they felt they could treat empiricist/positivist theories of science as a source of authoritative answers to the questions that concerned them about the nature of scientific inquiry.[6] Even if this use of philosophical theory were unproblematic, the difficulty remains that the questions about science that interest philosophers are often quite different from those of interest to archaeologists who must grapple with the challenge of instituting scientific modes of practice.

This disconnect between philosophy and archaeology is sometimes taken as grounds for arguing that archaeologists are justified in ignoring the details and presuppositions of the philosophical models they import; these are, properly, nothing more than a source of inspiration. This view seems to underlie Fred Plog's argument that no matter how jarring philosophers might find the juxtaposition of Hempelian models of explanation and confirmation with Kuhnian objections to empiricism, such indiscriminate eclecticism is perfectly legitimate if it is a catalyst for creative self-consciousness: "archaeologists are under no obligation to maintain the ritual purity of particular philosophical doctrines—however sacred" (1982: 28). Although archaeologists may do well to ignore many of the niceties of philosophical dispute, the "ritual purity" in question here has to do with precisely those philosophical doctrines that divide the empiricism of traditional archaeology from the avowed antiempiricism of the New Archaeology. To recommend tolerance of patent and relevant inconsistency in effect repudiates the commitment to conceptually rigorous analysis of the presuppositions of practice that motivated the turn to philosophy in the first place. And this repudiation, it would seem, undermines precisely the advantages—in clarifying the principles and assumptions of practice—that Plog himself considers the important return on an investment in exploring philosophical literature. If one starts with the claim that any philosophical theory will do, it is a short step to the conclusion that all are irrelevant.

On occasion philosophers also take the view that their insights and analyses are have nothing to offer archaeological practice. Embree (1989b) insists that philosophy is what philosophers do and archaeology is what archaeologists do: their focal problems and governing standards of practice arise from different traditions that have little in common. Richard A. Watson takes an even stronger line: the foundational, skeptical problems that are distinctively philosophical have no bearing whatsoever on the pragmatic concerns of scientific inquiry (1991).[7] In both cases, the argument for independence (or irrelevance) turns on an assumption about the sharpness of disciplinary boundaries that is untenable, particularly for a field like philosophy of science. There is considerable overlap in the broad questions that concern self-reflective scientists and philosophers of science; both ask what evidence constitutes grounds for accepting a hypothesis, what it is that makes an account explanatory, what the limits are of empirical knowledge, and what the status is of theoretical claims about unobservable phenomena. This should not be surprising; after all, the philosophical analysis of science was initially provoked by the second-order problems that gave rise to

new sciences and have since engaged practitioners when, as in the case of archaeology, they have taken a reflective turn. Although analytic philosophers of science defined and institutionalized a sharply drawn division of intellectual labor in the years after *Philosophy of Science* published its inaugural issue, this break with earlier science-grounded traditions of practice has been decisively reversed by recent naturalizing trends. The institutional boundaries that Embree and Watson treat as insurmountable are at least permeable if not, by now, seriously eroded.[8]

Nonetheless, differences remain that make a difference in how philosophers and archaeologists (for example) approach the problems they share. One way of stating what these come to is suggested by Wesley Salmon (1983) when he examines the role played by pragmatic considerations in explanation. The central point of pragmatic theories of explanation (as proposed, e.g., by van Fraassen 1980 and Garfinkel 1981, and elaborated by Risjord 2000: 660–671) is that explanations are answers to "why" questions; what counts as an explanation depends on how the question is formulated and on the way it is understood in a given context.[9] Context-specific interests in the target of explanation determine what exactly requires explanation, and presuppositions about the nature of the target determine what sorts of factors will be considered relevant for resolving those puzzles. In Garfinkel's now canonical example, the robber Willie Sutton took a journalist's question —"Why do you rob banks?"—to be a question about his choice of banks over other possible targets for robbery; he answered, "Because that's where the money is" (Garfinkel 1981: 22–25). Clearly the journalist and Sutton found different aspects of Sutton's criminal activities puzzling; each presupposed a different "contrast space" of alternatives against which explanatory salience was defined. Even when questioners focus on the same features of the explanatory target, their responses may diverge. A psychologist and an economist might agree that the fact, not the expression, of Sutton's criminality is at issue. But the economist might be more inclined to consider by the unemployment statistics for males of Sutton's socioeconomic background an explanatorily relevant factor, while the psychologist would focus attention on the specific psychosocial conditions that led Sutton, among his impoverished peers, to choose crime rather than a life of working poverty or welfare. By analogy, archaeologists and philosophers may share an interest in questions about scientific practice; but if the motivations and presuppositions behind these questions differ, then what they find puzzling and what they consider relevant to an explanatory answer will diverge. As Salmon puts it, the moral is "Ask a philosophical question and you may get a philosophical answer" (1983: 10).

The pragmatics of explanation suggest a strategy for coming to grips with what it is that distinguishes a philosopher's question about science from the second-order concerns of scientists themselves. Two related areas are relevant: motivating interests and the assumptions that inform the request for an explanation. Salmon focuses on differences of interest when he observes that philosophy, like mathematics, is often distinguished by its concern with abstract problems that may have little immediate practical import: "it is often impossible to tell in advance what concrete applications, if any, will result from such endeavors" (1983: 11). Insofar as philosophers working in the tradition of "received view" empiricism/positivism focus on highly abstract questions about the formal structure of the language of science (as recommended by Carnap 1934), they cannot be expected to provide much insight into principles that govern practice in specific fields of inquiry. This orientation has been sharply contested, however, as contributing little even to the *philosophical* understanding of science. The postpositivist commitment to ground philosophical analysis in concrete understanding of particular sciences narrows the gulf between the standpoint of the philosopher and that of the practicing scientist; the contrast space in which philosophical explanations are formulated may still diverge from that of the practitioner, but not as a matter of categorical difference between the focal questions that count as philosophical and those that count as scientific. As this tradition of practice evolves— turning from a preoccupation with the most abstract and universalizable dimensions of scientific practice to its particulars—it is to be expected that philosophers will increasingly produce detailed, discipline-specific analyses of science that engage questions that arise in practice much more directly than did the theories generated by their positivist predecessors.

Moreover, as more attention is given to the details of science practice, the likelihood diminishes that philosophical models will be predicated on presuppositions about the nature of science that diverge sharply from those of practitioners. No doubt there will continue to be deep and contentious disagreement about fundamental assumptions, but it is as likely to arise among practitioners as along disciplinary lines, between practicing scientists and philosophers. As empirically grounded studies of science have developed, the facts about scientific research have proven to be much more complex and ambiguous than most philosophers had appreciated, but they remain robust enough to impose significant constraints on what can reasonably be claimed about the sources, stability, and limits of empirical knowledge. Certainly, the unreality of positivist theories—the flaws that compromised them both as general models of science and in application to fields like archaeology—are less apt to arise or be sustained when the standards of adequacy governing philosophical analysis include not only requirements of internal coherence but also fidelity to historical and contemporary realities of scientific practice.

In short, the disjunction between philosophy and archaeology identified by the most uncompromising critics of archaeological philosophizing is by no means inevitable or irreparable. The theories of science that the New Archaeologists imported in the 1970s and 1980s proved to be problematic in application to archaeological problems for contingent reasons: they were descriptively inadequate and did not deal with scientific practice at a level, or in the kind of concrete detail, that could have yielded useful methodological directives. As serious as this flaw is, it does not support a general argument against importation along the lines suggested by Morgan, nor does it establish Embree's or Watson's conclusion that philosophical answers are categorically irrelevant to archaeologists' second-order questions about the nature of science. These critics do make it clear, however, that if archaeologists are to make effective use of philosophy as a source of constructive guidelines for conceptually reframing research practice, they must be discriminating in what they import. They must be sure that their questions and those addressed by philosophers pertain to the same subject and that what philosophers find puzzling about this subject is similar enough to the

concerns of archaeologists to ensure the relevance of philosophical answers to archaeological questions. Moreover, they must consider the soundness and commensurability of the presuppositions that inform the questions that philosophers, as opposed to practitioners, ask about science (it is here that questions about philosophical pros and cons and about empirical adequacy arise). Careful attention must be paid to the details of the arguments and assumptions that make up the context in which philosophical questions are framed and answered. And in this case there is greater rather than less need to promote, within archaeology, a tradition of disciplined second-order inquiry in which philosophical methods of conceptual analysis are systematically applied to archaeological problems.

INTERACTION PATTERNS

Although Schiffer (1981) and Flannery (1982) appear to take opposite sides on the question of whether philosophy (in any form) can play a useful role in archaeology, on closer inspection their analyses reinforce the conclusion that what matters is the form that philosophizing takes; philosophical techniques of analysis are themselves one of the most valuable resources archaeologists can import from philosophy, and they are the key to using substantive philosophical models effectively.

Despite their differences, both Schiffer and Flannery explain the failings of previous exchanges between philosophy and archaeology in terms of a counterproductive "interaction pattern" (Schiffer's term, 1981). Schiffer sees the culprits as external philosophical commentators like Morgan who responded not only critically but also condescendingly to "flawed analyses and misuse of [philosophical] concepts" in the archaeological literature (1981: 899). On Flannery's analysis, the problem lay with an internal commentator elite whose theorizing was, in his view appropriately, the object of the philosophers' derision. Both identify a structural imbalance that arises whenever a group of commentators emerge who purport to control the standards and ideals that practitioners in the field must strive to realize. Schiffer objects that at least some of the philosophers who entered archaeological discussions could be accused of opportunism; they scored easy critical points but they never came to grips with the second-order

problems that led archaeologists to the philosophical literature in the first place (see n. 4). Flannery derides the opportunism of internal commentators who criticized the conceptual blunders of colleagues struggling with the intransigent practicalities of actual research. These commentators exploited a contentious situation, achieving a maximum of professional exposure with a minimum of scholarly labor and, most important, with virtual immunity to peer criticism. This last point is crucial: because archaeologists lack confidence in their own judgment (or, more relevantly, because they lack grounds for rebuttal), they have tended, Flannery argues, to believe anything put before them by the commentary elite (1982: 277), especially when it is mystified as philosophical wisdom. There was little incentive for the internal commentators to learn much about the philosophy they appropriated; given this institutionalized structure of rewards and (lack of) constraints, it proved all too easy to indulge in unconstructive and self-serving polemic.

In their recommendations, Flannery and Schiffer both urge archaeologists to institute what amount to checks and balances that will enforce intellectual accountability, but there the similarities end. Schiffer urges philosophers to take a more sympathetic interest in archaeology, to engage archaeological debate in ways that will rectify imbalances in the "flow of information and argument" that fostered the unhelpful tenor of past discussion. In this connection he emphasizes the value *for philosophers* of better-informed, constructive analysis of archaeological practice, while at the same time reaffirming his conviction that when "problems of a philosophical nature . . . arise in the course of investigation" (1981: 901), philosophers may have much to offer archaeology. By contrast, Flannery applauds the sharp retort of professional philosophers to the commentator elite; they did what archaeologists (for the most part) could not do in challenging their peers' pretensions. He recommends that archaeologists give up their preoccupation with philosophical issues altogether and return to what they do best: archaeological fieldwork and the reconstruction of culture history.

There is, nonetheless, much in Flannery's position that supports Clarke's conviction that "the growth of archaeology depends on the vigorous and explicit development of archaeological philos-

ophy and theory" (Clarke 1972c: 239). In the dialogue he constructs as a "parable for the archaeology of the 1980s," the Old Timer observes that archaeology, like football, is fundamentally a game of strategy and he suggests that great innovations at the level of strategy have transformed the game in recent years. But, he argues, innovations come from veteran players and coaches who fully appreciate the practical demands and limitations of the game, not from self-serving commentators who watch from the sidelines. Flannery builds on this analogy to suggest that archaeological practice will grow in effectiveness only through constant assessment and innovation at a strategic level. Attention must be paid to what he calls "game plans (or 'research designs,' if you will), and what are called differing philosophies" (1982: 271). Far from offering an unequivocal condemnation of second-order (philosophical) reflection on the presuppositions of practice, Flannery seems to share the view that such reflection is indispensable so long as it is well-grounded in the analysis of actual practice and substantive problems.

Where Flannery and Schiffer crucially disagree is on the question of *who* should do this philosophizing. While Schiffer argues for the fuller involvement of philosophers, Flannery rejects all potential commentators but those who have emerged as senior practitioners within the field. It would seem, however, that although coaches and veterans who can draw on their experience have much to offer, those new to the field and those with external training may be most likely to bring a fresh perspective to bear on long-standing internal problems. Strategic analysis is a matter of raising second-order questions about how practice might best proceed, a process that requires an ability both to disembed and to see beyond assumptions that may be invisible to insiders. Philosophical skills of analysis and familiarity with a range of philosophical theories about science can be invaluable in this connection. Although the Salmons consider philosophers' analyses of science extrinsic to the practice of science, they argue that it can sometimes be useful to practitioners for just that reason: "As John Venn, a 19th-century philosopher, wrote in his epoch-making work on probability (1866): 'No science can be safely abandoned entirely to its own devotees. Its details, of course, can only be studied by those who make it their special occupation, but its general principles

are sure to be cramped if it is not exposed occasionally to the free criticism of those whose main culture has been of a more general character'" (M. Salmon and Salmon 1979: 72).[10]

But whoever takes on the task of reflective strategy-building in archaeology, it is clear that their analyses will be of little value to anyone unless they are grounded in direct consideration of practice, in the spirit of what both archaeologists (Clarke 1972c, 1973a) and philosophers (McMullin 1970) refer to as "internal" philosophies of science; on this Flannery and Schiffer are agreed.[11] In addition, Flannery and Schiffer emphasize that unless a set of scholarly standards and a tradition of *critical* self-consciousness is established, reflection on the aims and presuppositions of practice cannot be expected to provide researchers with any significant practical advantage. In short, far from establishing that the proponents of philosophical self-consciousness were misguided, Flannery's and Schiffer's very different criticisms reveal that there is now greater need for philosophical analysis than ever before.

Several useful points can be extracted from critiques of the philosophical turn taken by contemporary archaeologists. First, there is a distinction to be drawn between philosophy of science considered as a form of inquiry—namely, second-order, conceptual analysis of the aims, assumptions, and methodological standards that govern some area of scientific research—and the results of such inquiry. The main target of criticism in the debate about the relevance of philosophy to archaeology has been the use of philosophical results. Few deny the value of philosophy in the former, methodological sense, except when they identify philosophical analysis exclusively with particular theories propounded by philosophers, and slide from criticism of specific theories to a blanket rejection of philosophical concerns and forms of analysis.

Second, although many profess a yearning for an earlier, simpler Clarkean state of innocence, controversy over the presuppositions of traditional forms of research make this impossible.[12] The presuppositions of research—its aims, regulative (methodological and epistemological) ideals, and orienting theories—are now contentious subjects of debate. Choices must be made; and unless they are to be endorsed dogmatically, they require systematic formulation and reasoned defense. There is, in effect, no unself-conscious, self-contained tradition to which to return (if there ever was one). Moreover, the preference for explicit and closely argued conceptual foundations is not simply a matter of scholarly taste; the experience of both traditional and New Archaeology bears out Kluckhohn's and Clarke's conviction that the coherence, sophistication, and plausibility of these presuppositions do affect research practice. Taken together, these considerations suggest that second-order philosophical analysis is an indispensable part of the discipline of archaeology.

Third, the critics of recent philosophical misadventures identify a number of pitfalls that must be avoided if the interchange between philosophy and archaeology is to be fruitful. They make it clear that philosophical and archaeological interests may differ enough that answers to philosophical questions about science will not be directly transferable to archaeological contexts, even when the same questions seem to be at issue. But the existence of such a disjunction does not establish that philosophical results are *categorically* irrelevant to archaeology. As Rorty would allow, even the most abstract and traditional philosophical theories of science may be a useful resource for understanding the implications of, or alternatives to, conventional epistemological assumptions that figure in archaeological debates. In addition, these theories vary considerably in their level of abstraction; the practice-grounded analyses typical of postpositivist philosophy of science are much more likely to engage the questions that arise in archaeological practice than were their antecedents. In any case, the details of the arguments that constitute second-order theories of research practice are of the essence; whether built internally or imported from outside archaeology, they will be useful only insofar as they are internally coherent and accurate in what they claim (or assume) about research practice. A willingness to engage debate about first principles is not sufficient on its own to ensure disciplinary growth. The process of hammering out conceptually robust foundations for practice requires the cultivation, in philosophy, of a discipline of empirical analysis that was marginalized by logical positivism; in archaeology, it requires a discipline of conceptual analysis that is taken as seriously as the skills of empirical inquiry that are the field's defining core.

Interpretive Dilemmas

Crisis Arguments in the New Archaeology

Whether or not the New Archaeology was revolutionary, by the early 1980s friends and foes alike were referring to its rapid rise to prominence a decade earlier as a watershed that had substantially shifted the terms of internal debate. Some were intent on exploiting the possibilities opened up by the New Archaeology, while others regretted what they saw as a failure to realize its promise; still others yearned for the halcyon days of lost innocence and were actively working to undo the damage of upheaval. Those committed to the ideals of the New Archaeology were grappling with the implications of internal tensions and contradictions (identified in chapter 4) that had begun to emerge a decade earlier, as these were becoming the target of post- and antiprocessual critique. The essays included in this section were all originally written in this period of growing crisis; they reflect a conviction that the hoped-for revolution had barely begun by the time it was declared over.

My aim in these essays has been to develop a systematic account of what I take to be the substantive core of the New Archaeology: the constructive insights about evidential reasoning and the prospects for using it to set explanatory models of the cultural past on a firm(er) empirical foundation. To develop such an account it is necessary to rethink the rejection of inductive forms of inference by which New Archaeologists marked their commitment to scientific modes of practice and their opposition to traditional archaeology. In particular, analogical inference is as indispensable in evaluating as in formulating hypotheses; I develop this argument in chapter 9, "The Reaction against Analogy." Moreover, a reliance on analogy and related forms of inference does not necessarily open the floodgates to speculative excess. I find, in less prominent aspects of the New Archaeology program (in its practice rather than its programmatic statements), a number of promising strategies for responding to this worry; these are initially identified in chapter 7, "The Interpretive Dilemma."

Properly understood, they give purchase not only on the questions central to an eco-materialist perspective but also on some that lie well outside the ambit of the New Archaeology: questions about the symbolic and structural dimensions of the cultural past (considered in chapter 8), and about the presentist assumptions and interests that underlie archaeological interpretation (addressed in chapter 10).

Although interesting interpretive claims invariably fall short of certainty, they are not all equally and radically insecure; in the larger context of philosophical and transdisciplinary debate about objectivist ideals, as Bernstein (1983) argues, there are options that lie "beyond objectivism and relativism." In chapter 11, I use archaeological variants of these options to illustrate and refine some of Bernstein's suggestions about inferential practices that can realize them. The interpretive dilemma negotiated by archaeologists is by no means unique, and archaeological practice suggests a generalized method of exploiting diverse sources to build multiple lines of evidence that has relevance well beyond archaeology.

7

The Interpretive Dilemma

When the New Archaeologists undertook to operationalize a solution to the interpretive dilemma inherent in traditional archaeology, they vacillated between two interconnected strategies for securing interpretive inferences; to extend a figure used by Patty Jo Watson (1979), they worked both on the source side and on subject side of the inferential equation, despite periodically repudiating one in favor of the other. Their first impulse was to act on the positivist conviction that the sources of an interpretive hypothesis, the considerations that play a role in the "context of discovery," are ultimately irrelevant to its justification as a credible account of its archaeological subject in the "context of verification." On this account, rigorous archaeological testing must be the basis for accepting or rejecting claims about the cultural past, and should supersede any appeal to the source-side considerations that played a role in their formulation (e.g., assessments of analogical strength or prior plausibility in light of accepted theories about cultural phenomena). This emphasis on testing was a recurrent theme in Lewis Binford's earliest proposals for a new archaeology: the priorities of traditional practice must be inverted so the emphasis is on archaeological testing, not post hoc plausibility, as the final arbiter of interpretive adequacy (1967: 11).

Within a decade, however, even the staunchest advocates of processualism began to express misgivings about the viability of a strictly deductive testing program; all too often the results were either trivial or manifestly uncertain.[1] Binford laid the blame for this debacle at the door of the "lost second generation" of New Archaeologists, who, on his account, had never appreciated the limitations of the testing procedures he had recommended (see, for example, his introduction to *For Theory Building,* 1977b). In his diagnosis of why testing failed to pay off as expected, Binford appeals to a contextualist line of argument that had been another central plank in the New Archaeology platform: facts of the record do not have clear and unambiguous "meaning" (1978: 1); they tell for or against a test hypothesis only under interpretation, and in this regard they are themselves interpretive hypotheses.

Originally this argument was cited as cause for optimism: with sufficiently rich and sophisticated theoretical resources, the impoverished data of the archaeological record could provide access to virtually all aspects of past cultural systems. It was intended to undercut an intractable dilemma that had confined traditional archaeology to two unpalatable options (see the beginning of chapter 3 for an earlier formulation of this dilemma). Given an implicit commitment to empiricism—specifically, the thesis that the legitimate content of any

knowledge claim is limited to the observations from which it is derived or against which it can be verified—archaeologists seem trapped: either they must limit themselves to a kind of "artifact physics" (DeBoer and Lathrap 1979: 103), venturing little beyond description of the contents of the archaeological record, or, if committed to anthropological goals, they must be prepared to engage in the construction of speculative just-so stories as the only means available for drawing interpretive conclusions about the cultural past. Kuhnian insights about the theory-ladenness of all observation reopened underlying questions about the limitations of the evidence that might be extracted from the archaeological record, but they also sharpened this dilemma. Because, as a matter of (contextualist) principle, no empirical description is interpretively innocent, the epistemically conservative horn of the dilemma must be regarded as a false option; the prospects for escaping the dilemma thus lie in strategies for setting interpretive inference, the option that constitutes the second horn of the dilemma, on a firm foundation.

By the early 1980s Binford turned from analysis of the implications of this Kuhnian point for traditional archaeology to drawing out its implications for the burgeoning New Archaeology. He objected that many of the "lost generation" had failed to understand the extent to which all research and all knowledge claims are, as he and Sabloff put it, paradigm-relative (L. Binford and Sabloff 1982). What Binford acknowledges, with this internal critique, is that Kuhnian objections to the very idea of empirical foundations apply as much to his own deductivist testing proposals as to the inductivist empiricism of traditional archaeology.[2] If the data against which archaeological hypotheses are to be tested (on the subject side of the equation) have significance as evidence only under interpretation, then they cannot be treated as an autonomous, theory-independent empirical foundation for evaluating interpretive hypotheses. The arguments of relevance that establish evidential significance in the context of verification are not categorically different from, and more secure than, the evidence that archaeologists bring into play when they formulate hypotheses and make judgments about their initial plausibility (on the source side of the equation). Binford accepted this point as early as 1977 when, in *For Theory Build-*

ing (1977a), he discussed the interpretive paradox created by the fact that "the scientist must use conceptual tools to evaluate alternative conceptual tools that have been advanced regarding the ways the world works" (1977b: 3). He later appealed directly to Kuhn, observing, with Sabloff, that "nature does not dictate the meanings we assign to it. . . . [W]hen we seek to explain nature through theories we are seeking to explain our conceptualization of nature, rather than some objective 'true' nature" (L. Binford and Sabloff 1982: 138).

Consistently maintained, this line of argument implies that archaeological testing is conceptually "locked in" (L. Binford 1981b: 29); if the theoretical presuppositions that underlie explanatory reconstructions of the past also inform the interpretation of the archaeological data used to test them, then testing is threatened by a vicious circularity. In retrospect Binford and Sabloff describe this circularity as an inescapable feature of the testing program initiated by "second-generation" New Archaeologists in the late 1960s and 1970s;

> Objectivity was not attainable either inductively or deductively. . . . [O]ne's observational means for conceptualizing experience were rooted in one's paradigm. The testing of theories was thus an illusion, ultimately bound by paradigmatic subjectivity. (1982: 138)

> Archaeologists of the late 1960s and early 1970s who argued for the potential of deductive procedures had not fully thought through the problem of the dependent status of their ideas regarding the past. Archaeological knowledge of the past is totally dependent upon the meanings which archaeologists give to observations on the archaeological record. Thus, archaeologically justified views of the past are dependent upon paradigmatic views regarding the significance of archaeological observations. (149)

In short, Kuhnian considerations seem to entail that subject-side work could never be expected to provide, on its own, a resolution of the interpretive dilemma that lay at the heart of traditional archaeology—a legacy, the New Archaeologists had argued, of its implicit empiricism. Binford faulted those who heeded his call to implement a rigorously deductive methodology for turning to archaeological testing too soon; their first priority should have been to establish properly scientific linking principles—a "Rosetta stone" for reliable archaeological code-breaking (1982b: 129)—

capable of securing the interpretation of archaeological data so they could serve as a credible basis for subject-side testing. On this reassessment, the first priority for those who are committed to a positivist testing program must be to secure the bridging principles—the laws necessary for low-level explanations, on John Fritz and Plog's analysis (1970)—that support retrodictive ascriptions of meaning to archaeological data; these must be made explicit and their credibility established on empirical rather than merely conventional grounds.

SOURCE-SIDE RESEARCH: THE PRINCIPLE AND THE PRACTICE

Among those who took up the challenge of developing a robust program of source-side research, there was disagreement from the outset about what, exactly, would be required by way of background knowledge to set evidential claims on a firm foundation. The positivist model of explanation invoked by Fritz and Plog and by Schiffer (see chapters 3 and 4) establishes at least one commonly accepted point: that the source-side knowledge necessary for credible explanation (at any level) must take the form of well-confirmed, widely applicable generalizations about the relationships that hold between material culture and more ephemeral aspects of cultural systems. This demand was the basis for Tringham's objection to particularistic forms of experimental and ethnoarchaeological research (1978: 177). It also informs Schiffer's concern to stimulate work on "general questions" (1978a: 232) and Hole's insistence that "it may be voguish and even fun to go out and watch people doing things but these factors do not ensure that the results of the observations will necessarily be useful in archaeology" (1979: 197). By 1981 Binford observes that "the point that we must use general principles in giving 'historical' meaning to our observations no longer seems at issue" (1981b: 23).

But there remained significant differences over the question of what *kind* of general knowledge archaeologists would need to secure claims about evidential significance, and here there are close parallels with the debate about models of explanation outlined in chapter 4. The central tenets of Hempelian positivism suggest that the principles covering low-level explanations of archaeological data should take the form of universal (or statistical) generalizations that capture invariant regularities: constant conjunctions among observables. In this spirit Schiffer recommended that archaeologists develop a corpus of "general statements" that "relate two or more variables without regard to time or place" (1978a: 233). At the same time, a number of others objected that "general facts" —propositions that merely describe contingent associations among variables—cannot secure explanatory understanding, no matter how well confirmed they may be. Only processual *laws* that capture genuinely causal relations among variables can provide the basis for reliable retrodictive inference. Thus, the kind of general knowledge Hole urged ethnoarchaeologists to produce is an understanding of "the underlying principles of behavior," "the more timeless essentials" (1979: 203, 212) that are instantiated in the particular, idiosyncratic forms of life accessible to research in the ethnographic present. In a similar vein, Tringham (1978) argued that the most valuable thing experimental research has to offer archaeologists is an appreciation of the causal connections that hold between archaeological features and their postulated antecedents. Gould likewise insists that archaeologists must base interpretive inference on uniformitarian propositions that "posit . . . *necessary* relationships between the various kinds of observed evidence" rather than on correlations ("resemblances" or "interesting coincidences") that may be accidental and, therefore, may not hold beyond the observed context (Gould and Watson 1982: 374, emphasis added; see also P. Watson 1980, 1982). Despite having endorsed Hempelian covering law models in his early work, by 1981 Binford explicitly rejected their most prominent applications to actualistic research. He was sharply critical of Schiffer's proposals for middle-range research, which he thought "fail[ed] to make the critical distinction between description and explanation" (1981b: 27). And he was similarly impatient with Yellen's ethnoarchaeological study of the !Kung (1977), which he found wanting because the correlations Yellen documents between "consumer variables"—such factors as camp size, density and distribution of materials, and the attributes of the consumers themselves, such as the number of people and the length of their stay at a particular site—provide no understanding of the underlying causal conditions responsible for the

behaviors observed and their material signatures (L. Binford 1978: 359).

What concerns Binford in these cases is that a preoccupation with "general facts" threatens to reproduce, in the actualistic research undertaken by New Archaeologists, all the limitations inherent in traditional archaeology. Documenting regularities in ethnohistoric or experimental settings provides no basis for inference beyond observed cases; it simply *raises* the interesting and potentially informative questions of why the observable patterning occurs when and as it does. Only when these more fundamental causal questions are answered will it be possible to determine with any confidence the range of antecedents that would be capable of producing the archaeological record and which among those may (or must) have existed in the past. What ascriptions of evidential significance require is just what archaeological explanation requires more generally on Binford's modeling approach to explanation (see chapter 4): an understanding of the causal mechanisms responsible for manifest regularities and their instances. This requirement, however, generates a familiar conundrum. If, as a strict positivist would maintain, knowledge of contingent relations of dependence between observables is all we can establish without risking the insecurity of a speculative "detour" through the realm of unobservables (Hempel 1958), then source-side knowledge of present contexts, however extensive, can provide no reliable guide to the past; Hume's problem of induction is inescapable. In this case the original interpretive dilemma reasserts itself, albeit in the restricted form entailed by Kuhnian contextualism: the "artifact physics" option can no longer be understood to offer an escape from interpretive speculation. Binford and those who share his processualist commitments[3] claim, by contrast, that the investigation of contemporary contexts *can* provide an understanding of underlying causal relations and processes that can be expected, by virtue of the necessity of connection they embody, to have held in the unobserved past. It is this profoundly nonpositivistic conception of causal knowledge that underwrites Binford's conviction that the interpretive dilemma can be escaped; archaeological data may be a thoroughly theory-laden construct, but if the ladening theory establishes the right kind of (causal, deterministic) links between behavioral antecedent and

material effect, the resulting facts of the record can serve as robust test evidence. Thus the task Binford confronts is to hammer out a viable, nonreductive conception of causality and a set of practical proposals for establishing the causal knowledge necessary to secure low-level ascriptions of cultural meaning to archaeological data.

The question of how source-side research might be used to resolve the interpretive dilemma was a central concern of Binford's when he presented the results of his Nunamiut research (1978). He was at pains to establish that his account of Nunamiut practices offers the kind of explanatory understanding that inductivist researchers like Yellen had consistently failed to provide. To develop a genuinely explanatory account, he argued, Yellen would have to move beyond simply redescribing !Kung behavior in normative terms, as highly stable and invariant in its conformity to cultural convention, and construct a *theory* that explains the fact of this stability—by contrast, for example, with the striking variability of comparable Nunamiut practices. Binford's ecomaterialist account focuses attention on the underlying "input and entropy variables"—the environmental constraints and associated logistical considerations—that, he argues, structure the decision making of both the !Kung and the Nunamiut. The practice of !Kung butchers exhibits stable regularities, Binford suggests, because they operate in an environment where the resources they exploit are relatively plentiful and reliable; their Nunamiut counterparts must continually adjust their behavioral strategies to deal with highly changeable conditions in which resources are often scarce and insecure.

Although the general directive is clear—explanatory understanding requires archaeologists to develop a robust theoretical understanding of the causal factors that structure human behavior under specified conditions—its implications for the !Kung case are less obvious than Binford suggests. Yellen's analysis does not, in fact, lack theoretical underpinnings. On Binford's own account, Yellen presupposes a normative conception of human agency and culture according to which human behavior and its material consequences are a product of the norms that constitute a particular cultural context; these local, contingent features of the cultural lifeworld are the primary causes of variability to be cited when explaining behavioral

regularities under this paradigm. The real thrust of Binford's critique is not that Yellen fails to develop or engage a sufficiently rich theory of cultural phenomena but that he should have considered a *different* set of theoretical postulates.[4] As Binford elaborates this point, however, his central objection to the normative paradigm implicit in Yellen's account of the !Kung is not that it is false or irrelevant to the case, but that it threatens to foreclose the kinds of explanatory questions about *why* observed regularities hold that Binford considers salient if source-side research is to provide a secure foundation for archaeological inference. If cultural traits are understood to be arbitrary conventions—if they and their observable (behavioral, material) manifestations result from culturally conditioned patterns of preference among human agents—then by their nature there is no explanation to be given of them beyond a description of the conventions they embody and the particular cultural contexts in which they obtain. As with traditional archaeology, Binford's concern is that when Yellen relies on this (normative) conception of culture he capitulates to the interpretive dilemma; conceived in these terms, cultural phenomena cannot be expected to reveal the kind of causal constraints that, properly understood, could serve as an intellectual anchor for secure retrodictive inference from archaeological data to past conditions of cultural life.

But the worrisome methodological implications of a normative paradigm do nothing to establish that it is false. It is an open and empirical question whether human, cultural phenomena are in fact causally conditioned in a way that could underwrite the kind of security of inference Binford requires; in some contexts, to varying degrees, human behavior may well be structured by conformity to tradition-specific norms, and these conventions may be sufficiently unstable and inscrutable that archaeological inference is rendered unavoidably insecure. The burden of proof is on Binford and those loyal to the processualist cause to show that the normative paradigm is substantially wrong in what it claims about human agency and cultural phenomena and that, by contrast, causal constraints of the kind they posit do structure collective human behavior fundamentally and pervasively. Although Binford and Sabloff insist that their (and other New Archaeologists') primary concern had always been to make a break with tra-

ditional archaeology, not to argue "for the adoption of any particular alternative" (1982: 148), it is clear that an overriding interest in escaping the interpretive dilemma leads Binford, at least, to consider viable only those theoretical options that construe human, cultural behavior as a tractable subject for the kind of scientific investigation he endorses.

As a result, Binford faces the difficulty of justifying commitment to a particular paradigm— the eco-materialist, processual paradigm—over its alternatives, especially those that emphasize the contingent, normative dimensions of cultural life. If Binford is serious about the lessons he draws from Kuhn, it is not clear how he can accomplish this. If, as Binford and Sabloff argue, paradigms are sufficiently all-encompassing that testing is unavoidably paradigm-dependent—"locked in"— then empirical evidence can only be used to refute the commitments of one paradigm when interpreted in the terms afforded by an alternative paradigm. On the strong form of Kuhnian contextualism that Binford and Sabloff affirm, evidence, qua interpreted experience, cannot provide a neutral, extra-paradigmatic standpoint from which to judge the adequacy of competing sets of presuppositions about "the way the world is." In that case it seems unavoidable that Binford's efforts to avoid naive empiricism must yield a new version of the interpretive dilemma.

In its contextualist form, the interpretive dilemma afflicts not just the use of archaeological data as evidence for testing hypotheses about the cultural past, its primary (subject-side) locus in traditional archaeology, but also any source-side evaluation of the background assumptions, ranging from low-level linking principles to comprehensive paradigms, on which archaeologists rely when they ascribe evidential significance to archaeological data. This is an interpretive dilemma raised to a second order: the theory that ladens archaeological evidence is as paradigm-dependent as the evidence itself. In addition, as I suggested above, there is a sense in which, strictly speaking, this new dilemma is no longer *di*lemmic. Contextualist arguments decisively eliminate "artifact physics" as a viable form of practice (the horn of the dilemma embraced by the conservative wing of traditional archaeology); consequently, the only option open to intellectual conservatives and radicals alike seems to be one or another variety of paradigm-informed speculation.

RESOLUTION OF THE NEW INTERPRETIVE DILEMMA

Binford has been just as confident that the challenges posed by the new interpretive dilemma can be met as he was that the traditionalists' dilemma could be circumvented. He insists, with Sabloff, that "we may accept Kuhnian insights regarding the importance of paradigms and their impact on our ideas of objectivity without accepting his particular approach to paradigm change" (L. Binford and Sabloff 1982: 139). Although scientific inquiry is thoroughly paradigm-governed, large-scale paradigmatic change need not be a matter of arbitrary conversion from one self-contained schema for structuring and explaining experience to another, governed by external (noncognitive) sociological and historical factors rather than by internal (cognitive and empirical) considerations of evidence and logical implication. By no means are the relativist conclusions drawn by postprocessual critics inescapable; their "statements of . . . paradigmatic bias," their "posturing," is just a form of fashionable defeatism, rooted in a weakness of epistemic will rather than the inexorable logic of Kuhnian analysis (L. Binford 1982b: 125, 134). In fact, Binford and Sabloff claim, "archaeologists today are in an excellent position to show how such rational paradigm growth can be achieved" (1982: 139). But insofar as they are able to make good the claim that paradigms can be systematically evaluated, it would seem they undercut the contextualist premises that give rise to Binford's critical arguments against both traditional archaeology and "second-generation" New Archaeologists.

In many contexts in which Binford defends the eco-materialist paradigm against contemporary critics he indulges in just the kind of "pseudo-scientific" modes of paradigm debate he says he abhors (1982b). Sometimes he claims that eco-materialism is self-evidently true and normativism obviously false; he likens the normativist conception of cultural phenomena to a "paradigm [that] leads us to consider the earth as flat" and asks why we should "waste . . . time and energy in testing our theories as to why the earth is flat [when] . . . we could just as well learn, through our search for objective means of evaluating our ideas, that the earth [is] round?" (1983: 137).[5] But here he follows the pattern of argument I described earlier (chap-

ter 3): rather than identify the body of evidence that demonstrates the manifest superiority of eco-materialist assumptions—the equivalent, in this debate, to sailing around the world—he resorts to ad hominem arguments against his opponents who, he objects, are opportunists and obstructionists, unable or unwilling to recognize that a "functional approach viewed in systems, not psychological terms" is the most productive approach to inquiry open to archaeologists (223). He declares, for example, that "it is a false paradigm that treats as extra-natural the human sociocultural experience and that already claims as a failure those scientific methods that, in general, have never been implemented" (137). Although he seems to intend "false" in an empirical sense, given the analogy with "flat earth" theories, the only argument he offers for this conclusion is that normative theories are false in the way that prophets and political visionaries are false: they call for faith in a form of archaeological practice that cannot deliver what it promises. An eco-materialist paradigm is to be preferred because it will sustain the sort of scientific program of inquiry to which he is committed; it offers a view of the cultural subject as just the sort of materially determined system that archaeologists can reasonably expect to reconstruct with scientific reliability from its material "exoskeleton." This claim may justify tentatively accepting eco-materialism as a working hypothesis, but it does not settle the question of its credibility as an account of the subject domain, an inferential slide that Binford often makes when defending eco-materialism against normative theories and postprocessual critics.

In less polemical exchanges, in which Binford focuses on the application of processual assumptions to particular problems, he makes effective use of two strategies of inquiry that do hold considerable promise for meeting the challenges of the interpretive dilemma, albeit in ways much too local and contingent to sustain his strongest programmatic claims. The first calls into question the scope of paradigm dependence; the second, the degree to which paradigms "lock in" the interpretation of evidence within the domains they cover.

Often when Binford is intent on demonstrating the prospects for stabilizing evidential claims he emphasizes the advantages of interpretive inference that is based strictly on biophysical linking principles. He invokes radiocarbon dating and

other physical dating techniques in this connection (e.g., 1981b: 135), and he argues that in his own Nunamiut study he was able to establish uniformitarian claims about the relationship between butchering practices, storage strategies, and the distribution of faunal remains because of an understanding of the "economic anatomy of caribou and sheep" (1983: 19). Although he clearly finds these cases compelling because they concern what he takes to be noncultural, and therefore noncontingent, constraints on human behavior, the account he gives of their inferential strength has wider significance.[6] For example, he notes that the ascription of "temporal meaning" (a date) to archaeological material depends on knowledge of "processes that are in no sense dependent for their characteristics or patterns of interaction upon interactions between [in this case] agricultural manifestations or political growth" (1983: 135); the relevant linking principles can be established independently of any theoretical (paradigm-specific) assumptions about the nature of human cultural behavior, or the specific cultural events or conditions that might be assumed responsible for a particular configuration of archaeological material. This property of independence is by no means unique to biophysical principles; it can be realized whenever the background knowledge relevant for interpreting an aspect of the archaeological record lies outside the scope of the particular test hypotheses (or, more generally, the interpretive paradigm) of interest to archaeologists, even when both concern human cultural life, broadly construed.[7] What Binford exploits in this localized appeal to paradigm independence is the fact that contra the strongest forms of contextualism he invokes (e.g., L. Binford and Sabloff 1982; L. Binford 1981b), no paradigm informing the interpretation of archaeological data is likely to be so comprehensive as to determine all aspects of what we can understand about the evidential significance of these data.[8] Consequently, even the most richly interpreted evidence can (sometimes) provide noncircular grounds for evaluating hypotheses about the cultural past.

Binford's first strategy is a matter of anchoring interpretive inferences tentatively—in relation to particular test hypotheses or paradigm assumptions—through the judicious use of source-side resources. It is most clearly instantiated in cases in which the evidence relevant for testing (or establishing) a claim about the cultural past requires a narrowly circumscribed interpretive reconstruction of the efficient causes responsible for specific aspects of the surviving archaeological record; under those conditions, the scope of a paradigm (or ladening theory) is likely to be limited in ways that allow for critical independence between different lines of evidence or between evidence and test hypothesis. But even when an orienting paradigm supplies the assumptions in terms of which source- or subject-side data are interpreted, these data may not conform to the expectations of the ladening theory. For example, when Binford describes the points of contention that provoked his long-running disagreement with Bordes over the interpretation of Mousterian assemblages,[9] he notes a growing number of anomalies in the empirical variability painstakingly documented by French Paleolithic archaeologists that even the strongest advocates of the extant normative paradigm found difficult to interpret (1972b: 252). Although Binford says he turned to actualistic research because there was nothing more to be gained at that point by further analysis of the archaeological data—the record itself would not provide the interpretive resources necessary for breaking the impasse he had reached in debate with Bordes—nonetheless it was the recalcitrance of these data that raised the critical questions he later pursued by other means. Even the use of normative interpretive principles did not ensure that the record would provide evidence of normative variability.[10]

The capacity of empirical data to delimit the scope and credibility of interpretive principles is also apparent on the source side of the equation, sometimes even when assumptions central to a dominant paradigm are concerned. Though Binford insists that his Nunamiut study is not a paradigm-testing exercise (1978: 6), he does routinely invoke the Nunamiut as a telling counterexample to normative theories. On his account, the Nunamiut are themselves well aware of the extent to which their behavior must be organized in response to changeable and closely constraining environmental conditions; conformity to cultural norms is a luxury they cannot afford. The centrality of environmental factors and logistical considerations in this case makes it clear, Binford argues, that norms and conventions cannot be given explanatory priority across the board (456); they

are incapable of explaining at least some forms and aspects of cultural life (viz., that exemplified by the Nunamiut). Binford also sometimes argues, more ambitiously, that the Nunamiut are a special case that proves a more general rule. Given the exigencies of life in the subarctic and the explicit pragmatism of the Nunamiut, their practices reveal with particular clarity the causal dynamics of response to environmental factors that structure all cultural systems. He reports a Nunamiut object lesson to the effect that the essential wisdom concerning norms is to know when to violate or amend them in response to context-specific contingencies. Generalizing within the instance, Binford argues that human adaptive success, and all the varied cultural forms that have taken shape as a consequence, reflects a unique capacity for projective planning and flexibility in response to changing environmental conditions; therefore, he concludes, it is ultimately the environment that accounts for the stability or variability of cultural behavior. Even the most highly conceptual dimensions of cultural life must be structured by the exigencies of "realistic coping with the concrete problems presented . . . [by] the environment"; it is preposterous, he declares, to suppose that human agents "convert experiences of life into concepts that do not bear some relationship to . . . experience" (456). With these arguments Binford claims source-side empirical support for his favored paradigm. Although his commitment to an eco-materialist perspective informed his choice of the Nunamiut as an ethnographic subject and every aspect of his fieldwork with them, he takes the resulting evidence to provide robust, noncircular grounds for rejecting the claims of any paradigm that grants explanatory primacy to cultural convention; the Nunamiut stand as a telling counterexample against normativism.

Yet while this case may support Binford's critical arguments against the strongest (most widely generalized) forms of the normative paradigm, it does not in fact provide clear-cut support for his more expansive claims on behalf of eco-materialist alternatives. As heavily interpreted in ecosystemic terms as it is, the evidence Binford draws from the Nunamiut suggests that his favored paradigm is no more viable as a comprehensive framework for archaeological interpretation than are the normative theories he rejects.

By Binford's own account, the Nunamiut are a limiting case on a continuum that includes cultures, like that of the !Kung, whose behavior seems to be minimally constrained by environmental factors; its invariance is to be explained by the fact that the !Kung do not require the strategic flexibility and long-term planning so essential to survival in the subarctic. But far from demonstrating that environmental factors are everywhere determinants of cultural behavior, this comparison suggests that in many contexts cultural norms and idiosyncratic preferences (rather than environmental factors) are indeed the contingencies to which human decision making is most directly responsive. Not only do the requirements of "realistic coping" allow for wide variance in the ways experience is conceptualized and behavior organized, they may allow for subsistence strategies that are far from optimal ecologically and perhaps even for systematic conceptual distortion. Thus an ecosystem paradigm can be counted on to explain the specifics of behavioral variability only in some contexts: those where the environment imposes especially tight constraints on survival. In many (perhaps most) cultural contexts some form of normative theory will be required in addition, or instead, to account for details of how these constraints are negotiated. To draw a lesson from the Nunamiut, the wisdom concerning paradigms is to know the limits of their applicability; when formulated as comprehensive theories of cultural behavior, neither of the options Binford considers —normativism or eco-materialism—seems adequate on its own, to the exclusion of the other, as a framework for archaeological interpretation.

Ironically, the weakness of Binford's source-side arguments for the central tenets of eco-materialism suggests a second promising strategy of response to the interpretive dilemma: even when evidence is selected and interpreted in light of a favored paradigm, it may prove recalcitrant in ways that call into question key assumptions of the paradigm itself. Where the first strategy for meeting the challenge of paradigm dependence relies on one prevalent feature of research practice—that paradigms typically do not cover all sources of background knowledge relevant for testing hypotheses derived from them—this second strategy exploits another. Even in the areas covered, paradigms are often not so tightly determining

of how we make sense of experience, and experience itself is not so plastic, that observation can be counted on to deliver all and only what the dominant paradigm dictates. Although these two methodological insights imply a qualification of the strong Kuhnian contextualism affirmed by Binford and Sabloff, they do not offer intellectual anchors that can secure archaeological interpretation once and for all; there may be no single, comprehensive foundation for interpretation, but the promise remains that the circle of paradigm dependence can be broken on a localized basis.

The crucial insight here is that the process of fitting experience into the conceptual boxes provided by a paradigm (to paraphrase L. Binford and Sabloff 1982) is indeed a *process,* and it is interactive on a number of dimensions. Critiques of naive empiricism make it clear that experience is never a given, never uninterpreted. But neither is it wholly constituted by the creative acts through which we structure it and make it intelligible. It is experience *of* an independent factuality that we encounter, probe, explain, and interact with—a world that we experience as existing and acting autonomously of us, our interests, and our theories; as highly constructed as it is, this world of experience is not (just) an artifact of the paradigm assumptions we bring to interpretation.[11] As a commonsense realist might put it, experience has a dual quality; as a source of evidence it cannot function as an autonomous (nondefeasible) foundation for belief, but it can sometimes serve as a crucial reality check. The key to making effective use of the duality of experience is to exploit as many different conceptual and empirical resources as possible; none of the methodological strategies I have described can decisively break the grip of paradigm dependence on their own but their strengths are complementary, especially when the process of bringing them into play is recursive. Source-side arguments, like those that underpin ecosystem and normative paradigms, may suggest a promising way to think about cultural phenomena, and systematic actualistic research can transform paradigm-based intuitions into sharply focused reconstructive and explanatory models of a particular cultural past. As often as not the archaeological evidence fits none of these models; such failure reopens a range of questions about the credibility of the interpretive principles

and paradigm assumptions that were the source of their initial plausibility, generating a new round of source-side research. Sometimes these anomalies make it clear that quite fundamental framework assumptions must be reassessed. But even when revolutionary change is not on the horizon, the accumulation of jointly archaeological and actualistic insight may create conceptual tensions that require continuous piecemeal revision of what we think we understand about the cultural subject, both in particular cases and at the level of orienting theoretical commitments.

In this process it is the continuous movement between source- and subject-side research that mitigates against the threat of paralyzing paradigm-dependence. Inevitably some elements of this system must be exempted from critical scrutiny while others become the focus of empirical investigation or conceptual analysis; as Kelley and Hanen have argued, a precondition for systematic inquiry of any kind is that some elements of the network of assumptions it presupposes function as a stable core while others are to varying degrees provisional (what they refer to as the archaeological "Core System"; 1988: 111, 118). It is crucial, however, that no element of this "web of belief" (Quine and Ullian 1978) be held permanently immune to revision. Even the most stable core assumptions must periodically face critical scrutiny as the content of our understanding evolves. The factors that drive this dynamic process are, jointly, constraints of internal coherence (which are themselves subject to revision) and the possibility that experience can be a source of disruptive empirical input to knowledge systems even though it cannot provide them a stable epistemic foundation.

Although naive empiricism is surely false for all the reasons advanced by New Archaeologists and their predecessors, in their strongest form the Kuhnian insights that they sometimes adopted in reaction—the "insights regarding the importance of paradigms and their impact on our ideas of objectivity" embraced by Binford and Sabloff (1982: 139)—are equally problematic. Paradigms are not seamless, all-encompassing, or all-pervasive. There is a dynamic tension within any one paradigm between its more purely experiential (empirical) and conceptual (theoretical) compo-

nents, as well as between the paradigms that orient us, in divergent ways, to different aspects of the world we live in. These tensions give us critical leverage in assessing even our most paradigm-dependent beliefs and assumptions. If we extrapolate from the promising aspects of Binford's practice, these tensions make it possible to play evidential and conceptual resources off against one another in a way that may not guarantee convergence on a single truth but can provide grounds for assessing degrees of plausibility and for rejecting (sometimes decisively) a good many alternatives. It is this concatenation of resources that can carry archaeology beyond paradigm-dependent speculation when the innocence of naive empiricism is no longer a viable option.

Epistemological Issues Raised by Symbolic and Structuralist Archaeology

There seem compelling reasons why archaeologists should adopt some form of structuralist approach, and yet even advocates of a structural archaeology sometimes assume that since it would concern itself with a notoriously inaccessible dimension of past cultures, it can claim to be no more than an exercise in creative speculation. I will argue that this assumption presupposes a false dilemma that opposes any study of the ideational, symbolic dimensions of the cultural past to properly scientific, empirically rigorous forms of inquiry; structural archaeology need not be consigned to the speculative horn of this dilemma simply because its theories are empirically underdetermined. The most promising and successful structuralist analyses of material culture exploit a methodological option that escapes the dilemma and that seems well suited to a structural archaeology. Glassie's study of Virginian folk housing (1975) and Clarke's analysis of archaeological material from Glastonbury (1973b) are cases in point, and I will rely on them to illustrate how this option can be implemented. My main concern is, then, with the epistemological questions that a structural archaeology raises about the kind of scientific or other knowledge archaeologists should striving to realize.

Consider first a philosophical analysis of structuralism, Pettit's *Concept of Structuralism* (1975),

for an indication of what structuralism might have to offer archaeology and why archaeologists should take such an approach seriously. In broad outline, Pettit characterizes structuralism as a research program that involves systematically extending to nonlinguistic fields a framework of linguistic concepts—a linguistic metaphor, underpinning a paramorphic model (1975: 39, citing Harré 1972: 174)—so that they can be seen to be like language in important respects and hence a proper subject for a variant of linguistic analysis. This framework of concepts serves as an analytic model that guides inquiry by providing a way of conceptualizing the phenomena in question that "draws us to an entirely new perspective on the subject," raising new questions and opening up new lines of inquiry (Pettit 1975: 109); in an archaeological context, Clarke identifies these as "controlling models" (1972c: 5; see the discussion of models in chapter 4 of this volume). In the case of structuralism, objects in the new field—cuisine, fashion, the "customary arts" (Pettit 1975: 42), or assemblages of material culture that survive in the archaeological record—are reconceived in semiological terms as cultural constructs that are analogous to sentence structures; in Pettit's terms, they encode or produce meaning effects by the arrangement of their component wordlike elements. Their meaning arises from a series of con-

trasts set up between distinct classes of elements subject to specific principles of choice that operate on two dimensions: constraints of syntagmatic ordering determine what articulation of elements (or classes of elements) will constitute a "well-formed," meaningful "string"; and the range of paradigmatic alternatives within a given structure allows for fine-grained manipulation of meaning content.

A structuralist analysis can be initiated when it seems plausible that something like a linguistic mechanism of articulation may be operating in a nonlinguistic field or, more strongly, when it seems that such a mechanism must be postulated to account for the systematic way in which well-formed, meaningful objects are constructed in that field. When a field is conceptualized in structuralist terms, as meaning-structured or meaning-bearing, the question arises of how meaning is encoded in nonlinguistic constructs; an inquiry modeled on linguistic analysis thus must establish an understanding of the articulating mechanisms involved.

While Pettit's account captures the essential character and promise of structuralism as a general research program—one defined by commitment to a particular (linguistic) analytic model in terms of which a field may be set up for semiological analysis—it also throws into relief several areas in which extending a linguistic metaphor to nonlinguistic fields can create difficulties. In general, nonlinguistic cultural phenomena do not seem to produce meaning effects in quite the same way or with quite the precision as do linguistic constructs; they do not convey specific messages regarding states of mind on strict analogy to sentences or speech acts. As Pettit observes of semiotic analysis generally: "It is important to notice one disanalogy between speech acts and acts of this kind [dressing up for a day at the races, having a light snack]. This is that semiological acts are not generally acts of communication. . . . I fail to tell you something if you do not recognise what effect I intend. I can dress up or have a light snack whether or not you, or any others, recognise what I am after" (1975: 36). This caution suggests that there are good reasons to suspect that linguistic models and the semiological approach in general may be of limited value in many areas of archaeological interest.

On the other hand, there is an important sense in which cultural items must be considered meaningful constructs *as* cultural; they do often embody structures of articulation that suggest the influence of tradition-specific conventions defining what constitutes a well-formed construct. They are material things that have been appropriated by cultural agents and made cultural through the imposition (or objectification) of order and intersubjective "models of intelligibility" or "innate logics of classification"; in this sense, the form and content of cultural constructs are (at least in part) determined by meanings that constitute a particular worldview. But however much they express and reinforce this worldview, they cannot be assumed to produce the kind of deliberate and systematic meaning effects that characterize its linguistic expressions. The linguistic analogy holds primarily at the level of the encoding process; meanings, and a mediating competence, may govern the structuring of nonlinguistic items but there is likely to be considerable latitude in the degree to which they support the systematic decoding of specific meanings.

The significance of thus qualifying the analogy that underlies the structuralist program is twofold. First, when nonlinguistic constructs lack clear-cut meaning effects, the would-be structuralist studying a subject such as cuisine or fashion or other aspects of material culture must demonstrate that the structures manifest in the phenomena in question are, to a significant extent, meaning-determined. Second, even when the basic analogy of meaning-determined structure clearly holds, as Pettit notes (e.g., 1975: 36–38), intuitions about proper (meaningful) form may be much less firm where nonlinguistic constructs are concerned, given that they cannot be assumed to have been created with the intention of producing language-like meaning effects. That is, the articulating mechanisms involved may not be strictly analogous to the sharply defined competences and sets of recursive structuring principles identified in the analysis of linguistic phenomena. Archaeological structuralists may be able to demonstrate that something like a syntax or competence must be postulated to account for the structured variability observed in a particular assemblage of material culture, but models of linguistic articulating mechanisms may not be directly applicable; the mechanisms involved may be quite different. The onus is thus on the structuralist operating in a non-

linguistic field to define the specific sense in which the phenomena under study are meaningful and to develop appropriate explanatory models.

The second qualification simply reaffirms the point that the linguistic metaphor operates as an analytic model; it provides a general conceptual framework for research and although it may suggest the kinds of explanatory models that would be appropriate for the field in question, it does not necessarily provide them ready-made. While this qualification defines the task that confronts a structural or contextual archaeology, it is the first that presents the immediate challenge to archaeologists who must make a case for viewing their data as meaningful and for framing research under the guidance of a linguistic metaphor. The sort of argument that can be used to set up a field for structuralist analysis is suggested by Chomsky's argument (1959), against behaviorism, that innate cognitive capacities must be postulated to make sense of the human ability to acquire and use language. On this "poverty of stimulus" argument, whenever the output of a system is much more complex than the input or stimulus, the factors that account for its behavior should be sought within the system. In an archaeological context this suggests that when the richness and variability of the material record is too great to be explicable solely in terms of response to environmental constraints or stimuli, factors internal to the cultural system must be considered. Bourdieu (1977) relies on such an argument in his classic analysis of Kabyle house structure to establish that a structuralist approach is appropriate for analyzing material culture. It is implausible that technological imperatives or functional requirements could account for the form and layout of these houses, as they manifest such a complex of boundaries and articulating parts. An adequate explanation must take cognitive factors into account, in particular the rich cosmology and codified social relations embodied in the "structuring structures" that, on Bourdieu's account, are "revealed only in the objects they structure" (1977: 90).

Variants of this poverty of stimulus argument appear in the archaeological literature whenever the complexity of material variability seems to outstrip the resources of the dominant eco-materialist paradigm and suggests the need to consider the internal social and ideational aspects of past cultures. But the argument from complexity of output does not on its own establish the need to adopt a *structuralist* approach rather than research programs that might bring other features of a cultural formation into focus. To establish that structuralist analysis is appropriate for a given subject domain, a further argument is required to the effect that cognitive, ideational factors are likely to have played a significant role in structuring the content of that domain. Leach makes such a case for archaeological material when he argues that most archaeologists have reason to believe that they are dealing with intentional beings who have distinctively human cognitive capacities for self-determination, the prototype of which is the ability to acquire and use language (1973: 763–764). In this case it cannot be assumed, he insists, that the cultures or individuals that archaeologists study responded directly to environmental stimulus; their behavior must be understood to involve a capacity to "engage in 'work' (*praxis*)" (765). They survive by deliberately manipulating and transforming the environment to which they adapt, in part through projecting culturally specific "cognitive maps" onto the material world. Consequently, Leach concludes, "archaeologists must appreciate that the material objects revealed by their excavations are not things in themselves, nor are they just artifacts—things made by men—they are representations of ideas" (763).

The combination of a generic poverty of stimulus argument and Leach's appeal to a distinctive human, cultural capacity for intentional action establishes that the archaeological record is always a potential subject for linguistic-type analysis; it is reasonable to attempt to disembed the principles of articulation, and perhaps the underlying ideas, that structure the cultural material encountered in the archaeological record. In fact, these arguments establish considerably more. They introduce the linguistic source model as a metaphysical thesis about the nature of cultural phenomena that brings a crucial and otherwise overlooked dimension of archaeological material into view: namely, that it is meaningful in the sense that systems of meaning are instrumental to its formation. These arguments suggest that formal variability in the archaeological record is due, at least in part, to structuring mechanisms that operate on a cognitive and ideational level, an implication that suggests, in turn, that inquiry into this dimension of past cultures is not merely an inter-

esting option opened up by a novel perspective. The degree to which structural factors play a role is an open (empirical) question, but the possibility that they are explanatorily relevant can never be ruled out of consideration in advance. It must always be assumed that archaeologists may have to concern themselves with such factors if they are to give an adequate account of the cultural significance of archaeological data.

Structuralism, then, offers archaeologists a way of conceptualizing their data as material *culture,* at least some aspects of which must be understood as meaningfully constituted and, in that sense, semiological. The difficulty, however, is that as a research program, structuralism characteristically "lays bare the underlying principles of operation" that are presumed responsible for manifest patterning in the record; it seeks to disclose a structural domain—"the fundamental elements of a phenomenon, their articulation, and the consequences of the interplay of their different levels"—which has "objective existence" but is not itself directly, observationally accessible (Glucksmann 1974: 174, 153). For many archaeologists who might be inclined to take a structuralist approach seriously, this inaccessibility raises serious epistemological problems. The central issue here was also raised by Leach: a structuralist program in archaeology directs attention to the complex inner workings, particularly the cognitive workings, of past cultural agents; yet these, Leach insists, constitute the interior of a "Black Box" (1973: 765) that is decisively closed to the archaeologist because it is never accessible to direct inspection. He takes the position that "as soon as you go beyond asking 'what' questions" and "start asking 'how' and 'why' questions" then "you are moving away from verifiable fact and into the realm of pure speculation," particularly when the "how" and "why" questions are directed at the details of how "the prehistoric game of social chess was played out" (1973: 764; see also 1977). Leach goes on to say that although speculations about the internal content and structure of the archaeologists' black box can never be expected to "rate better than well-informed guesses," it is still important, indeed essential, that archaeologists should make them: "All I am saying is that you should recognize your guesses for what they are, and not delude yourselves into thinking that, by resort to statistics and computers, you can convert your guesses into scientifically established facts" (1973: 768).

Leach's epistemic worries set up a dilemma for the archaeologist. If the structuralist argument is taken seriously and it is recognized that the cognitive and ideational content of the cultural black box must be dealt with because its material output cannot be assumed to be explicable in strictly functional-adaptive terms, then there seems to be no recourse but to abandon empirical inquiry and take up precisely the type of nonscientific guessing from which contemporary archaeologists have been intent on distancing themselves. Some structuralists have been prepared to accept these terms and embrace the speculative horn of this dilemma, despite a strong commitment to rigorous standards of empirical analysis. Glassie comments, "Once the artifact, whether document or house, has been analyzed, the student has a choice. He may stop; from the angle of scientific method he cannot go farther. Or, he may adopt the risky sort of explanation traditional to history and move from assembled facts to hypothetical causes, thus eschewing methodological purity for understanding" (1975: 185). The sense Glassie conveys is that insofar as archaeologists are sensitive to the richness of archaeological material as a cultural record, they will be forced to adopt nonscientific, speculative modes of reasoning; and in that case, they might as well allow themselves to be guided by intuitions and methods drawn from linguistics as by any other interpretive source model.

By contrast, Clarke endorses rigorously scientific modes of practice in arguments that closely parallel those of the New Archaeology, but unlike his North American counterparts he never gives up a commitment to investigate the normative dimensions of cultural life. His brief for an "analytical archaeology" turns on an extended argument for controlling models, in the form of a richly semiotic variant of systems theory, that focus attention specifically on the "role of material culture as an information communication system" (1968: 401). When he undertakes to specify how the *analysis* of archaeological data should proceed, however, he grasps the other horn of Leach's dilemma. He proposes an "empirical approach" characterized by the use of formal techniques to search for regularities, from which all forms of "theoretical bias" are systematically excised; analysis is an exercise in pattern detection that ultimately serves

the purpose of organizing data "for our own predictive convenience" (637). Otherwise sympathetic commentators immediately objected to what Hymes described as "a clear discrepancy between [Clarke's] theoretical analogies and actual practice" (1970: 10), arguing that a methodology marked by the "absence of attention to qualitative structure and its analysis" could not but fail to provide an understanding of the meanings and meaningful structures embodied in archaeological data: "the step or leap from debris to a general theory of what the debris represents—the 'code behind the messages' to use Clarke's own analogy—is not to be gotten by pressing the analysis of debris as far as it will go" (19). Leach's dilemma thus reasserts itself; in archaeology, at least, rigorously scientific practice seems to be inherently at odds with the goals of structuralist analysis.

This is, I argue, a false dilemma generated by an untenable and self-defeating skepticism about the possibility of establishing any reliable, empirically grounded knowledge about the cultural past. Certainly it is not unique to archaeology or to structuralist inquiry that interesting theories are underdetermined by all available data, or that unobservable dimensions of the cultural reality in question should be the primary object of inquiry. Mellor made this point directly in his rebuttal to the skepticism he discerns in Leach's remarks:

> No doubt the data will always be flimsy, the tests inconclusive, the scope for imaginative alternative theories great. None of this reduces archaeological theorizing to the level of guesswork. The complexity of the subject and the relative paucity of data may well be part of what makes archaeology, like cosmology, endlessly fascinating and likely to be endlessly unsettled. But it is a great mistake to suppose that what is endlessly fascinating and unsettled therefore cannot be scientific. (1973: 498)

The fact that a structural archaeology must reach beyond the observable record does not establish that (among archaeological research programs) it is uniquely or necessarily unscientific and limited to arbitrary speculation.

If this argument of philosophical principle is accepted, then the structural archaeologist should have some epistemological options that escape Leach's dilemma. One is to treat the structuralist program as a procedure for constructing models that, on the linguistic metaphor, bring order to

disparate bits of cultural phenomena by providing an account of the cognitive and ideational factors presumed to have been instrumental in generating them. While inevitably these models will be underdetermined by the accessible empirical data, by no means are they constructed as convenient or conventional fictions. They are formulated on the basis of an explicitly realist presupposition that some such mechanisms or processes did exist and operate independently of our knowledge (or lack of knowledge) of them and are indirectly accessible to us through their tangible surviving effects. Because these models carry quite specific ontological commitments—they make claims about actual past conditions responsible for the surviving archaeological record—they will be subject to two sets of constraints that set them apart from the products of purely speculative interpretation: plausibility considerations introduced by the analytic model (as a thesis about the nature of the phenomena in question) and mediated by background knowledge about the conditions or mechanisms that could produce such phenomena; and empirical constraints on what may reasonably be claimed about the cultural past adduced from the material record of conditions and processes that actually existed in the past. Even when explanatory models refer to such intangibles as cognitive or ideational factors, effective use of these constraints can underwrite a measured confidence in the claims they make about the past. Such confidence provides, in turn, grounds for resisting Leach's skepticism about the possibility of any nonspeculative knowledge of the cultural past.

Pettit's account of structuralism as a research program captures the overall form of the methodology by which these constraints are brought to bear on explanatory theory. Its point of departure is the conceptual restructuring of a field by an analytic model that delimits a search space for candidate explanatory models. Within this framework specific models can be constructed, using background knowledge as a source for characterizing mechanisms that, by analogy with better-known sources, could have produced the subject phenomena. Although this process of construction is open-ended, the analytic model can significantly constrain the options considered; for example, a structuralist orientation rules out models that categorically privilege ecological, technological factors as the key determinants of variability in ma-

terial culture and it directs attention to source contexts in which articulating mechanisms of a cognitive sort are known to operate. In addition, the systematic analysis of source contexts can delineate not only a range of mechanisms that could produce the effects in question but also the conditions under which they operate, establishing grounds for a highly selective projection onto the past of those features of known (or imaginable) mechanisms that most likely could have been present in the past and responsible for the existing archaeological record.

The collection and analysis of archaeological data are then matters of probing the surviving effects of mechanisms that actually obtained in the past, bringing a new set of empirical constraints to bear on the modeling process. These constraints make themselves felt through a mutually conditioning interaction between fact and theory that is better captured by Collingwood's logic of question-and-answer (1978 [1939]: 30–43) than by the hypothetico-deductive model more typically cited in archaeological contexts. The facts themselves take shape in the process of probing for evidence that bears on the claims of a particular model; "you can't collect evidence before you begin thinking . . . because thinking means asking questions . . . , and nothing is evidence except in relation to some definite question" (Collingwood 1946: 281). Collingwood illustrates this point with a detective story (1946: 266–298) in which hypotheses about murderous motives and criminal means—prospective answers to the encompassing question of who committed the crime (and how and why)—give rise to sharply focused questions about what material clues should exist (or not) if one suspect or course of action rather than another was the cause of death. For all its question and model relativity, this procedure is not necessarily viciously circular. Data are interpreted as evidence in a process of trying out the explanatory models suggested by the analytic framework to see if, when the data are conceived as the outcome of one type of mechanism rather than another, they are better integrated or take on a more intelligible form. Internally, this is a process of asking whether a postulated mechanism can account for all the evidence, or whether it brings to light specific features of the record—formerly unrecognized properties or patterns of association—that could only be expected if the given mechanism

had, in fact, been responsible for the content and structure of the surviving record. In an archaeological context, controlled question-and-answer testing is a matter of determining the applicability of prospective explanatory models; it is telling to the degree that these models are formulated in enough detail that there is a genuine possibility of their being subverted by the empirical evidence they help bring to light.

It is here important to recognize that, as Mellor has commented, "such intellectual bootstrap operations are not in principle *ad hoc,* nor are they peculiar to archaeology[;] . . . [they are] a corollary of theories inevitably going beyond all the data they can explain and against which they can be tested" (1973: 479). They are unavoidably common scientific practice and represent the sort of methodological option that, I suggest, is open to a structural archaeology. The procedure of "bringing a rich idea to sparse data to govern its description" and thus make explanatory sense of it (Pettit 1975: 88) only lapses into unscientific speculation if explanatory models are so vaguely formulated that they will accommodate any body of data, or if the description of evidential fact is so manipulated it can be fit to any theoretical framework. This, Pettit suggests, is the weakness of Lévi-Strauss's approach to structuralist analysis, which he characterizes as "little more than a license for the free exercise of imagination" (1975: 92); but it is not a shortcoming that need characterize structuralism as a research program (see Leach 1970). In the end, the potential of a structuralist archaeology depends on whether those committed to it can move beyond arguments that open up the field to structuralist analysis and develop, within this rubric, explanatory models that are sharply enough formulated to sustain rigorous interrogation of the archaeological record.

To illustrate how these methodological options beyond speculation might be effectively exploited by a structural archaeology, consider two examples: Glassie's analysis of Middle Virginian folk housing (1975), an influential example of structuralist procedure in application to a non-linguistic field, and Clarke's structuralist analysis of archaeological data from the Iron Age settlement at Glastonbury, Somerset (1973b). Clarke makes effective use of a much richer methodology than he recommends, one that parallels Glassie's practice in many respects. And despite

Glassie's official skepticism, the strategies of inquiry he and Clarke employ show how empirical considerations can constrain a structural analysis of material culture such that the explanatory models proposed as its outcome warrant (tentative) acceptance as considerably more than appealing fictions.

Glassie opens his analysis with an account of the long-entrenched conventions that live on in Virginia. In doing so he challenges any strictly functional view of the architectural components of this tradition, suggesting that they embody a distinctive worldview fundamental to Virginian life and must be regarded, along structuralist lines, as meaning-determined constructs. The linguistic nature of this analytic model only becomes clear when Glassie begins to exploit its inherent standards of plausibility, drawing out what he calls a "general idea" with which to approach the vernacular forms of architecture he considers. He proposes that where folk architecture constitutes a recognizable tradition manifest in a limited range of forms, it must be assumed that the design process was governed by something like a linguistic competence; Glassie calls it an architectural competence.

Glassie's objective in studying Middle Virginia folk housing is to develop an explanatory model of the specific competence, the "unconscious cultural logic," that influenced Virginia builders and defines the architectural tradition they produced. His aim is to form his "general idea" into more specific explanatory models that would, he says, "enable the analyst to locate an unexpected abundance of information in discrete things—things floating free of their contexts—and to relate apparently unconnected phenomena into a system" (1975: 41). That is, he seeks to disembed underlying cognitive and cultural principles that, once grasped, capture the intelligible structure of the surviving fragments of an architectural tradition, giving them coherent explanatory form and meaning.

Although Glassie frequently represents the processes of data collection and theory or model formulation as quite separate aspects of research, in his practice they are intimately connected. He constitutes architectural data as evidence through a recursive process of question-directed probing of the data; he asks, What principles must have guided Virginian designers and builders such that they generated the particular (limited) range of architectural forms that can be observed today? And in response he posits a basic inventory of geometric forms—squares defined by a standard unit of diagonal measurement extended into a series of rectangles—as well as a set of structuring rules specifying how to add, mass, pierce, and otherwise elaborate these forms so as to arrive at the recurring architectural solutions that constitute "well-formed" houses in the Middle Virginia tradition. As highly theoretical as it is, the resulting model is by no means a tissue of arbitrary speculation. The principles that make up Glassie's model are specified closely enough that they risk contradiction by the facts of the architectural tradition; sometimes he discovers that they are empirically untenable and sometimes they direct attention to structural features of the architectural tradition that he had not previously recognized. For example, Glassie describes how, at an early stage in the research, he had to revise his initial hypothesis about the geometric forms basic to the tradition. His orienting structuralist analogy suggested that there must be some such basic building component to which rules of assembly could apply (1975: 13–21), but he found that the units manipulated by Middle Virginia house builders were not defined by their end measurements as he had expected; they were, instead, defined by a diagonal measurement. Here Glassie's theoretical commitments enter directly into the process of data collection and analysis; they give the data form and significance as evidence of a postulated design process, but at the same time the resulting evidence constrains his claims about the nature (and reality) of an underlying architectural competence. It is this procedure—Glassie's transgression of the requirements of scientific purity—that lends his model initial credibility as an account of the structuring principles that actually (if tacitly) informed the work of Virginian house builders. There may be other explanations for the recurrent structure distinctive of Virginian houses, but Glassie's so precisely captures the underlying structure of the architectural forms it is intended to explain that it seems to take on factual status itself once it is articulated; it disembeds facts of relational structure inherent in the details of the architectural field.

Clarke's study of the site layout and assemblages from Glastonbury illustrates many of the same principles as Glassie's practice, in this case

in a reanalysis of archaeological data that had been collected a generation earlier (Clarke 1973b: 802). At a first level of analysis Clarke posits a series of "mental or cultural categories" drawn from the theoretical analogies he uses to set up archaeological data (qua cultural material) as information-bearing cultural material (809). He puts these categories to work in analyzing the archaeological data, disembedding potentially significant (intentional) regularities from background "noise." In the process he refines the categories themselves and formulates increasingly specific explanatory (structuralist) hypotheses about the particular "building 'rules'" (810) that might have informed the decisions made by the prehistoric occupants of Glastonbury (e.g., about how to select and exploit available "building stocks" and "site building potential"). By these means, Clarke identifies a number of striking structural regularities in the layout of the site and in the associations among artifacts and features, finding, for example, that the site is made up of basic "site occupation unit[s] based on pairs of round houses with varying auxiliaries" that fit a pattern—an architectural grammar—evident in the layout of many British and Irish Late Bronze Age and Iron Age sites (827).

The results of this first stage of analysis immediately raise a number of new questions about the meaningful content, the "messages," articulated by the occupants of Glastonbury when they followed the rules of competence Clarke discerns in the structure of their surviving archaeological record. Clarke takes up these questions in a preliminary way, drawing on ethnohistoric information preserved in the "records of classical authors and in the Irish vernacular tradition" (1973b: 843). On this basis he formulates a set of postulates about the nature of the social, political, and residential organization of Iron Age society and about the economic and other factors that might "predispose a society to move toward a limited set of family structure and residence patterns" of the sort instantiated at Glastonbury (847). These he describes as "crude and elementary preliminaries," which nonetheless provide the point of departure for a new round of empirical analysis; as hypothetical answers to new questions "they suggest at once the ways in which they might be tested and the directions in which they must be refined," drawing attention to quite different ranges of ar-

chaeological data than those Clarke had thus far taken into consideration (867).

At every stage of this structuralist, information-theoretic study of Glastonbury, Clarke's analysis of the archaeological record is richly interpreted; it never conforms to the requirements of his resolutely theory-free "empirical method." Even at the most preliminary stages of empirical analysis Clarke proceeds on the assumption, supplied by his orienting model, that the archaeological record is cultural, information-bearing material and, as such, can be expected to reveal specific kinds of nonrandom patterning. He is careful to point out that the considerable scaffolding of (potentially confounding) theoretical assumptions he has introduced is itself subject to close empirical control; at each juncture in the process of framing questions and prospective answers he considers "ways in which [the models deployed] might be tested and the directions in which they must be refined" (1973b: 867). To this end he exploits plausibility constraints in the initial formation of explanatory models and constraints imposed by the archaeological record, elicited in a Collingwoodian question-and-answer procedure that involves probing both archaeological and ethnohistoric sources for evidence that, in the most decisive cases, could only obtain if a particular prospective answer were (approximately) true.

The explanatory models developed by Clarke and Glassie are credible because they arise from a process in which fact and theory are integrated in precisely the ways that both researchers, for different reasons, resist when they make programmatic statements about the limits of inquiry. While they may have sacrificed certain (untenable) ideals of objectivity—specifically, those that require that hypotheses be tested by means of confrontation with an autonomous (immutable) set of facts—they gain explanatory models that are richly grounded in and conditioned by empirical detail. Although most archaeologists will not have access, as Glassie did directly and Clarke indirectly, to the collateral evidence of oral traditions that express the worldview embodied by architectural and other material culture, their reconstruction of a structural mediating competence in the first stages of their analyses does seem to exemplify a viable strategy for dealing with the symbolic and cognitive dimensions of archaeological material. When it is possible to reconstruct the beliefs or

worldviews that underlie a structural competence, the analyst is in a position to extend the orienting structuralist model to deeper levels of the cognitive reality in question. But there will always be further possibilities for explanation whenever one level of generative mechanism has been brought into view and questions are raised about the underlying conditions responsible for its form, existence, and operation.

My thesis is, then, that a structuralist approach offers archaeologists a compelling way to conceptualize archaeological data as cultural material when there is reason to think that this material is meaning-structured and meaning-bearing. Although a structuralist archaeology is often understood to raise the specter of a paralyzing epistemic dilemma, there is nothing in the commitment to explore the cognitive dimensions of the cultural past that renders it categorically unscientific. Inasmuch as structuralist archaeologists engage a process of reaching beyond observables, formulating and testing models of the conditions and processes responsible for the archaeological record, their work is well within the scope of practices that characterize science at its best and most successful. The great value of a structuralist approach is that it challenges archaeologists to come to terms with the distinctively cultural aspects of the material they study.

The Reaction against Analogy

To confine our studies to mere antiquities is like
reading by candle-light at noonday.

Daniel Wilson, *Prehistoric Man: Researches into the
Origin of Civilisation in the Old and New World* (1862),
as quoted by Orme (1974)

However much analogical inference has broadened interpretive horizons and however indispensable it has seemed to the interpretation of archaeological data, arguments by analogy have long been an object of uneasy mistrust among archaeologists. In fact, this mistrust has grown steadily in the past hundred years despite the essential role that Orme (1973, 1974, 1981) shows ethnographic analogy to have played in shaping contemporary conceptions of prehistory. As professional archaeologists struggled to differentiate their discipline from nineteenth-century antiquarianism and armchair anthropology, analogy became a particular target of criticism; the speculations of early evolutionary theorists had made its potential to mislead especially clear. By the mid-1950s, however, a growing number of Anglo-American archaeologists had come to see analogical inference as indispensable and sought ways to make it a respectable methodological tool. They continued to face skeptical challenges, but in 1961, in an influential review of this protracted debate, Ascher responded with a series of optimistic proposals for "placing analogy on a firmer foundation" (1961: 323). Yet within just a few years, all attempts to redeem analogy were once again rejected out of hand, this time by advocates of the self-consciously scientific New Archaeology, who insisted that no amount of cautious reformulation

could establish analogical arguments with the security appropriate to properly scientific research.

What ensued was a reaction against analogy in which historic mistrust of its insecurity grew to entirely new proportions. At the very least, the New Archaeologists insisted, the use of analogical inference should be strictly limited; it should serve only as a means of generating hypotheses whose credibility must be established on independent, empirical (non-analogical) grounds. Some critics, such as Freeman (1968) and Gould (1980; Gould and Watson 1982), argue that it should be denied even that role. Because analogical inference is a matter of projecting aspects of the present onto the past, it carries an unavoidable risk of limiting what archaeologists can understand of the past, obscuring what may be unique about past cultural forms. Gould declares, on this basis, that "analogy is an idea whose time is gone" (1980: x); it should be replaced by non-analogical methods of formulating and evaluating interpretive hypotheses.

In the first sections of this chapter I review some of the developments in archaeological thinking about analogy that led to this strong reaction against it. In subsequent sections I argue that the critics who categorically reject analogical reasoning largely fail to identify viable alternatives to it, and that indeed the alternatives they propose are themselves analogical in form. This reliance on

analogy does not mean, however, that archaeological interpretation is reduced to mere speculation. Its critics also fail to show that analogical inference is as categorically unreliable and misleading as they claim. In the final sections of the chapter I argue that despite their hostility to all forms of analogical inference, the New Archaeologists often inadvertently provide valuable insights about ways it can be improved that complement (rather than supersede) the suggestions made by Ascher and his predecessors. For a general characterization of these convergent proposals, I draw on accounts of analogical inference that are standard in informal logic. My thesis is that although a candid appreciation of the limitations of analogical inference is certainly appropriate, its use in archaeological contexts is neither dispensable nor radically faulty. It can play a legitimate, constructive role in archaeological inquiry within certain guidelines that have been emerging, under pressure of increasingly sharp criticism, since the inception of a methodologically self-critical archaeology.

HISTORICAL AMBIVALENCE ABOUT ANALOGY: OBJECTIONS AND PROPOSALS

EARLY USES AND ABUSES OF ANALOGY

Early uses of analogical reasoning are often characterized by an expansive enthusiasm for its potential as a source of insights about prehistory. They have been discussed by Charlton (1981), who traces them back to classical Athenian historiography, and by Orme (1974, 1981) in connection with her analyses of the impact that expanding ethnographic knowledge has had on archaeologically based conceptions of prehistory since the sixteenth century. Orme argues that contact with contemporary "savages" made it possible to conceive of British and, more generally, of European prehistory entirely differently than when it had been understood exclusively in terms of the lifeworld of sixteenth-century Europe and its historically documented antecedents. At what Orme calls the "practical" level of "recognition and interpretation of artefacts" (1981: 2), whole classes of enigmatic material that had been ascribed mythic or magical significance were recognized as artifacts of human, prehistoric origin. This recognition led, slowly, to the broader realization that the ancestors of the modern Britons very likely included

"men as savage as the Indians who lived long before the start of recorded history," that is, "before the Roman Conquest" (31). On the face of it, this constitutes the sort of broadening of interpretive perspectives that has traditionally vindicated a reliance on analogy, leading its proponents to see it as an antidote to narrow ethnocentrism and as a rich source of insights about "varied and heterogeneous reasons or causes" that may account for otherwise enigmatic archaeological materials (Ucko 1969: 262).

But there was another side to these early, horizon-expanding uses of ethnographic analogy. The change of attitude about prehistory documented by Orme also gave rise to the development of grand theoretical schemes for "discerning and explaining the processes of human cultural development" (Orme 1981: 2), the cornerstone of which was the notoriously overextended use of ethnographic analogy that characterized classical evolutionary theory. Orme finds the comparisons of prehistoric and "primitive" peoples so thoroughly absorbed by antiquaries by the eighteenth century that they unquestioningly equate the prehistoric with the (modern) "primitive" (1981: 11, 1973: 489). As ethnographic contacts and reports proliferated, however, a great variety of contemporary "primitive" cultures were identified, suggesting, on the basis of the prehistoric-primitive equation, that human prehistory was vastly more complex and diverse than originally thought. This newly recognized variability was given structure and made intelligible by nineteenth-century evolutionists who proposed that contemporary cultures should be understood to embody various degrees of cultural achievement that could be projected onto the past as stages in a determinate course of development. Contemporary "primitives" were thus presumed to be comparable to the earliest prehistoric forms of "savagery"; they are the evolutionary starting point in a sequence of technological, economic, and political stages of development that culminate in the industrialized civilizations of Great Britain and Western Europe. Once formulated, this speculative scheme functioned, in turn, as a template for the interpretive reconstruction of prehistoric cultures wherever archaeological materials were considered in their interpretation. Ascher cites this as the first systematic, if ultimately misguided, use of analogy in archaeological interpretation (1961: 317).

The classic example of analogical interpretation conceived in the tradition of nineteenth-century evolutionary thought is Sollas's much-cited series of lectures, *Ancient Hunters* (1924; see Ascher 1961: 317–318), in which four ethnographically documented hunting cultures are identified as the contemporary counterparts of four archaeologically known prehistoric ages.[1] In selecting these interpretive analogs, Sollas was directly influenced by Tylorian evolutionism; thus his interpretation proceeds on the unquestioned assumption that the modern ethnographic "primitives" he cites represent their prehistoric counterparts in the strong sense of being, quite literally, their descendants. He argues that the populations who originally developed the prehistoric hunting adaptations that make up his four prehistoric ages would each have occupied "what is now the focus of civilization" during the period when they represented the highest level of human cultural achievement (Sollas 1924: 599). As successively more intelligent, more technologically sophisticated and adaptively successful races emerged to displace them, each was "expelled and driven to the uttermost parts of the earth," where, on Sollas's account, their descendants live to this day in an arrested state of development. Given this literal construal of the descriptive metaphors used to characterize modern primitives as "survivals" or "representatives" of past forms of life, Sollas draws the following conclusions about the archaeological record of the three ages of prehistory that concern him: "The Mousterians have vanished altogether and are represented by their industries alone at the antipodes; the Aurignacians are represented in part by the bushmen of the southern extremity of Africa; the Magdalenians, also in part, by the Eskimo on the frozen margin of the North American continent and, as well, perhaps, by the Red Indians, on the one hand, and, on the other, by the Gaunches and sporadic representatives in France" (599).

The formal relations of comparison set up in the sixteenth century between prehistoric cultures and modern primitives are thus supplanted by the presumption that actual historical and, indeed, "genetic" connections exist between prehistoric cultures and their contemporary analogs.[2] In Sollas's account these formal relations of comparison are reified and evolutionary theory, itself an interpretive postulate based on analogy, is accorded the

status of a factual account of prehistory. The answers to virtually all interpretive questions that might be raised about specific prehistoric cultures are thereby determined in advance. Rather than functioning as a source of guidelines for selecting analogs, this theory dictates that prehistoric subjects will be quite literally assimilated to contemporary cultures that are assumed, on the encompassing theoretical scheme, to represent the same stage of evolutionary development.

Although Sollas was to some degree selective, often recognizing partial representation of past cultures in their present analogs, his interpretive scheme was a patently arbitrary ideological construct. No matter how striking the factual anomalies—Ascher observes that these compromise every aspect of the account (1961: 318)—the orienting theoretical framework was never itself considered open to question. Far from helping to liberate antiquarian interpretation from its ethnocentric limitations, Sollas presses the expanding range of ethnographic sources into the service of a scheme that reiterates precontact patterns of interpretation. Rather than postulating a past "peopled with characters from Caesar and Tacitus, living in a world curiously akin to the sixteenth century" (Orme 1981: 3), he envisions prehistory as having been peopled by "savages" modeled on those who had recently been subjugated by Europeans. And he understands cultural diversity to represent a rigid course of "intellectual progress" governed by a principle of "right . . . founded on might" (Sollas 1924: 599) uncannily like that which animated the politics of nineteenth-century imperialism.[3]

REACTIONS AGAINST THE EXCESSES OF CLASSICAL EVOLUTIONISM

What the critics of analogy originally reacted against were analogical interpretations, like Sollas's reconstructions, that turn on a "simple and direct reading of the past from the present" (to use Gould's phrase; Gould and Watson 1982: 446). This response was not restricted, however, to these worst-case examples of overextended analogical reasoning; Sollas-type cases were feared to exemplify a certain liability to error inherent in any use of analogy. The reason for generalizing this worry is the concern that if analogical inference is compelling, it presupposes some form of uniformitarian principle that establishes grounds

for treating the similarities known to hold between an interpretive subject and its analog as a reliable indicator that further similarities hold in areas where direct comparisons cannot be made. Not only are the scope and reliability of uniformitarian assumptions inevitably suspect, leaving the inferences based on them inconclusive, but in making them there is always what J. Grahame Clark describes as "the real danger of setting up a vicious circle and of assuming what one is trying to discover" (1951: 52). In Sollas's work this danger is fully realized: by reifying the comparison between modern and prehistoric "primitives," recasting it as a relationship of direct descent, he assumes precisely the similarities that his analogical arguments are meant to establish.

The larger worry inspired by Sollas-type interpretations is that because we lack any independent access to the past by which we might directly check the accuracy of both the assumptions and the conclusions of analogical arguments, we have no means of reliably detecting and avoiding error of the kind exemplified by overextending analogies. That analogical inference is always liable to error thus becomes the basis for generalized skepticism about its credibility as a class of inference. This skepticism gives rise to the further worry that when archaeologists rely on analogy, they inevitably risk assimilating past to present; if they appeal to ethnohistoric sources, they cannot avoid constructing the cultural past in the image of the present—more to the point, in an ethnocentric image of the present.

Writing about the state of anthropological research in 1939, and more specifically about archaeology in 1940, Kluckhohn describes a pervasive wariness of any of the "more abstract aspects of anthropological thought" and a debilitating preoccupation with empirical description (1939: 328), the lingering effects of overreaction to the excesses of evolutionary speculation. He urged his colleagues to confront the evolutionist debacle directly, to learn from it and develop interpretive procedures that do not devolve into arbitrary speculation. Twenty years later Ascher (1961) could review a considerable body of archaeological literature on "analogy in archaeological interpretation" in which a number of detailed proposals had been made for improving the standing of analogical inference. Two strands of thinking are evident in this literature: one has to do with ways of rectifying the errors associated with evolutionist interpretation, and the other takes the form of strategies for addressing more general worries raised by increasingly skeptical critiques of analogy.

One response to the overextended analogies of evolutionist reconstructions was to restrict and substantiate the principles—the reified assumptions of uniformity—on which they depend. Rather than presume genetic connections to exist whenever general theories suggest that particular prehistoric and contemporary cultures may be comparable, Clark recommended that archaeologists seek analogs among living cultures where actual historical ties to the prehistoric subject can be demonstrated. They would do well, he argued, to "pay more attention to the Folk-Culture of the area in which they happen to be working" (J. Clark 1951: 55), on the principle that where cultural continuity can be demonstrated, some features of antecedent, prehistoric ways of life may be expected to survive in the highly conservative "rural substratum" or "peasant basis" of contemporary societies. Here Clark invokes a widely shared conviction that "analogies torn from their historical contexts may be very deceptive" (55) and articulates the complementary principle that if historical continuity can be established, "historical context" can be treated as a constant. If prehistoric and ethnohistoric cultures can be shown to be part of a continuous tradition, then similarities in their material culture can be presumed to have been shaped by similar conditions and associated with the same behavioral or functional variables. This line of reasoning underpins the preference for direct historic analogies that is a persistent theme in subsequent literature on analogy.

Given the "vast temporal and spatial tract" (Ascher 1961: 319) for which there is an archaeological record but no surviving, historically connected analogs, even the strongest advocates of direct historic analogy, like Clark, recognize a legitimate role for new or unconnected analogs. Clark's proposal was that under these circumstances, archaeologists should make use of a "comparative method" for selecting relevant analogs, where relevance is specified by interpretive principles based on refinements of those that had informed evolutionist reconstructions of prehistory. Although Clark is careful to insist that cultures cannot be assumed to represent determinate stages in a "unique and universal" model of

cultural development (Ascher 1961: 319), he does recommend that archaeologists seek analogs, on a case-by-case basis, among cultures "at a common level of subsistence[,] . . . existing under ecological conditions which approximate those reconstructed for the prehistoric culture under investigation" (J. Clark 1953a: 355). This constitutes a neo-evolutionary, adaptationist principle; cultural groups that rely on similar technologies to subsist in (or manipulate and exploit) similar environments are likely to be similar in other respects as well. In his own interpretations of Star Carr (1954), Clark takes a wide range of cultural phenomena to be reconstructable on the basis of this principle. He formulates interpretive conclusions not only about the subsistence practices of this Mesolithic community but also about its demography, internal division of labor, and social organization, all on the basis of similarities between the environment and technology of Star Carr and those of the "hunting peoples of North America and Greenland" (J. Clark 1954: 12). His method illustrates the second of two principles for selecting analogs that resulted when, on Asher's account, the broad uniformitarianism of classical evolutionary theory was "partitioned" and "set in a restrained format" (1961: 318–319) by archaeologists who took up the challenge articulated by Kluckhohn.

Ascher takes these refinements of evolutionist reasoning to be a promising development and incorporates them into the first of his three proposals for "placing analogy on a firmer foundation" (1961: 322).[4] But at the same time he and later Orme (1974) acknowledge the parallel development of an increasingly pessimistic tradition of criticism of analogy. Its point of departure was a candid mistrust of these sanitized and restricted forms of analogical reasoning that was expressed even by their strongest proponents. Clark, among others, was quite explicit about his reservations: "we know from our knowledge of living peoples [that a] great diversity of cultural expression may be found among communities subject to the same economic limitations and occupying similar, if not identical environments" (1953a: 355). He was even prepared to recognize that this potential cultural diversity might undermine the reliability of folk culture analogies. He observes that "primitive" cultures and the "primitive" components of the "highly civilized parts of Europe" might them-

selves have a developmentally complex history; historical continuity may encompass profound change and this change could well affect even the most apparently stable and anachronistic aspects of the descendant cultures.

In response to these concerns, Clark suggested that whenever archaeologists appeal to folk culture analogs in interpretation, they should use a critical historical method to "strip away the civilized accretions and reveal the essential barbarian core" (1951: 57). Because this still admits of a worrisome degree of arbitrariness—Clark observes that "prehistorians are liable to select evidence from Folk-Culture which suits their own interpretations of the archaeological evidence" (61)—he recommends that the folk culture analogy be reinforced by establishing economic commonalities between the prehistoric subject and its historically connected analogs. "Economic history," Clark says, "forms a true connecting link" (61) on which archaeologists should rely as much as possible; descendant cultures can be considered a reliable analog for their forebears only to the extent that they retain the same subsistence patterns and technology (and presumably also live in the same environments).

These qualifications suggest that historically connected analogies are, after all, on the same footing as unconnected analogies; they are subject to the same adaptationist criteria that Clark proposes for selecting new, unconnected analogs.[5] And in that case, Clark's reservations about new analogy must also be taken to apply to folk culture analogies; both are vulnerable to error, because cultures may diverge sharply in their responses to any given set of economic or ecological constraints. This observation underlines the inescapable fact, which has counted heavily with the critics of analogy, that none of the criteria for selecting analogs—neither historical connection nor economic and ecological similarity—can guarantee that the complex association of traits characteristic of a prehistoric culture will be found in any contemporary cultural context. M. A. Smith, a British critic whose position is discussed by both Ascher (1961: 322) and Orme (1974: 203), puts this concern in particularly stark terms. She argues that once the full extent of ethnographic diversity is recognized, it must be conceded that "between the human activities we should like to know about and their visible results there is no

logically necessary link"; consequently, it is "a hopeless task [a matter of 'logical alchemy'] to try to get from what remains to the activities by argument" (M. A. Smith 1955: 6). With this sort of criticism, worries about notoriously bad uses of analogy, specifically those supplied by classical evolutionists, are once again generalized. Because there seem to be no principles of connection or selection that can guarantee the credibility of analogical inferences from present to past cultural forms, all such inference is considered highly and equally questionable.

THREE RESPONSES TO SKEPTICAL DOUBTS

Although similar skeptical doubts were articulated with increasing clarity by a number of critics in the 1950s, they did not deter Clark and many of his contemporaries; they offered a number of additional proposals for improving the credibility of analogical inference, three of which are significant. One extends Clark's initial response to evolutionary theorizing; it is a matter of further restricting the interpretive principles that govern appeals to analogy. Two others represent new departures: the second emphasizes the need to improve the source material, the repertoire of analogs, on which archaeologists draw in constructing analogical interpretations; the third is a recommendation that analogical hypotheses be tested against the surviving evidence of their prehistoric subjects.

As an example of the first of these strategies, consider Christopher Hawkes's recommendations for further limiting the selection of analogs. Hawkes is deeply mistrustful of interpretation that depends on "ideas of anthropological 'process' or of ecological determination" (1954: 160). He recommends that archaeologists base their reconstructive hypotheses on historical and quasi-historical "modes of cognition" whenever possible; they should always seek "some point of reference within the historical order" (160), as a source not of direct historical analogs but of documentary evidence bearing directly on archaeological subjects. When they deal with cultures that lie beyond even the most extended historical "diffusion sphere," Hawkes proposes that anthropological and ecological principles of interpretation be qualified by the recognition that as the researcher moves away from the reconstruction of strictly technical or technically determined realms, the reliability of the interpretive inferences drops dramatically. The more autonomous an aspect of culture is of physical, natural constraints—"the more specifically human are men's activities" (C. Hawkes 1954: 162)—the greater the scope for a "diversity of cultural expressions" (J. Clark 1953a: 355). And such diversity makes it "harder to infer" anything about past cultures without the benefit of textual documentation in which the specifically human, intentional component of these forms of life is, Hawkes presumes, directly revealed: "the more human, the less intelligible" (1954: 162).

In these passages Hawkes takes Clark's concerns about cultural diversity to heart: even when cultures share a common environment and are technologically and economically similar, they will not necessarily conform to a distinctive pattern of social response or cultural expression. Although Ascher regards this stance as an admission that "the new analogy is ineffectual in important areas" (1961: 321), Hawkes did not conclude, with Smith, that the interpretive reconstruction of prehistoric cultures is entirely hopeless or, more to the point, wholly insecure. Hawkes responded to these worries by attempting to determine just how far formal, material analogies can reliably carry interpretive inference when one appreciates that different aspects of culture are liable to different degrees of divergent variability. He treats the physical science–based reconstruction of technology as itself the primary and most reliable form of inference available to archaeologists. Ethnographic data can then be used to postulate, with decreasing reliability, the subsistence practices and economic systems, and the sociopolitical and spiritual-religious institutions, that may have been associated with that technology in prehistoric contexts. Rather than simply seeking analogs in cultures that existed under similar conditions, Hawkes relies on a "ladder of inference" to suggest that archaeologists using analogical inference should be discriminating about the aspects of past cultures that are inferred on the basis of material similarities.

A quite different response to the problem of controlling analogical inference was developed in connection with the "direct historic approach" advocated by North American archaeologists in the 1930s and 1940s. Strong and Steward, among others, recommended an inferential strategy of

working progressively back from the historically, ethnographically known to the unknown by using a combination of resources to construct a "sequence of roughly sequential [antecedent] epochs" (Strong 1942: 393; see also Steward 1942: 337).[6] Like Hawkes and Clark, the advocates of the direct historic method recognized that historical continuity in the same environmental context does not guarantee the similarity of prehistoric and historic or ethnographic cultural expressions. But unlike Clark and Hawkes, and in anticipation of proposals later made by the New Archaeologists, they insisted that such reconstructions should not be accepted solely on the basis of the plausibility of the interpretive arguments used to generate them; they should be tested archaeologically. In this spirit, Strong argued that "archaeological research can correct as well as confirm hypotheses derived from ethnological data" (1936: 363), and in his own research he was able to demonstrate just how effective a tool it can be in exposing errors of interpretation caused by mistaken analogical assumptions.[7] His practice, as much as his proposals, vindicates Clark's optimism that analogical inference can be systematically evaluated and strengthened despite the fact that "all analogies are very approximate and to a large extent subjective" (J. Clark 1953b: 241). Clark also sometimes emphasizes the role that archaeological testing can play in exposing error in and selecting among analogs. While ethnographic comparisons can "prompt the right questions"—indeed, their "main function is precisely to stimulate and give direction to prehistoric research"—he argues that it is "only archaeology in conjunction with the various natural sciences on which prehistorians freely draw [that] can give the right answers" (1953a: 355).

By the time Ascher published his 1961 synthesis of strategies for improving archaeological uses of analogy, a third complementary strategy for reinforcing new and historical modes of analogical interpretation had taken shape. It is a matter of strengthening analogical arguments by enriching the stock of analogs available as a basis for interpretation and it was, at least implicitly, a response to the practical objection that analogical inference was frequently weakened by a "paucity of ethnographic studies in areas relevant to archaeology" (Orme 1974: 205). Ascher himself provides the fullest explication of this strategy. He proposes (in the second of his three suggestions; see n. 4) not only that archaeologists should make fuller, more systematic use of the existing ethnographic literature but that they should undertake to fill the gaps in this literature. In particular, they should develop their own ethnographic studies of the "process[es] of continuous change" by which living communities create, use, recycle, and discard material things that thereby gradually "becom[e] . . . archaeological data" (Ascher 1961: 324). It is this sort of study (later referred to as actualistic research) that "holds the most fruitful promise for analogy in archaeological interpretation" (324). However tightly archaeologists restrict the interpretive principles that govern their selection of analogs, in the end it is the quality of the ethnohistoric sources on which they rely that determines how systematically they can compare source and subject, and how effectively they can assess assumptions of relevance.

Similar proposals for upgrading archaeological interpretation by improving its sources had been made five years earlier by Kleindienst and Watson (1956); these were presented as an extension of Taylor's conjunctive approach (1967 [1948]) and of initiatives taken by Raymond Thompson in his pioneering study of Yucatecan pottery production (1958). While Ascher identifies Kleindienst and Watson as proponents of ethnoarchaeological research who share his enthusiasm for its potential to improve archaeological research, he does not discuss Thompson's ethnoarchaeological research; he considers Thompson only as a critic of analogy who insisted that archaeological interpretation is irrevocably subjective. This omission is interesting, inasmuch as Thompson's explicit reason for undertaking his Yucatecan ceramics study was to "contribute to our understanding of the processes, limitations and potentialities of inference in archaeological research" (1958: 30).

THE SUBJECTIVIST CHALLENGE

On Thompson's account, analogical inference plays a role in the "probative" phase of archaeological research, in which archaeologists evaluate the interpretive conclusions generated in an initial "indicative" phase. They first identify evidence ("indications") that some aspect of their data was associated with "a particular range of sociocultural behavior" (1956: 329), and then determine whether it is plausible that such a correlation

could have obtained in an archaeological context by checking to see if it has an analog in any known ethnographic context.[8] Although Thompson regards ethnographic analogy as a crucial check on hypothesis formation, he also considers the probative phase unavoidably subjective. Whether or not an ethnographic counterpart is found for an archaeologically indicated correlation depends on how archaeological material is described and categorized, itself a matter of subjective judgment.[9] Thompson concludes that no amount of empirical or methodological rigor can eliminate this element of subjectivity; in the end, the credibility of reconstructive (analogical) hypotheses must depend on an assessment of the competence of the researchers who propose them.

By contrast to Ascher, Thompson suggests that improvement in the source material available to archaeologists promises only to put them in a position to intuitively grasp (to find indicated) and to justify (to find anthropologically plausible) a wider range of possible interpretations of their data. Indeed, there is a sense in which expanding the repertoire of interpretive options, while enhancing the credibility of the pool of candidate hypotheses as a whole, may make the selection of any one (analogical) interpretation from among them more rather than less arbitrary. Ascher thus judges Thompson to have "abandoned hope of making any impartial judgment of the reasonableness of an archaeological interpretation" (1961: 321). He offers a general argument against these skeptical conclusions, making the case that careful assessment of the closeness of fit or historical connections between prospective analogs and an archaeological subject does provide a basis for systematically assessing the "degrees of likelihood" associated with a range of interpretive options. The process he recommends is to eliminate interpretive options until a best solution emerges; "solutions to any problem are at best approximations arrived at by the elimination of those least likely" (323). Ascher therefore finds Thompson's skepticism unpersuasive: "If a systematic approach were used . . . and the alternative solutions for a particular situation stated instead of the usual statement of a single solution . . . there would be no need to examine credentials . . . but only the argument and the result. There is no touch of alchemy in the procedure outlined" (323).

Viewed in this light, Ascher's advocacy of eth-noarchaeology is motivated by a commitment to provide not just a wider range of interpretive options but also the grounds for weighing these options—assessing their residual uncertainty against specifiable background information and determining what additional information, archaeological or ethnographic, is needed to reduce uncertainty. Although Ascher does not make this argument directly, he clearly assumes that ethnoarchaeological research is capable of establishing facts about the behavioral and other processes responsible for an archaeological record that are not entirely an artifact of subjective judgment. There may be considerable flexibility in how a researcher characterizes cultural properties and processes when setting up comparisons with archaeological data, but the choice of typological categories is not, for all that, as arbitrary as supposed by Thompson. This is not to say that Thompson's subjective element can be eliminated from all the levels of inquiry where he finds it in evidence; but Ascher's analysis does suggest that ethnoarchaeological research of the kind Thompson undertook on Yucatecan ceramic production can provide grounds for the systematic *inter*subjective assessment of analogical arguments.

As the critics of analogy all emphasize, analogical arguments are, by definition, ampliative; because the conclusions of these arguments claim more extensive similarities than their premises establish, they are always liable to error. What Ascher resists is the assumption, made by critics such as Smith and Thompson, that when a genre of interpretive inference falls below the level of logical certainty—when any example of this form of inference may be in error—then all such inference must be considered equally at risk of error. All that follows from a demonstration that analogical inference is always insecure is that analogical conclusions must be treated as tentative and held open to revision as archaeologists expand the background knowledge and archaeological evidence on which they are based. This response to the "chronic ambiguity [suffered by thinking about] analogy since the nadir of classical evolutionary simplicity" (Ascher 1961: 322) trades on an appreciation that archaeologists can and routinely do discriminate between more and less well-supported and credible interpretive arguments. On this reconstruction of Ascher's argument, then, its central tenet is that archaeologists should give

up the paralyzing demand for certainty and make fuller, more systematic use of the resources they have for assessing the relative strength and cogency of analogical arguments.

THE UNDERLYING DILEMMA

The extended dialectic of argument and counterargument about the promise and pitfalls of analogical reasoning is yet another context in which archaeologists have negotiated the interpretive dilemma set out in earlier chapters.[10] As DeBoer and Lathrap describe it, this is "the familiar quandary of choosing between a significant pursuit based on a faulty method or one which is methodologically sound but trivial in purpose" (1979: 103; see also Klejn 1977: 6–11). When analogy is the method in question, "Either [the archaeologist] becomes a practitioner of an overextended uniformitarianism in which past cultural behavior is 'read' from our knowledge of present cultural behavior, or he must eschew his commitment to understanding behavior altogether and engage in a kind of 'artifact physics' in which the form and distribution of behavioral by-products are measured in a behavioral vacuum" (DeBoer and Lathrap 1979: 103).

Each critical reaction against analogy and each ameliorating response articulated in the 1930s through the 1950s represents an attempt to come to grips with this dilemma. Early critiques of evolutionist theorizing typically affirmed the major premise of the dilemma and, on Kluckhohn's assessment, most archaeologists felt compelled to embrace its "trivial but safe" horn, avoiding any form of interpretive inference that might risk error or speculating beyond the archaeologically given data. Those who accepted Kluckhohn's point and recognized that this risk-minimizing approach is ultimately untenable took up the challenge of demonstrating that analogical inference could be fortified against the notorious failings of evolutionist interpretation. The strategies they explored include the proposals made by Clark and Hawkes for restricting the selection of analogs; the arguments for improving the background knowledge on which interpretations are based, advanced by Ascher and by Kleindienst and Watson; and methods, like those exploited by Strong, for checking specific postulates of similarity archaeologically.

By contrast, critics such as Smith and Thompson reaffirm the intransigence of this dilemma and its assumptions about the limits of archaeological interpretation. If they consider analogy to be dispensable, they embrace the first option and abandon interpretive ambitions as unrealizable. But if, with Thompson, they believe that analogical inference and its associated subjectivity is an unavoidable feature of all archaeological inquiry, they endorse the speculative horn of the dilemma. Ascher responds to these skeptical conclusions by providing a synthesis of emerging wisdom about methodological options that avoid the skeptic's dilemma.

THE (NEW) REACTION AGAINST ANALOGY

ELIMINATION STRATEGIES

In the decade immediately following Ascher's synthesis, a new and uncompromising reaction against analogy took hold. It was an outgrowth of the New Archaeologists' conviction that "nontrivial" ends could be pursued without resort to any form of inductive inference; analogical reasoning was just one especially prominent and problematic example of the inductivism associated with traditional archaeology. In this spirit, Lewis Binford rejected out of hand Ascher's ameliorating suggestion that the security and credibility of analogical arguments might be improved by enriching their ethnohistoric sources. It is a mistake, he insists, to assume that by "'placing analogy on a firmer foundation' we could in any way directly increase our knowledge of archaeologically documented societies" (1967: 10). No amount of improvement in the understanding of ethnohistoric contexts will establish the empirical credibility of an interpretive hypothesis about the past; such credibility can be gained only by a program of (deductive) archaeological testing. By relying on knowledge of the present, Binford objects, "we are painting ourselves into a methodological corner" (1968a: 14).

Although Binford did subsequently come to an appreciation of the (inductive) complexity of archaeological testing (see chapters 4 and 7), his uncompromising hostility to analogical inference was widely shared. For example, Freeman insisted that analogical inference should be elim-

inated from all aspects of archaeological inquiry, including the formation of interpretive hypotheses: "an understanding of the archaeological residues" can and should be "based directly on the comparison of these residues" (1968: 262). Freeman never explained exactly how systematic comparison or analysis of the data could, in itself, transcend the level of a purely descriptive "artifact physics," and without such transcendence the threat of reverting to the "trivial but methodologically safe" horn of the dilemma is imminent. Gould took up Freeman's cause against analogy a decade later and addressed the residual problem of specifying how one might move from the analysis of "residues" to an understanding of their cultural antecedents without resorting to analogy. Unlike Freeman, he does not recommend that interpretive theory be formulated without any input from our experience and knowledge of contemporary situations; rather, he argues that these sources should be used as a basis for developing interpretive principles that "posit necessary relationships between the various kinds of observed evidence" (Gould and Watson 1982: 30). What Gould resists are appeals to unsubstantiated principles of "generic uniformity."

The sort of principles Gould has in mind are, primarily, laws established in the natural, biological sciences. He observes that "many principles developed in evolutionary biology and ecology can safely be assumed to have operated uniformly in the past as they do in the present," and insofar as human behavior is subject to these laws, it too conforms to certain uniformitarian principles: "do we seriously doubt that because people, along with everything else in nature, are subject to the effects of gravity today, they have been subject to these same effects in the same ways at all times and everywhere in the past?" (1980: 50, 112). The most secure inferential course open to archaeologists is to interpret their data by means of an "ecological connection": they should identify the physical, biological "limiting factors" that impose invariant constraints on human behavior; isolate the "aspects of human behavior that are most closely related to [them]" (50); and then use these as a basis for formulating hypotheses about the broad behavioral complexes that must have characterized particular human populations in the past given the conditions under which they lived. The inference from present to past is thus mediated by

"genuine" uniformitarian principles—principles that have been firmly established in the natural and biological sciences—and it projects onto the past only the antecedents of invariant regularities that exist in the biologically, physically constrained dimensions of human behavior.

As Gould himself acknowledges, however, the range of human behavior that is constrained in this way is extremely limited. The principles he invokes take archaeological interpretation decisively "beyond the realm of analogies and into a different order of discourse at the level of general principles" (1980: 112) only in the limiting case of behaviors that are uniquely and directly a consequence of ecological or material conditions. Such conditions might include, for example, a restrictive natural environment that sharply limits the options for survival of a population (given their technological capabilities), or physical constraints on producing a particular type of artifact that allow for only one production technology, or properties of a finished artifact that ensure that evidence of wear could be produced by only one pattern of use. But even in these cases it cannot be expected that complete (deductive) explanatory closure will be realized. It is an open and contentious question how closely material, ecological factors determine human behavior: the network of interacting variables is always complex; cultural groups often actively modify the features of their environment that Gould cites as "limiting conditions"; and when the environment allows any latitude in adaptive response, nondeterministic cultural factors immediately come into play.[11]

Although Gould, unlike more resolutely reductive theorists,[12] is an enthusiastic advocate of ecological modes of interpretation in archaeology, he is sympathetic to the point that much human behavior may not be directly or comprehensively explicable in terms of ecological constraints. "Human beings," he insists, "are not particles or inanimate entities whose behavior can be explained solely in relation to general laws like those used in the physical sciences"; moreover, it is unacceptable to restrict inquiry, for the sake of methodological purity, to just "those aspects of behavior that can be reliably covered by laws" (1980: xi, 37). Consistent with this position, Gould qualifies his endorsement of law-mediated interpretation, noting that humans are able to evolve "traditional skills, knowledge, and technology [that] can all serve to

overcome . . . limiting factors" and can, in fact, "act as limiting factors" in themselves (53). Indirect reasoning through an "ecological connection" serves to establish the parameters within which contingent, idiosyncratic patterns of cultural behavior can emerge; for any finer-grained anthropological understanding of past behavior, ecologically informed reconstruction will have to be supplemented by other modes of interpretation. Gould proposes that this supplement take the form of a method he describes as "argument by anomaly."

Gould sets up just as sharp a contrast between "argument by anomaly" and analogical forms of inference as he had between analogy and his method of indirect reasoning by ecological connection. And yet, as he describes them, arguments by anomaly are thoroughly analogical. Gould argues that although much human behavior is not determined by biophysical conditions, it can be treated as significantly "like" the adaptive behavior of nonhuman, biological species in its outcome. The behavioral patterns that emerge can largely be explained "as if" they were ecologically adaptive, like the directly conditioned behavior of biological entities; they are one component of a comprehensive strategy that functions to minimize the risks to population survival posed by environmental factors. Gould makes this interpretive principle explicit when he observes that "limiting factors operate in the realm of human behavior and produce the same effects as they do upon species in nature" (1980: 109); for example, they impose limits on the size of populations that can survive in any given environment such that "even under the most optimal conditions, the behavior of all people, everywhere, is constrained by limiting factors of some kind in the past as much as in the present" (111). Moreover, he acknowledges that this principle, which is formulated in biological contexts as the "principle of the limit" (52), is imported to archaeology on the basis of a comparison (which is analogical) between humans and other biological species. Both types of population, he argues, are implicated in a complex net of causal relationships ensuring that, as in the case of insecticide poisoning,[13] they will be affected by perturbations in other (material, biological) components of the encompassing ecological system no matter how isolated or culturally insulated they may seem to be.

Gould then elaborates several "principles about

human adaptation in general" (1980: 109), drawing out the implications of biological limit theory for a species that has unique social and ideational resources to deploy in its accommodation to biophysical constraints. He notes, for example, that the more imposing the risk created by a particular limiting factor, the more extensive will be the socially mediated response to it, and he describes general conditions—relative freedom from stress—under which such technological elaboration, or "optimizing behavior," will occur (110). These principles, which Gould believes should be capable of accounting for all aspects of human behavior, constitute a baseline for archaeological interpretation: they specify the behavioral patterns that would be most rational from an ecological point of view under a range of biophysical conditions. The "method of anomaly" is a procedure by which archaeological evidence is used to test for areas in which actual past behavior deviates from eco-utilitarian expectations. If the anomalies identified by these means cannot be explained in ecological terms, then, Gould suggests, an appeal may be made to ideational factors. But there is very little that Gould does not think will yield, ultimately, to explanation in terms of limiting factors and ecological-utilitarian rationality: "behavior that might appear maladaptive at one level of interpretation . . . may be viewed as adaptive at another level" (principle 4, 1980: 109); in his own case studies, even the most arbitrarily symbolic aspects of behavior are understood to serve some role in articulating the human population with its environment.[14]

Gould's alternative, non-analogical method of interpretation thus consists of two components, both of which are analogical. The first, indirect reasoning by ecological connection, allows for law-mediated reconstruction and explanation of those aspects of past behaviors that are directly conditioned by biophysical "limiting factors." Although this approach may be non-analogical in its limiting cases, in most applications it will depend on analogical inferences about the adaptive nature of human behavior that become explicit in Gould's second strategy. On his "argument by anomaly," cultural behaviors that cannot be explained by direct appeal to limiting conditions are to be treated "as if" they were adaptive and "as if," therefore, they can be reconstructed and explained in functional-ecological terms; they may not directly man-

ifest an eco-utilitarian rationality, but the analyst should search for ways in which they indirectly and ultimately serve the biological ends of the human population.

In the end Gould can claim that his own methods are non-analogical only by fiat of definition; he identifies analogical inference exclusively with the kind of uncontrolled, single-source analogical arguments that prompted the reaction against analogy in archaeology.[15] He justifies this move by appealing to dictionary definitions that establish, he claims, that to characterize his own law-mediated method of interpretation as analogical would be "stretching the concept of analogy far beyond its logical or commonly accepted meaning" (Gould and Watson 1982: 25). In fact, the analyses of analogical reasoning that are standard in logic textbooks and in the literature on informal logic offer a much broader view of analogy than Gould allows. This is a matter of more than just semantic interest. These standard accounts of analogy bring into clear focus a number of similarities between the narrowly defined forms of analogical inference that Gould rejects and the forms of inference he considers non-analogical, making it clear that all are ampliative. Such similarities imply that Gould's interpretive conclusions do not enjoy a special (deductive, nonampliative) level of security. They also suggest that there are important continuities between Gould's proposals for strengthening interpretive inference and those developed in previous rounds of debate about the role of analogy in archaeological reasoning.

DEFINING AND DEFENDING ANALOGICAL ARGUMENT

THE "LOGICAL MEANING" OF ANALOGY

Accounts of the logic of analogy typically begin with the observation that analogical inference consists of the *selective* transposition of information from the source to the subject of the analogy on the basis of a comparison that, fully developed, specifies how the terms compared are similar, different, or of unknown likeness. To use the terminology introduced by Keynes (1921), and elaborated in important ways by Hesse (1966), these dimensions of comparison establish the positive, negative, and neutral components of an analogy (see also Achinstein 1964). When Gould claims that analogies are based only on similarities, he

departs significantly from standard logical usage: according to these sources, which Lewis Binford introduced to the archaeological literature in 1967, the premises of an analogical argument establish a relationship of partial similarity that involves a consideration of differences as well. In fact, one recurrent theme in the philosophical literature on analogy is that it is a "glaring error" (Bunge 1973: 130; see also Bunge 1969) to claim that analogy is exclusively a relation of similarity. Fischer describes it as a fallacy—the fallacy of "perfect analogy"—and insists that arguments based only on a consideration of similarities either appeal to a relationship of identity or homology, and thus are not analogical at all, or are examples of "false analogy" (1970: 259). A well-formed argument by analogy proceeds from observations about similarities and differences, specified in the premises, to the conclusion that some specific aspects of the neutral analogy may, in fact, constitute further points of positive analogy (see also Scriven 1976: 210–215; Mackie 1972: 175). The justification for the conclusion about further similarities, and hence the strength of the argument as a whole, depends on the nature of the comparison presented in the premises.

At its simplest, the comparison supporting an analogical inference is a purely formal, point-for-point assessment of similarities or differences in properties of the source and subject.[16] Interpretive conclusions are drawn, in this case, on the basis of an assumption that when two objects share some properties, they may be expected to share others; such arguments are entirely indiscriminate with respect to what properties may constitute the additional (underdetermined) positive analogy. It is this sort of inference that concerns Gould and Freeman and earlier critics of evolutionary theorizing; as they point out, such arguments are justified only insofar as a suppressed premise—a principle of uniformity—can be assumed that affirms that the patterns of association observed among properties in familiar contexts hold for all contexts. Otherwise the similarities between source and subject may be entirely accidental and not indicative of further similarities.

These weakest, most tenuous cases do not, however, exhaust the full range of arguments that are analogical in form. Analogical comparisons generally incorporate considerations of relevance that bring into play knowledge about underlying

"principles of connection" that structure the association of properties in source and in subject.[17] As Copi puts it, "Although there may be disagreement about what analogies are relevant, that is, what attributes are relevant for proving the presence of certain other attributes in a given instance, it is doubtful that there is any disagreement about the *meaning* of relevance. . . . One attribute or circumstance is relevant to another, for purposes of analogical argument, if the first affects the second, that is, if it has a *causal* or determining effect on that other" (1982: 400; emphasis in the original).

Considerations of relevance enter analogical arguments when analogs are compared for the relations that hold among the properties they share rather than for the simple presence or absence of these properties. Analogies that incorporate considerations of relevance of this kind are typically "relational" analogies. As Uemov argues (1970), a number of different sorts of relational comparison are possible. The relations compared may be formal or they may be relations of proportionality; they may be contingent relations of constant conjunction, or they may be relations of functional-structural or causal-consequential dependence, these last being the sorts of connections Copi invokes as the basis for considerations of relevance. The most compelling relational comparisons establish that source and subject not only share properties but are also similar with respect to the "determining structures" (Weitzenfeld 1984: 143) that are responsible for the presence and interrelationships of those properties. When an understanding of these determining structures is fully developed and a "complete theoretical account is available" for the subject domain (Shaw and Ashley 1983: 430), it may be possible to replace analogical inference with a (deductive) theoretical explanation. It is this possibility that has inspired the most recent reaction against analogy in archaeological contexts. It is striking, however, that in the context of philosophical debate about the status of analogical arguments, Shaw and Ashley identify archaeological interpretation as precisely the sort of case in which analogical inference is likely to stand, observing that "many useful analogical arguments (e.g., those made by an anthropologist about social functions in a 'primitive' tribe) occur which we are not at all in a position to

replace with a full explanation" (1983: 431; see Wylie 1988b).

Although Shaw and Ashley do not develop this philosophical intuition in any detail, the archaeological debate suggests at least two reasons why reasoning by analogy is likely to be a persistent feature of archaeological interpretation. In the first place, however well-established anthropological theory (or, for that matter, psychological, sociological, or ecological theory) may be, its application to an archaeological subject is always a matter of extending an established theory to new domains; and as Hesse (1966) and others have argued, such an extension depends fundamentally on analogical reasoning. This point has been made in archaeological contexts not only by skeptical critics like Smith but also by the most optimistic proponents of analogy, like Clark, who acknowledge that a common determining structure cannot be assumed to hold even when a source and subject are historically connected or are subject to the same ecological constraints. This concern reappears in more recent literature in the guise of objections to the implicit functionalism and eco-determinism associated with the New Archaeology and evolutionary archaeology.

The second reason why even the most theoretically robust inferences about the cultural past are likely to be irreducibly analogical is that far from being a potential basis for interpretation, the connections between material and behavioral or other cultural variables—the determining structures or relations of structural and functional interdependence—are just what archaeologists cannot observe directly; they are among the features of past cultural contexts that must be reconstructed inferentially. While this epistemic opacity rules out the possibility of establishing a direct relational analogy, it does not follow that archaeological inference must rely solely on what Uemov describes as a purely formal, superficial analogy of properties (1970: 271). A consideration of causal and functional relations as they hold in source contexts can provide an understanding of how the properties compared between source and subject contexts are produced and under what conditions they can be expected to co-occur. Even if this analysis does not establish that the subject *must* be similar in further specific ways to known source contexts, it can support a reasoned assessment of

the relevance of known to inferred similarities, and it can inform sharply focused testing for evidence that the determining structure linking these properties in known contexts could have obtained (or, with specifiable likelihood, did obtain) in the subject context.

In short, the literature on the logic of analogy delineates a continuum of types and strengths of analogical inference, ranging from those that are based on a formal comparison for similarities in the presence or absence of discrete properties to those in which a comparison is made for similarities that, to varying degrees, can be shown to be a *relevant* basis for inferring further similarities. Where relevance is established by an appeal to principles of connection that hold between the properties compared and those inferred, analogical arguments at the relevance end of the spectrum incorporate precisely the sort of information that Gould took to distinguish his indirect reasoning by ecological connection.

In its limiting case, where the causal, functional relations structuring the subject domain are captured by a well-established explanatory theory, this continuum of types of analogy may give way to non-analogical forms of inference: formally valid (deductive) inference from known to hypothesized properties based on lawlike principles of connection. Although Gould's ambition is to bring about this final transformation of archaeological inference, it is not likely to be realized by his proposed methods of inference; they fall well within the ambit of analogical forms of inference and can be expected to remain analogical for reasons Gould himself sets out in some detail. The irony is that insofar as Gould succeeds in showing that his methods can raise interpretive inference above the level of speculation, he demonstrates that analogical inference can escape his own charge that it is radically faulty.

CRITERIA OF STRENGTH IN ANALOGICAL ARGUMENT

There are a number of ways in which analogical forms of inference can be systematically strengthened and evaluated. In reviewing them my aim is to identify two basic strategies for assessing analogical arguments that are implicit in standard criteria for evaluating analogies, and to show that these strategies are fundamental to the proposals for improving archaeological inference made both by Gould and by the proponents of analogy. I will thereby prepare the ground for making the case that the second radical objection to analogy—that it is not only insecure but also unavoidably distorting of what we can understand of the past—can be decisively turned.

Even when analogical arguments are based primarily on a comparison for the extent of similarity (rather than the relevance of similarity), a number of criteria can be used to determine their relative strength. Consider a case discussed by Merrilee Salmon (1982: 60–63) that illustrates the value and also the pitfalls of these criteria: the analogical interpretation of stone gorgets proposed by Curren (1977: 97–101). Curren suggests that these groundstone artifacts may have been pottery-making tools, and he supports this interpretation by noting that an extensive positive analogy holds between modern potters' tools (or ribs) and the gorgets, particularly with respect to their shape and edge treatment; all are thin with curved and beveled or serrated edges and central perforations. He also takes into consideration the primary negative analogy—that potters' ribs are never made of stone, the material of which most gorgets are made—arguing that this may not be a significant difference, because modern potters use ribs made of such a wide variety of materials, including wood, metal, and bone. Whatever plausibility this initial analogy enjoys derives from its being based on a systematic comparison of source and subject that establishes not only a number of similarities between them but also weighs these similarities against the differences. These considerations (of the extent and proportion of similarity) provide a measure of the degree of fit between the source (or analog) and the subject of interpretation; together they constitute the primary criterion for evaluating a formal analogy.

A quite different criterion is at work when Curren turns the observation of dissimilarities to his advantage by showing that the correlation between morphological and functional attributes—between the known and inferred similarities—holds consistently across a wide range of source contexts despite variability in the materials of which potters' tools are made or in the type of ceramic production involved. Rather than expand the comparison for similarities between a particu-

lar source and the subject of interpretation, Curren expands the range of sources on which he draws, identifying a narrowly circumscribed correlation of properties that holds however much the sources differ in other respects. A final criterion concerns the relationship between the premises and the conclusions of an analogical argument: the overall strength of the argument will be improved to the extent that the similarities inferred in the conclusion are modest relative to the breadth and specificity of those cited in the premises. The standard criteria for evaluating formal analogies are thus the number and the extent of the similarity between source and subject, the number and diversity of sources cited in the premises in which known and inferred similarities co-occur as postulated for the subject, and the expansiveness of the conclusions relative to the premises.

When Curren takes into account the dissimilarities between gorgets and potters' ribs, he moves beyond a purely formal comparison and begins to introduce preliminary considerations of relevance. In fact, I argue that the formal criteria for evaluating analogical arguments just outlined are a good measure of their strength precisely insofar as they direct attention to two patterns of correlation that provide researchers preliminary evidence that an underlying principle of connection may hold between the properties shared by source and subject and the further properties observed in the source that are attributed, on this basis, to the subject. There are, then, two distinct strategies for strengthening an analogical argument. The first is to broaden the base for interpretation with the aim of identifying clusters of attributes that reliably co-occur, as Curren does when he considers a range of different pottery-making tools and practices. The invariant association of the key attributes of shape and structure that Curren identifies with potters' ribs provides him with the basis for arguing that these properties are deliberately selected for or created because they meet the functional requirements of ceramic production. And this argument suggests, in turn, that where these properties reliably co-occur in prehistoric gorgets as well as in potters' ribs, they are a relevant basis for inferring that their uses may have been the same.

When pressed, comparisons that establish extensive similarity between a particular source and subject offer a second strategy for strengthening

analogical arguments that pulls in a quite different direction. To cite a different example: Hill defends the initial plausibility of the hypothesis that prehistoric pueblo room types served the same functions as their analogs in contemporary pueblos on the grounds that "the similarities between the suspected analogs are so great that they almost cannot be coincidental" (1966: 15). In other words, the mapping of source onto subject is so complete, with respect to properties that can be compared, that it seems likely that a relational analogy underlies the formal analogy. If the implicit principle of connection were made explicit, it would be similar to Curren's; the distinctive form of pueblo rooms reflects an intention to use them in particular ways that both informed their original construction and determined what activities took place in them when they were built and occupied.

Formal criteria can be deployed, then, in such a way that they serve as surrogates for direct, relevance-establishing (or relevance-measuring) appeals to a relational similarity between source and subject. Such deployment yields a range of transitional forms of analogical argument that lie between those based on a purely formal comparison between source(s) and subject, and those that depend on well-established theoretical knowledge about the causal, functional relations that actually structure both source and subject. Fully developed, the arguments of relevance backing these latter analogies demonstrate that the same determining structures operate in both source and subject; given this relational analogy, they can be expected (in the limiting case, with deductive certainty) to manifest the same formal or behavioral properties. But even when considerations of relevance remain largely implicit, it is often possible to decisively rule out the worst-case instances of direct and arbitrary projection of present onto past that are responsible for the recurrent pattern of reaction against analogy. In the interpretations offered by classical evolutionists, for example, the formal comparison of contemporary "primitive" source contexts with prehistoric cultures is notoriously unsystematic. Dissimilarities between sources and subjects are rarely even considered, much less weighed against the similarities; and although a wide range of sources are cited, there is no attempt to demonstrate that specific configurations of attributes are invariant across them. The fragmentary similarities established in the prem-

ises fail to provide grounds for postulating an underlying relational analogy, let alone for assuming, as classical evolutionary theorists tended to do, the literal identity of the determining structures that are presumed to shape both prehistoric "primitive" cultures and their contemporary analogs. In short, the conclusions of classical evolutionary arguments so far overreach what the premises establish with respect to the similarities between sources and subjects that they exemplify Fischer's fallacy of simplistic analogy.

Clark's proposals for systematic formal comparison represent an improvement on these early evolutionist arguments inasmuch as they focus critical attention on the (uniformitarian) principles of connection—the relational assumptions—on which they depend. His own interpretation of Star Carr is interesting, in this connection, because it illustrates how the two strategies described above can be used together to evaluate a delimited version of these relational assumptions. He proceeds by observing that in the circumpolar source contexts he considers, the features of environment, technology, and resource base that they have in common are associated with distinctive sociocultural attributes: they show similar patterns of mobility, community size, division of labor, and internal social organization. Like Curren, he expands the bases for interpretation, but at the same time he draws on sources that are comprehensively like prehistoric Star Carr with respect to attributes (technological, ecological, economic) that can be compared across source(s) and subject; the sociocultural properties he projects onto Star Carr are those that are consistently associated with shared features of material culture and environmental context in ethnohistoric sources. These analogical premises are compelling to the extent that they suggest that the association between inferred and observed attributes is not accidental: organizational and demographic aspects of both source and subject contexts may have been shaped by the same kinds of adaptationist determining structures.

SOURCE- AND SUBJECT-SIDE STRATEGIES FOR ESTABLISHING RELEVANCE

The weakness of Clark's ecologically based interpretation and also of the folk culture analogies he recommends is that they provide no direct evidence for the crucial (implicit) relational analogy.

As the critics of analogy have pointed out, despite setting evolutionary (adaptationist) assumptions in a "restrained" format, Clark still depends on assumptions about the uniformity of human response to environmental conditions that are controversial, given counterexamples introduced by Clark himself. The trouble with interpretative arguments governed by Clark's adaptationist criteria is that they are incomplete; they represent a refined use of formal analogy, but still they trade on an intimation rather than a demonstration of relevance. To move beyond this transitional form of analogical argument it is necessary to work aggressively on both sides of the analogical equation (P. Watson 1979: 281). Establishing principles of connection based on careful analysis of prospective sources—as important as this is both for the selection and for the evaluation of analogs—is not enough. In addition to determining what kind of determining structure may link particular material and cultural or behavioral variables, it is crucial to determine whether it is likely that this structure could have held (or did hold) in a specific past cultural context.

The New Archaeologists made it a priority to test hypotheses about the cultural processes and determining structures at work in both source and subject contexts because they saw such testing as a means of eliminating dependence on analogy altogether. At its best, this method illustrates how effective the combination of subject-side work with archaeological testing can be in establishing considerations of relevance as a ground, beyond formal comparison, for specific analogical conclusions. Consider, for example, how Hill proceeds in developing a case for his interpretation of pueblo room function. He first argues that archaeological room types are defined by precisely the formal attributes that would be required of any room that was specifically intended to serve the functions he attributes to them. For example, general living and food preparation activities, in contrast to storage functions, require the relatively larger spaces, the features ensuring light and ventilation, and the special facilities for food preparation that are typical of one subset of rooms in prehistoric pueblos identified. He thereby suggests that a determining structure linking form and function may account for the distinctive configuration of rooms in ethnohistoric pueblos, and the completeness of mapping between source and

subject suggests that this structure can be attributed to prehistoric pueblos as well. This claim is just a preliminary, however. What really strengthens Hill's account are his empirical tests for evidence that should (or could not) be present in the archaeological record if the prehistoric rooms had, in fact, been used for the purposes he ascribes to them. Contrary to Hill's claims, however, the results of these tests do not establish non-analogical grounds for his interpretive hypotheses. Rather, they serve to expand the range of the positive analogy that can be demonstrated to hold between source and subject in areas where similarities could not have been expected unless the test hypothesis and, specifically, the posit of a common determining structure (showing the dependence of form on function) were approximately true.

By contrast, Curren's interpretive argument is a negative object lesson that throws into even sharper relief the importance of testing relational assumptions archaeologically. He rests the case for his interpretive conclusions about the function of gorgets entirely on source-side arguments for the plausibility of treating common features of form as evidence that stone gorgets served as potters' ribs. Curren goes well beyond Hill in the use he makes of evidence that the actual use of ribs as ceramic tools (in contemporary contexts) depends primarily on their shape and that potters select for shape, more than anything else, as the feature that determines the functional value of the tools. Even so, in a rebuttal to Curren's argument Starna objects that by developing his interpretation this way, Curren "separated what are clearly two interdependent parts of a single process" (Starna 1979: 337). In particular, he failed to "take the next logical step" (337) of establishing that the principle of connection linking form to function could have held in the subject context. When Starna examined the relevant archaeological material, he found that stone gorgets frequently occur in archaeological contexts that are preceramic or show no evidence of ceramic production, decisively undermining any assumption that their formal similarity to potters' ribs constitute a relevant indicator of ceramic-making function.

Curren's analogical argument ultimately fails for want of attention to the question of whether archaeological evidence bears out his supposition that a relational analogy holds between its source(s) and subject; Starna demonstrates that

the requirements of smoothing and shaping pottery could not have been responsible for the properties of form that characterize gorgets in at least some of the prehistoric contexts where they occur. By contrast, Hill's analogical argument is compromised by his failure to make more systematic use of source-side resources to establish the principles of connection that he presupposes; he was among the New Archaeologists who were roundly criticized for testing ethnographically naive hypotheses about the organization and dynamics of pueblo societies (see chapter 4). Nonetheless, the test implications Hill drew regarding the activities associated with various types of pueblo room were precise enough to be disconfirmed by archaeological data in some key areas. Discrepancies between his expectations concerning the association of pollens and other plant remains with postulated food-processing areas were among the most valuable outcomes of the study, raising questions not about Hill's room function hypotheses but about the overarching model of prehistoric subsistence patterns that informed his study as a whole (see chapter 13 for further detail on these test results and their implications).

Fully developed as tests for relevance, the source- and subject-side strategies for improving analogical arguments suggested by the logic of analogy thus offer mutually reinforcing procedures for checking the adequacy of analogical arguments that build on earlier proposals for appraising the import and relevance of prospective analogs. Analogical reasoning cannot be eliminated from most contexts of archaeological interpretation, but these methods can ensure that the (analogical) transposition of information from source(s) to subject is discriminating and selective. They make it clear that analogical arguments need not be formulated as simple, indiscriminate projections of present onto past, and that they are not all equally and undifferentiably insecure.

THE VALUE OF MULTIPLE SOURCES: REBUTTAL TO CHARGES OF DISTORTION

The real value of relational forms of analogical inference is not just that they offer potentially stronger forms of interpretive argument but that they can be a source of strikingly creative insight about the cultural past. A source that shares as little as a single attribute with the subject in question may serve as the basis for a closely circumscribed re-

constructive argument, if there is reason to believe that a common determining structure links the properties that can be compared directly to those that are inferred. In this case an interpretive model may be built up by appeal to a number of sources, each of which brings into view specific features of the subject on the basis of evidence (direct or indirect) that they are generated by localized determining structures that operate in both source and subject. If the subject combines attributes in a configuration not duplicated in any one known context, the resulting model will be a unique composite of features, each characteristic of different sources but associated together in none of them: it will be what Harré describes as a "multiply connected" paramorphic model (1970: 47–49; see chapter 4 above). Such a model may be fully plausible, given background knowledge of familiar sources, but still may characterize a subject that is radically unfamiliar, unlike any single, accessible analog.

It is a telling and relevant irony that Gould illustrates his strongest claim against analogy—that it is inherently limiting of what can be understood about the past—with an interpretive account that is itself analogical and that concretely demonstrates the creative potential of drawing selectively on a diverse range of limited analogies. He argues that it is misguided to interpret the archaeological remains of early human populations, especially evidence of their home base and kill sites, on the basis of analogs drawn from contemporary hunter-gatherers; "early man" may have lacked the use of fire and this condition would have "changed the 'ground rules' for survival" (Gould 1980: 30). In particular, without fire to protect against other predators, it would have been dangerous to bring meat from a kill site back to a home base for social sharing. Gould thus suggests that the behavior of living, nonhuman carnivores should be used to supplement and correct background assumptions about the unique capacities of humans and about the ways in which contemporary hunter-gatherers treat the spoils of successful hunting. Early humans should be understood to have been in some respects "like" con-

temporary hunter-gatherers, and in others "like" nonhuman carnivores; to model their behavior it is necessary to draw on quite different sources for an understanding of what it means to be a hunter who lacks fire but has other distinctively human cognitive and social capacities. Considered as an analogical argument, *pace* Gould, this is a brief for using multiple sources to constrain one another and to suggest both what can and what cannot be assumed about a quite distant and unfamiliar subject. The resulting model posits a form of life that is radically different from that in any contemporary contexts but is yet conceivable, given knowledge about a range of analogs whose relevance to the subject is closely delineated.

Contrary to the claims of the perennial critics of analogy, analogical inference is not categorically faulty or misleading; there are a great many options that lie between the horns of the dilemma defined by a generalized skepticism about analogical interpretation. The criteria outlined here for evaluating analogies, and the associated strategies for strengthening analogical arguments, provide a basis for rejecting just the kind of false or overextended appeals to analogy—the indiscriminate assimilation of past to present, of unfamiliar to familiar—that have been the source of chronic ambivalence about analogy. Beyond this, they suggest ways in which analogical inference can be strengthened by a careful appraisal of dissimilarities as well as similarities and, most important, by a discerning use of source- and subject-side evidence to establish arguments for the relevance of specific similarities in observable properties to further, inferred (closely delimited) similarities between unobservable aspects of the cultural past and their counterparts in living contexts. These strategies will never establish interpretive conclusions with certainty, but they do offer a viable alternative to "artifact physics" on the one hand, and unconstrained speculation on the other. They are strategies for eliminating error and assessing likelihood, improving credibility and delimiting uncertainty, in a field in which the most interesting questions inevitably lead beyond the safety of clear-cut, empirically secure answers.

Putting Shakertown Back Together

Critical Theory in Archaeology

THE QUEST FOR RELEVANCE

"Contemporary archaeology," says Kohl in a review of the state of the field, "is nothing if not tortuously self-conscious" (1981: 108); and yet, as he observes, this self-consciousness has been curiously limited. In the context of North American archaeology it gave rise to a "vehement advocacy" of positivist methods for realizing objective knowledge of other (past) cultures, while in other social sciences it led to an intensely critical "question-[ing of] the possibility of impartial, value-free social science research" (93).[1] One reason for this divergence is to be found in the concern to make archaeology relevant that informed the appeal to positivism; if a method could be devised for reliably interpreting archaeological data as evidence of the cultural past, it might put archaeologists in a position to establish an explanatory understanding of long-term, large-scale cultural dynamics that has broad and even pragmatic value.

By contrast, the self-consciousness of sociology and social anthropology to which Kohl refers did not arise so much from worries about the relevance of the field as from a desire to take stock of the social and political interests that it was already serving, deliberately or inadvertently. Kohl notes with a touch of irony that while archaeologists were refining their methodology so that they could play

a role in "explaining the past and possibly directing future social change," social anthropologists were "acknowledging their discipline's unsavory relationship to colonialism" (1981: 92). They were also grappling with what Handsman (1980b) has described as a state of "twinship," an epistemological dilemma endemic to the whole anthropological enterprise of understanding other cultures. Such understanding depends on the possibility of rendering these cultures intelligible *to us,* and this process, it was realized, inevitably involves some degree of distortion—specifically, distortion that obscures crucial differences between the investigators' culture of orientation and the cultures they study. Taken together, these forms of self-consciousness reflect a growing awareness that the whole enterprise of systematically investigating other cultures is *itself* a culturally specific, social enterprise, one that is rooted in and shaped by the interests and belief structures that constitute the context of the researcher. It is this sort of self-consciousness that Handsman and Kohl find lacking in archaeology.

As their own comments suggest, however, critical self-consciousness about the social and political entanglements of archaeology rapidly took center stage in the 1980s. Handsman and Kohl anticipate what is now a widely expressed concern over the accountability of archaeology to its non-

academic clients and to the communities whose cultural resources are the object of its attention (see chapter 17). Archaeologists are also increasingly aware of the influence that social or political factors may have on the practice and products of their field, as evident in analyses of the ways in which selective recruiting and the internal social structure of the research community affects the research agenda and the interpretive claims that gain widespread currency (Kelley and Hanen 1988: 99–164; Gero, Lacy, and Blakey 1983; DeBoer 1982; see the introduction to chapter 15 below). Here, I consider a deeper and more broadly encompassing form of critical self-consciousness that takes shape in the work of Handsman and Leone, among others who have been influenced by Frankfurt School critical theory, the German tradition in neo-Marxist social thought famous for its critique of the "objectivist delusions" of positivist social science.[2] I first identify the main insights that Handsman and Leone draw from critical theory; these provide the framework for discussing an epistemological problem that this perspective raises about the possibility that archaeologists can acquire any knowledge of the past. I then consider several concrete examples in which Handsman and Leone put this critical perspective to work, on this basis developing an analysis of how established tools of inquiry can be used to meet the epistemological challenge posed by systematic critique of the taken-for-granteds that underpin archaeological research.

CRITICAL THEORY: THE CRITIQUE OF OBJECTIVISM

The primary concern of critical theorists associated with the Frankfurt School was to recover and build on Marx's insight that knowledge and knowledge-producing enterprises are grounded in "fundamental characteristics of the human species," in particular the socially based productive activity (labor) that serves the species' fundamental interest in survival (Keat and Urry 1975: 222). In foregrounding the pragmatic, interested nature of knowledge production, critical theorists challenged the objectivist pretensions of positivist social scientists, especially as embodied in commitment to a "doctrine of value freedom." As it is formulated by Popper (1976; see also 1972), who is identified by a number of critical theorists

as its chief exponent, this doctrine in effect posits that our understanding of social and natural reality will approximate to an ideal of objective truth if the research community as a whole adopts a persistently critical attitude to knowledge claims, treating them as conjectures to be systematically tested and accepting them only tentatively pending further, potentially falsifying, tests. Objectivity thus depends not on the attitude—the neutrality or clear-sightedness—of individuals but on a *communal* tradition of rational, empirical criticism; it is this that ensures that error will be systematically eliminated, whatever value biases or preconceptions particular researchers bring to their investigations. The critical theorists' objection to this view of the scientific enterprise is summarized by Habermas:

> In all sciences, routines have been developed that guard against the subjectivity of opinion and a new discipline, the sociology of knowledge, has emerged to counter the uncontrolled influence of interests on a deeper level, which derive less from the individual than from the objective situation of social groups. But this accounts for only one side of the problem. Because science presumes that it must secure the objectivity of its statements against the pressure and seduction of particular interests, it deludes itself about the fundamental interests to which it owes not only its impetus but the conditions of possible objectivity themselves. (1971: 311)

Habermas argues that both the reality a discipline presumes to investigate and the routines it develops to eliminate subjective bias in its understanding of this reality are a function of fundamental knowledge-constitutive interests that the discipline serves.

In elaborating this thesis, Habermas broadens Marx's original, narrow conception of a survival-based interest in knowledge that facilitates productive, instrumental action in the world to include two other knowledge-constitutive interests. One is what Habermas calls a *practical interest* in developing the kind of common knowledge of reality that will promote consensus among community members, making it possible to effectively coordinate action; this sort of interest is said to arise from the communicative, interactive aspects of human life. The other is an *emancipatory interest* in escaping the constraints that existing social forms impose on the individual. The latter fosters

a search for reflective self-knowledge and knowledge of the *actual* conditions of existence that underlie the distorted appearances perpetrated by knowledge systems built up in the service of unreflective technical and practical interests. On Habermas's account, then, *critical* emancipatory knowledge supersedes the first two forms of knowledge; it embodies the human capacity to reflect on and actively transform the manner in which the fundamental interests in survival and social organization are met.

The first of these three interests, the interest in instrumentally useful knowledge, gives rise to the empirical-analytic disciplines whose central aim is to provide reliable knowledge of *what* happens (or may be expected to happen) in the natural or social world. To achieve this kind of understanding, practitioners in empirical-analytic disciplines conceptualize the reality they study as observable, manipulable phenomena (i.e., phenomena that are amenable to prediction and control) and they make empirical verification the epistemic standard that determines what knowledge claims should be accepted as true of such a subject. By contrast, practical interests give rise to hermeneutic-historical disciplines whose primary objects of investigation are the intersubjective meanings that constitute the ground for what Habermas describes as "possible action-orienting mutual understanding" (1971: 310; see also 1973). They promote consensus in such understanding through an explication of key meaning-constituting narratives and conventions, subject to interpretive standards of plausibility and coherence. What these two forms of inquiry have in common is that in both, the role of knowledge-constitutive interests is systematically obscured; consequently, they serve to enhance established modes of production and to reinforce the supporting social order. As Smart puts it, they "replace common-sense understandings with scientistic descriptions which better serve the purpose of the legitimation and rationalization of the given social order" (1976: 174).

What distinguishes critical theory and other disciplines predicated on an interest in emancipation from conventional analytic-empirical and historical-hermeneutic forms of inquiry is their central aim: to "reveal the role of interpretation and action in reaffirming and modifying the categories [of objective understanding in terms of which we comprehend and thus act in the world] in order that human beings may realize the historical relativity of the alienated and estranged world in which they exist" (Smart 1976: 162). The emancipatory disciplines take subjectivity as a subject for investigation in its own right; they provide an understanding of the appearances and forms of life that uncritical disciplines take for granted and indeed reinforce by representing them as objective facts or consensus understanding. Predicated as it is on a sustained critique of objectivism, a commitment to emancipatory goals entails a radical reformulation of traditional epistemic ideals; if knowledge claims can never be assumed to transcend the conditions of their production, perhaps they can be judged only pragmatically, in terms of their effectiveness in promoting an interest in emancipation.

These distinctive features of critical theory emerge most clearly in a famous debate between Popper and Adorno in which Adorno objects that Popper's ideals of value-free inquiry threaten to reduce science to a purely cognitive enterprise, preoccupied with intellectual gap filling and the elimination of inconsistencies in our knowledge systems (Frisby 1972; see also Adorno et al. 1976; McCarthy 1978: 53–90; Horkheimer 1972). This approach, he argues, obscures the extent to which the contradictions exposed by rational, empirical criticism are themselves a product of interest-constituted modes of apprehending reality and reflect contradictions inherent in that reality. These internal inconsistencies cannot be treated as "logical contradictions which can be corrected through more refined definitions"; contra Popper's recommendations, "criticism cannot be confined to the reformulation of contradictory statements within the cognitive realm" (Adorno 1976: 113). It must take the form of social criticism and social action acknowledging that social research is both constitutive of and constituted by its subject reality.

As articulated in this debate, critical theory is critical in two senses. First, it involves critical reflection on the knowledge-producing enterprise itself: self-consciousness about the extent to which knowledge claims are conditioned by their social context, and self-consciousness about the ways in which inquiry and understanding serve interests that are constitutive of that context. Second, where this double-edged self-consciousness brings into view the social, ideological entanglements of scientific inquiry, it provides a basis for reflective un-

CRITICAL THEORY AND ARCHAEOLOGY: CRITICAL SELF-CONSCIOUSNESS

Handsman and Leone advocate both forms of critical engagement when they appropriate elements of critical theory as a framework for archaeological research.[3] In connection with criticism in the first sense, they draw attention to the fact that despite an entrenched preoccupation with accuracy, archaeologists typically reconstruct the cultural past in the image of contemporary, familiar forms of life in a way that both embodies and serves dominant social and political interests. Handsman, for example, shows that this can be the case even with straightforwardly empirical reconstructions of historical settlement patterns (1980a, 1981, 1982a, 1982b). New England historians had long and, he claims, wrongly assumed that early settlement in the area was nucleated on the model of modern settlement patterns. This error arose and persisted because of the further assumption that premodern and modern day (nucleated) settlement are the product of the same social and economic factors; both reflect the decision making of individuals who function as autonomous, economically motivated agents in acquiring and disposing of land. In fact, Handsman argues, premodern settlement patterns embody structuring principles that were fundamental to a diffuse, all-encompassing kinship system; these constitute a context and a rationale for action regarding land very different from those governing contemporary contractual exchanges. Handsman's critical conclusion is that the distinctive features of this past will necessarily be obscured if it is unreflectively reconstructed in terms of conceptual categories drawn from the present, especially when these concern structural relations among people, such as economic and kin relations, that are basic and context-specific.

Leone makes essentially the same point about the cultural relativity of archaeological reconstructions in an analysis of how outdoor museums present the past to the public (1980, 1981b, 1983). These reconstructions of the past are, he argues, a modern day "ideo-technic" artifact (1981b: 305); through them, existing social forms are interpreted and legitimated as the inevitable (natural)

outcome of a past that is, in the process, denied any independent reality as a source of contrast with and critical knowledge of the present. Museum reconstructions of this kind not only misrepresent the past, as was the case with the historical reconstructions discussed by Handsman; in their misrepresentations they serve the interests of the present. They mediate the self-definition and self-legitimation of those who create and view them.[4]

Given this understanding of the nature and function of outdoor museums, Leone (1980) argues that the reenactment of Catholic:Protestant tensions at historic St. Mary's City should be understood as distorting the past in ways that provide a forum for acting out tensions (economic, religious, and political) inherent in the present community. The museum serves as a kind of ritual context in which unresolvable contradictions in the present are articulated in historical terms and symbolically resolved. In a parallel analysis Leone (1981b) argues that the reconstruction of Shakertown at Pleasant Hill serves to neutralize the critical import of what was once a social experiment dedicated to *actually* resolving, or escaping, the repressive conditions of then-emergent industrial capitalism. This neutralization is achieved by systematically fragmenting Shaker culture and presenting it in terms that render it intelligible and acceptable to the contemporary viewing public. The Shakers are characterized as "efficient, profitable, logical and ingenious," a community in which culture "rises from function, behavior from efficiency, and thought from material necessity" (1981b: 305); in this regard they are understood to embody our own highly valued ideals of economic rationality. While such an account respects a "sort of narrow accuracy," it also marginalizes and trivializes those aspects of Shaker life that deviate from contemporary (white, middle-class, Euro-American) social norms, and from a naturalizing vision of the past as leading inexorably to forms of life predicated on these norms. Communal living and ownership of property, ecstatic religious rites, and community-wide celibacy are represented as exotic curiosities distinctive of a community that ultimately disappeared; indeed, these eccentricities figure prominently in explanations for the failure of Shaker communalism. This dismissal, Leone argues, obscures the significance of the Shaker form of life as the tangible expression of a commitment to "be humane

industrialists," to realize a "radical realignment of sexuality, work, family, and thought" (308); it systematically marginalizes what amounted to a profoundly critical living commentary on, and exploration of alternatives to, then-dominant forms of industrial capitalism. Shaker culture is thus made to serve as a "secondary rationalization of our own [culture]" (305, in reference to Sahlins 1976: 54); the dissolution of Shaker communities stands as a negative object lesson that warns against certain sorts of dissent or departure from the norms that structure life in the modern, industrial capitalist world.

On Leone's analysis, then, reconstructions of Shaker life serve what Habermas has identified as a practical (and not emancipatory) interest; it helps ensure that individuals embedded in contemporary U.S. society share an underlying system of beliefs—an understanding of "how society ought to work"—that will, as Leone puts it (drawing here on Althusser), "permit [them] to operate smoothly in the everyday world" (1981a: 10). Like Habermas, he understands this basic knowledge-constitutive interest to structure not only fundamental reality-defining concepts but also the criteria of adequacy that frame systematic (disciplined) investigation of the past. The whole preoccupation with accuracy in detail is itself "a culture-specific effort to resolve the paradox between an unalterable past and a past thought essential for our self-definition" (12).

CRITICAL THEORY AND ARCHAEOLOGY: SOCIAL CRITICISM

With these analyses Leone and Handsman adopt just the sort of critical, self-conscious stance that Kohl finds lacking in archaeology. But a problem quickly arises when they shift from critiquing the interested nature of our understanding of the past to articulating constructive proposals for a program of research that is informed by an emancipatory interest. Both argue that once archaeologists become conscious of the interests that both shape and are served by their practice, they should undertake to help "modern Americans reappropriate their past consciously" (Handsman and Leone 1980). Criticism in the first sense should serve as a basis for systematically reassessing current myths about the past; the goal should be to expose distortions inherent in historical, archaeo-

logical reconstructions and, where something of our own forms of consciousness is thereby revealed, it should be the point of departure for critique of the contemporary social conditions that give rise to them. This is, then, an argument for taking a critical stance in the second sense associated with critical theory—that of prospective social criticism. The brief for an engaged critical archaeology is expressed, in practical terms, in Handsman's and Leone's recommendation that archaeologists should be particularly concerned to retrieve those pasts, or aspects of the past, that are generally lost in unreflective reconstruction: the past as different from the present, as exemplifying alternatives to it, and as revealing the contingencies of its formation. The difficulty is, however, that this position presupposes the possibility of securing an epistemic vantage point that is objective in the sense that it supersedes other distorted, interest-relative forms of historical understanding and provides a measure of their accuracy. It was precisely this ambition that critical theorists decisively challenged, arguing (on the basis of critiques of the first sort) that the quest for a stance that transcends human interests is a vain pretension that can be sustained only by systematic (positivistic) denial of the real historical and social conditions of knowledge production.

These directives for a critical archaeology give rise to a variant of the dilemma that when a radical critique of objectivism is accepted, it seems to leave no grounds for preferring any one interpretation to its alternatives; if all are interest-specific, then perhaps each is legitimate relative to its own presuppositions. Consistently maintained, a stance of critical self-consciousness seems to leave archaeologists no option but to accept that "the past can be known only as a function of the present" (Handsman 1980c: 2), and to "allow the past to be the image of the present it must" (Leone 1981a: 13). Although Leone and Handsman acknowledge these implications of a systematically critical archaeology, for the most part they are resolute in resisting corrosive relativism; they insist that in urging archaeologists to recognize the ideological nature of their enterprise, they do not intend to "impose skepticism in any absolute sense on our knowledge of the past" (Handsman and Leone 1980). Rather, their goal is to foster critical awareness that the past and museum presentations of it "have more to say to the present than

is generally understood" (Handsman and Leone 1980).[5] And, in fact, their own research clearly supports optimism that a critical stance need not undermine itself epistemologically; they show how it is possible, in practice, for a self-conscious archaeology to yield constructive insights about the past that are at the same time potentially transformative of our understanding of the present.

What is most striking about the case studies I described earlier is that in them Handsman and Leone build a case for rejecting or revising ideologically distorted views of the past by exploiting the very empirical-analytic methods and standards of epistemic adequacy—the very methodological routines—that unreflective researchers normally use to establish the credibility and accuracy of their reconstructions. Handsman, for example, describes the process of identifying and correcting errors in established models of historic settlement patterns as "asking new questions" and "reworking old data in new patterns" in light of these questions. His questions were motivated by suspicion that conventional interpretation had obscured difference and contingency in the past, constructing a seamless continuity between past and present in which nucleated settlement was projected back into the premodern period. To answer these questions he adopted what was, in effect, a process of systematically testing the assumptions that underlie these interpretations: assumptions about commonalities between past and present not only in the form of settlement but in the structuring principles responsible for settlement. Through detailed study of archival sources he found compelling evidence that, in fact, early settlement had not been nucleated as generally assumed and was shaped by quite different social relations and community dynamics than are familiar from the present and recent past (Handsman 1981: 2).

In a similar vein, Leone (1981b) criticizes the Shakertown museum presentations from the vantage point of having first noted a curious gap in the account they offer of Shaker life. While Shaker products and innovations dominate the exhibits—as tangible evidence of Shaker efficiency and productivity—Leone found no systematic reconstruction of the industrial system that was responsible for them. It was in the course of filling this gap, reconstructing the Shakers' unique blend of agrarian and industrial production, that it became apparent to Leone that Shaker society was not, in fact, organized by structuring principles and technological interests like our own. This critical argument turns on a skillful use of archival and material evidence to expose factual inadequacies in conventional reconstructions of Shaker life. This critique in turn calls into question implicit interpretive assumptions about fundamental similarities between Shaker and more mainstream forms of industrialism. In both cases, Handsman and Leone use standard tools of archaeological and historical inquiry to identify systematic distortions in the way the past is appropriated that reflect contradictions inherent in contemporary society.

The epistemological principle that Handsman and Leone presuppose—the condition that makes their practice possible—is that our subjectivity is itself partial and contradictory; all components of our understanding may be interest-constituted, but not so pervasively and coherently as to preclude the possibility of exposing localized error that reflects these interests. Their critical analyses make it clear that it is often possible to use conventional analytic-empirical and hermeneutic-historic methods to identify distortions in particular knowledge claims and to trace these distortions to the underlying assumptions and interpretive principles that are responsible for them. Handsman and Leone thus resist the most uncompromising relativist conclusions sometimes attributed to critical theory: they do not concede that the methodological routines of archaeology are so deeply structured by collective interests that they inevitably reproduce and obscure these interests. They treat interests as constitutive of knowledge in the limited sense that they selectively exempt from critical examination those especially value-charged assumptions about social and cultural reality that define and legitimate contemporary social life; the details of local historical and archaeological reconstructions can often subvert expectations that embody these orienting assumptions. If a critical approach is to be implemented, Handsman and Leone suggest (by example if not explicit directive) that archaeologists should apply their existing tools of inquiry more systematically and widely; they should subject to rational, empirical evaluation not just the details of reconstruction but also the underlying (interest-specific) assumptions that inform these reconstructions.

As a qualification of the most radical interpretations of Habermas's thesis about knowledge-constitutive interests, the epistemic stance adopted by Handsman and Leone is similar to a reformulation of critical theory proposed by Keat in *The Politics of Social Theory* (1981). Keat argues that given a human capacity for reflective self-criticism, methods developed in the empirical-analytic and historical-hermeneutic sciences can be used for emancipatory purposes; informed by an interest in critical self-understanding, they can expose and correct the errors that arise from other (uncritical) interests. On Keat's account, the recognition that interests may distort knowledge claims serves to direct attention to new sources of error, not to derail critical inquiry; by no means are knowledge claims so tightly tied to interests that they are impervious to rational, empirical criticism. It follows, then, that a commitment to an emancipatory interest in research does not reduce theory choice and theory evaluation to pragmatic considerations, nor does it require the development of a new and unique methodology (e.g., one derived from psychoanalysis, as recommended by Habermas).

Transposed to an archaeological context, this principle suggests that the substantive core of the revolution instituted by the New Archaeology should be extended to incorporate an explicitly self-critical dimension. The testing practices advocated by the New Archaeologists should be used to systematically evaluate the underlying assumptions that structure unself-conscious appropriations of the past at all levels of abstraction, from the interpretation of archaeological data as evidence to the formulation of explanatory models that can be tested against that evidence. It is important to note, however, that these practices cannot be expected to conform to the requirements of a positivist hypothetico-deductive (H-D) model of confirmation. They are typically structured not by a deductive logic of subsumption and instantiation but by a closely controlled inductive logic of analogy, by which information about better-known source contexts is used, selectively, as a basis for building models of the inaccessible features of past contexts (see chapter 9). As Leone observes, "we know artifacts never speak for themselves: we have to give them meaning" (1981a: 12), and this process of giving artifacts meaning depends on what we think we understand about familiar forms of production, social organization, and kinship or economic relations. Critical self-consciousness about the interested nature of archaeological inquiry focuses attention on the assumptions that inform this transposition of familiar to unfamiliar; its central goal is to "raise hidden assumptions to the surface" (Leone 1981a: 14). In particular, given the analogical structure of archaeological inference, this critical stance focuses attention on the assumptions of relevance that justify the transposition of information from particular contemporary sources to particular archaeological subjects: assumptions about underlying determining structures that (may) ensure similarities in the association of attributes beyond those that can be compared, source to subject. Leone and Handsman make it clear that these submerged premises, and not just the interpretive conclusions they support, should be the object of rational, empirical investigation. Only when archaeology is practiced with this degree of self-consciousness can it become a basis for criticism in the second sense: critical commentary on the social, ideological forms that have informed the reconstruction of "a past thought essential for our self-definition" (Leone 1981a: 12).

Archaeological Cables and Tacking

Beyond Objectivism and Relativism

CARTESIAN ANXIETIES

Archaeologists wrestle with what Dray describes as "a certain metaphysical anxiety . . . about the task of coming to know what literally does not exist" (1980: 29); as often as they champion methodological strategies for meeting this anxiety, they express deep pessimism, even wholesale skepticism, about the prospects for ever establishing credible knowledge of the cultural past. There are a number of striking parallels between this localized pattern of debate and an opposition, described by Bernstein, between objectivist and relativist positions that recurs across philosophical and empirical fields of inquiry. Despite clear indications that "absolutism . . . is no longer a live option," he finds objectivists unmoved in their conviction that there must be "objective foundations for philosophy, knowledge, or language" (Bernstein 1983: 12). If certainty and "absolute constraints" cannot be secured, they argue, we face the threat of "madness and chaos where nothing is fixed" (18); and because this is an intolerable conclusion—it undermines the authority of all knowledge claims—the premises that lead to it must be mistaken. The threat of unmitigated Cartesian anxiety stands as a reductio ad absurdum of relativist critiques. Relativists are equally unmoved in their conviction that the "quest for some

fixed point, some stable rock upon which we can secure our lives" (18) is manifestly bankrupt: unrealizable and in important respects undesirable. The result is an impasse in which the counterposed positions harden into rigid opposition.

Bernstein holds that there are options "beyond objectivism and relativism" that have been obscured by contemporary debate, but his argument for this thesis turns on a terminological ambiguity. He first characterizes relativism as any position that challenges the claims of the objectivist. The "essential claim" of the relativist is that "there can be no higher appeal than to a given conceptual scheme, language game, set of social practices, or historical epoch"; for the relativist there is "no substantive overarching framework or single metalanguage by which we can rationally adjudicate or univocally evaluate competing claims of alternative paradigms" (1983: 11, 8).[1] This definition of relativism suggests a continuum of positions, ranging from moderate critiques that leave open the possibility of reformulating objectivist ideals to the kind of uncompromising antiobjectivism according to which subjectivism and epistemic chaos is inescapable. In other contexts, however, Bernstein identifies relativism exclusively with positions at the radical end of this continuum. It consists of just those critiques of objectivism that entail the threat of cognitive anarchism to which

objectivists react: the threat evoked by Descartes's "allusions to madness, darkness, the dread of waking from a self-deceptive dream world" (17) or by Feyerabend's antimethodist dictum that "anything goes." Bernstein's thesis is that the abandonment of objectivism, which he considers unavoidable, does not necessarily force one to embrace an uncongenial relativism of this second sort; there remains considerable scope for understanding and critically, rationally assessing knowledge claims even if they cannot be legitimated by appeal to any single "ultimate grid." The options he recommends are, then, ones that escape the opposition between untenably absolutist forms of objectivism and relativism in the second, narrow sense.

In arguing this thesis, Bernstein has as his central objective the goal of challenging the assumption that objectivism and relativism (in the second sense) are exclusive, exhaustive epistemic alternatives. He rejects the skeptical presupposition that "unless we achieve finality we have not achieved anything" (1983: 69), and he objects that those who brand Kuhn, Feyerabend, and Winch as relativists misunderstand their central arguments; "the fierceness of the attacks on Kuhn is indicative of the grip of the Cartesian Either/Or" (60), according to which any critique of objectivist ideals (relativism in the first sense) is construed as a denial of the rationality of the enterprise as a whole (relativism in the second sense). In the process, antirelativists miss important constructive insights about the hermeneutic dimensions of scientific reasoning that allow researchers to proceed, often very effectively, without the benefit of any unitary, clear-cut "grid" of commensurating standards.

I find Bernstein's project compelling but I also find it necessary to disambiguate the pivotal concept of "relativism." On his initial, broad definition of relativism, Kuhn, Feyerabend, Winch, and Bernstein himself are all relativists; relativism just *is* the rejection of objectivism. In that case, what Bernstein argues for is not an escape from relativism and objectivism but the recovery of viable options that lie along a continuum of positions, all of which are relativist in this generic (first) sense; what he defends are various forms of mitigated relativism.[2] This point has more than terminological significance. By constructing the problem in this way—where the mistake to be rectified is the assumption that any critique of objec-

tivism necessarily entails the extreme forms of epistemic pessimism and anarchism associated with relativism in the narrow, pejorative sense—Bernstein commits himself to exploring more palatable, less corrosive relativisms rather than more plausible, realistic forms of objectivism. Important insights are lost in the process, accounting, I will argue, for his inability to move very far beyond the assertion that options exist "beyond objectivism and relativism."

ARCHAEOLOGY AND THE "OPTIONS BEYOND"

Bernstein argues that the epistemic options he defends are to be glimpsed in the sorts of inquiry described by such critics of objectivism in the social and natural sciences as Feyerabend, Kuhn, and Winch. They consider episodes in the history of science and research traditions in the social sciences in which researchers have had to find ways of comprehending forms of life, or evaluating incommensurable theories, when they lack any common (stable, ahistorical, transcontextual) standards to which they can appeal.[3] Although it is a long step from the generalities of Bernstein's discussion to the particularities of practice in a field like archaeology, I suggest that the way archaeologists handle reconstructive inference is an illuminating example of the methodological options to which Bernstein directs our attention.[4] I offer an account of Bernstein's core insights about these options and then elaborate the details with reference to archaeological practice.

BERNSTEIN'S MODEL

Bernstein's account of the alternatives to objectivism and relativism depends on a metaphor inspired by Peirce's suggestion that scientific arguments are more like cables than chains. When researchers grapple with incommensurable theories, Bernstein argues, they do not (indeed, they cannot) proceed by "a linear movement from premises to conclusions or from individual 'facts' to generalizations"; they must exploit "multiple strands and diverse types of evidence, data, hunches, and arguments to support a scientific hypotheses or theory" (1983: 69). As the cable metaphor suggests, even when there is no single commensurating ground for judgment—no one

line of argument that is sufficient on its own to secure an explanatory or interpretive conclusion—"the cumulative weight of [disparate, multidimensional considerations of] evidence, data, reasons, and arguments can be rationally decisive" (74). The relativist conclusion that "anything goes" does not follow from the fact that no one set of considerations is fundamental across the board, no one strand of argument conclusive.[5]

When Bernstein considers Kuhn's and Winch's accounts of cable-style argumentation, further dimensions of complexity emerge that raise anew the question of how (or even whether) arguments in pluralistic contexts can ever yield "rationally compelling" conclusions (1983: 20–30). Kuhn's analysis of revolutionary theory change in the natural sciences makes it clear that the assessment of competing theories depends on considerations that are not just diverse but also internally complex and unstable: the strands that make up a cable of comparative, evaluative argument may conflict with one another; even when researchers share criteria of adequacy they may apply them differently, yielding incompatible judgments about the relative strength of alternative theories; and the criteria are themselves open to revision as research traditions evolve (Bernstein 1983: 55). The work of weighing of factual, conceptual, logical, and pragmatic considerations typical of "the frontiers of inquiry" is therefore inevitably dynamic and interactive. It is a process in which the grounds for epistemic judgment are themselves essentially contested, so that they not only balance but also reshape one another.[6]

These complexities multiply when Bernstein considers the incommensurability with which social scientists grapple. In his analysis of social inquiry (1990), Winch focuses attention on problems of interpretation that arise *between* forms of life, generalizing what came to be known as Kuhnian insights (Bernstein 1983: 97). In these cases interlocutors struggle not just with differences between particular scientific (or economic, or political) worldviews but with a much deeper disjunction that calls into question the very possession of such views. To understand the practices that mediate these differences, Bernstein appeals to an account Geertz gives (1979 [1976]) of how anthropologists actually do (or can) avoid the pitfalls of taking either their own framework or that of their subjects as foundational. Geertz suggests that al-

though anthropologists must grasp the system of "experience-near" concepts in terms of which members of a culture ordinarily understand and represent their own actions and beliefs, ethnographic understanding requires that they also deploy interpretive, explanatory "experience-distant" concepts that may diverge sharply from internal understandings (1979 [1976]: 227–228). As Bernstein puts this point, "experience-near concepts must be balanced by the appropriate experience-distant concepts, concepts that are not necessarily familiar to the people being studied but that . . . make intelligible the symbolic forms [of their culture]" (1983: 95). The aim of ethnographic inquiry is thus to construct an account of how abstract, distant concepts (like the concept of a person)[7] are actualized in the experience-near concepts and practices of particular subject cultures. And the process by which this aim is accomplished is one of "dialectical tacking back and forth" (Geertz 1979 [1976]: 239).

This Geertzian image of dialectical tacking serves as a succinct second metaphor for the forms of practice described by Bernstein in which local comparisons are used to produce the multiple strands of argument captured by the cable metaphor.[8] In the passages Bernstein cites, Geertz describes tacking primarily as a movement between the distant—theoretical, abstract—concepts that ethnographers draw from their culture of origin and the concrete, experience-embedded concepts that they encounter in the study of cultures that differ from their own. Drawing on an account of interpretive practice developed by Gadamer, Bernstein later describes Geertz's ethnographic tacking as a hermeneutic process that involves a movement between "'parts' and the 'whole'," a "dialectical interplay between our own preunderstandings and the forms of life that we are seeking to understand" (1983: 133, 173). Conceived as a diagonal tack, the dimensions traversed in cross-context understanding are, on the one hand, abstract-to-concrete and, on the other, familiar-to-alien. The process of cross-framework inquiry is necessarily more complex than this diagonal traversal, however. There are at least two additional dimensions on which interpretive tacking occurs; taken together, they illuminate what the cable metaphor leaves out, capturing the dynamic of inference by which diverse strands of evaluative argument are constructed.

First, our own experience-distant concepts do not emerge in a cultural vacuum. To produce them, some form of dialectical tacking must occur on a vertical axis within the reference context of the investigator. The categories and presuppositions that make familiar experience intelligible must be analytically refined—we must grasp the general contours of our own symbolic, cultural life—and this analysis requires reflection on the experience-near concepts and practices, the pre-understandings, that constitute this context. This is just to say that before anthropologists can enter the process described by Geertz, they must engage in something akin to the leap of sociological imagination made famous by C. Wright Mills (1959).

In addition, as Bernstein argues with reference to Winch, it is naive to assume that reflective, experience-distant understanding is the sole province of the observing anthropologist. Anthropological subjects can be expected to have conceptual schemes of their own that order and explain their cultural practice at various levels of abstraction; in Geertzian terms, they have a repertoire of experience-distant concepts, and engage in their own internal process of vertical tacking to explanatory self-understanding. Thus the aim of anthropological tacking cannot simply be to establish how the experience-distant concepts of the ethnographer are instantiated in the experience-near practice of those they study. Ethnographers must also be concerned with grasping the experience-distant self-understanding that informs those practices, considered both as part of the *explanans* and, crucially, as a rival or complementary *explanandum*.

The process of tacking between near and distant concepts is further complicated by the fact that it must proceed inferentially, usually by way of a suppressed analogy. To understand others (near or distant) we typically draw on a repertoire of both practical knowledge and general theories about the human motivations, beliefs, and capabilities that can give rise to the sorts of action we observe, and we then formulate hypotheses, at various levels of abstraction, about the concepts (distant and near) that may constitute the animating worldview of those we seek to understand. If we are to avoid arbitrary imposition, the inferential tacking between our hypotheses and the practices of those we hope to understand must incorporate a critical dimension. It is important to ask

directly (if possible) if experience-distant hypotheses drawn from one context capture the form and meaning of practices rooted in quite different contexts, and otherwise, or in addition, to seek evidence that members of the culture represented engage in other practices or hold aligned beliefs that could only be expected if the experience-distant model in question is more or less right in what it posits about the concepts that inform their action. In short, the ethnographers' model must be responsive to evidence—experience-near or -distant —of its explanatory and empirical adequacy.

Finally, inferential tacking is an interactive process on all dimensions. In Gadamerian terms, when it succeeds, hermeneutic tacking realizes a fusion of horizons in which "our own horizon is enlarged and enriched" (Bernstein 1983: 143). The ethnographers' work is not just a matter of grasping the conceptual schemes internal to the subject community; ethnographers should be prepared to rethink their own experience-distant concepts as they compare them with those instantiated in other contexts, and to reassess their own experience-near beliefs and practices in light of what they learn.[9] For better or worse the process of negotiating cross-context understanding has the potential to extend and realign conceptual resources on both sides, including the criteria of adequacy that determine for each what will count as a better account.

The tacking process is thus at least three-dimensional (vertical, horizontal, and diagonal) and it is bidirectional on all dimensions. The experience-distant concepts that inform cross-context analysis must be refined from the researchers' own experience, a process that requires vertical tacking between practice and its symbolic, explanatory representation in their home contexts. These concepts then serve as an initial guide for grasping the experience-distant concepts that inform unfamiliar practices, a process that requires horizontal and diagonal tacking between our own concepts and those we seek to understand at the level of both reflective (distant) and experiential, practical (near) understanding. The comparative, reconstructive arguments formulated on each of these dimensions are subject to the same evaluative constraints as bear on the adjudication of competing (incommensurable) theories within a research tradition or form of life; and in all cases, the dialectical process of exchange stands to trans-

form each of the conceptual schemes that are brought into play. The tacking metaphor thus suggests that incommensurability between theories or worldviews is mediated (when and if it is) by a concatenation of cables of arguments, each woven in these multiple dimensions.

ARCHAEOLOGICAL TACKING

While this unpacking of the tacking metaphor offers, in broad outline, the structure of an answer to the question of how researchers proceed when they confront incommensurable theories or forms of life, it also raises a number of new, more specific questions about how we assess the credibility of the local inferences that constitute the various strands of the arguments by which, on Bernstein's account, rationally decisive conclusions are (sometimes) reached. A useful case to consider in this connection is the strategy of reconstructive argument developed by archaeologists. Insofar as archaeologists lack direct access to the articulate beliefs of cultural subjects who "literally do not exist," they must make explicit a range of assumptions and inferential steps that are often suppressed when it is possible to negotiate directly with those who participate in the unfamiliar forms of life we wish to understand.

Whatever its specific aims, archaeological interpretation depends on background knowledge of contemporary contexts; usually it proceeds by means of ethnographic and other forms of analogical inference (see chapter 9). It is therefore explicitly and heavily dependent on vertical tacking arguments within the source contexts (broadly construed) on which archaeologists rely to develop both the experience-distant concepts—theories about cultural development, differentiation, interaction, and adaptation—and the experience-near models of cultural practice that they use to interpret the archaeological record as evidence of past forms of life. These source-side arguments bring into play a number of empirical and conceptual constraints that are suggested by the tacking metaphor but are not discussed in any detail by Bernstein or Geertz. Archaeologists also exploit a diagonal tack from the categories of analysis and interpretive principles that they draw from source contexts to the observable, material consequences of the past practices that constitute the subject of inquiry. I will consider these two components of the research process in turn.

In practice, archaeological data often raise a series of initial questions: How, when, by whom, or as a consequence of what type of culture process was this material record produced, and what does it tell us about antecedent forms of cultural life? These questions direct researchers to a particular range of background information about source contexts that then serves as the basis for reconstructive inference: information from ethnohistoric, sociological, and psychological sources, as well as from the natural and life sciences that deal with the ecological and physical conditions of human, cultural life. The weaknesses as much as the strengths of the examples I have discussed in previous chapters illustrate the constraints that (should) bear on the initial tack from familiar sources to interpretive models. In his classic interpretation of the Mesolithic village Star Carr (1954) , J. Grahame Clark drew on Inuit ethnography for an analogical model of prehistoric subsistence practices and social organization, given evidence that similar resources were exploited using comparable tools in both contexts. Curren (1977) likewise argued, on the basis of comprehensive formal similarity, that spatula-shaped stone gorgets should be interpreted as potter's tools, and Hill (1966) ascribed specific functions to prehistoric pueblo rooms in the U.S. Southwest on the basis of their similarities in size and shape to ethnohistoric and contemporary pueblos. Longacre (1966, 1968), a colleague of Hill's, developed the further argument that clusters of these rooms represent social units—extended, matrilocal, and matrilineal family units—building on an additional comparison. He found that distinctive sets of ceramic design elements co-occur in spatially discrete areas within prehistoric pueblos, and he interpreted these in light of the ethnohistoric observation that pueblo women potters often work together in family-defined workshops, sharing and influencing one another's repertoire of designs.

In each of these cases, a combination of vertical and diagonal tacks carries the interpretive inference from a comparison between the material residues of practice in source and subject to a hypothesis about the concepts and conditions that organize prehistoric practice. And in each case this inference depends not just on a catalogue of similarities but also on arguments of relevance that identify in the source, and impute to the subject, "determining structures" (Weitzenfeld 1984)

that account for the function of artifacts and architecture, and that structure social relations of production and reproduction. Clark invokes a weak principle of ecological determinism when he assumes that environmental constraints are the crucial determinants of group size and subsistence regime, while Curren and Hill assume that a similar nexus of material constraint and functional consideration must have determined the shape, size, and distribution of the artifacts and architecture they interpret. Longacre's interpretation depends on background assumptions about how elements of ceramic design diffuse among potters and how a specific social organization of ceramic production might enhance or curtail this diffusion, producing a distinctive distribution of group-specific styles.

To widen the range of analogs on which they can draw, and to substantiate these assumptions of relevance, archaeologists routinely engage in a process of source-side vertical tacking in which they bring the resources of collateral disciplines to bear on the claims they make (or presuppose) about the determining structures that may, or must, produce specific types of material culture. Several of the cases mentioned above illustrate the potential for this tack to decisively eliminate some interpretive options and establish the initial credibility of others. For example, detailed analysis of existing Inuit ethnography by David (1973) made it clear that the subsistence patterns of tundra-living groups is even more variable than Clark recognized, while a rapidly expanding program of ethnoarchaeological research has distinguished a number of quite different foraging strategies that may be adopted in these and other environmental settings. And, most dramatically, closely worked studies of communication among Mexican ceramic artisans undertaken by Margaret F. Hardin (1970) challenge Longacre's presupposition that social proximity, embodied in a workshop association, is reflected in a sharing of design elements. Hardin demonstrated that these smallest constituents of ceramic design are, in fact, the features of ceramic traditions that diffuse most quickly and widely; it is similarities in the design structure into which widely shared elements are incorporated that reflect the close association of potters who work together.

In addition, as the New Archaeologists have insisted, work on the source side of the interpretive equation cannot stand alone; it is properly a guide to investigation of the subject, initiating a diagonal tack in which archaeological evidence is brought to bear on the question of whether, or with what degree of likelihood, a particular past context instantiates one or another of the reconstructive models that archaeologists have devised. As the fate of Curren's interpretation indicates, archaeological testing can be decisive in settling what can reasonably be claimed about a past cultural context, despite the uncertainties associated with any use of archaeological data as evidence. Starna's identification of securely dated gorgets in preceramic contexts (1979) renders untenable Curren's hypothesis that they served as potters' tools (see chapter 9). A more far-reaching example of this sort is Strong's (1935) and Wedel's (1936) disproof of the entrenched assumption that prehistoric Plains Indians were nomadic hunters, like those groups encountered in the Plains at the time of contact. The evidence of cultigens in prehistoric contexts, and of close cultural connections between prehistoric Plains cultures and displaced (contact period) agricultural groups, served to undermine not only conventional archaeological interpretations of Plains prehistory but also the assumptions about determining structures that informed them: specifically, the assumption that Native Americans lacked the technical skills and initiative to have been successful agriculturists in the Plains environment (see chapter 1). Hill's archaeological test of his hypothesis about room function further illustrates how the diagonal and horizontal tacks from interpretive model to archeological evidence can serve not only to expose error but also to constructively redirect interpretive theorizing (this aspect of Hill's study is discussed in more detail in chapter 13). In the process of testing for specific constellations of activity-related artifacts and plant remains, he noted a puzzling preponderance of wild plant remains. This suggested that the subsistence strategies of prehistoric pueblo groups were more diversified and flexible than previously recognized, thereby calling into question the assumption that because pueblo communities were sedentary, they must have been exclusively dependent on agricultural resources.

In some of these cases, testing procedures are decisive because an especially telling line of archaeological evidence was recovered that could unambiguously disprove entrenched interpretive

claims and assumptions; more often questions about the adequacy of an interpretive hypothesis are settled when independently constituted lines of evidence converge either in supporting or refuting its central claims about particular past practices. In all cases, however, interpretive conclusions depend on a number of different lines of argument that are developed on a vertical tack within sources, and on horizontal or diagonal tacks between source and subject. Their strength therefore derives not just from the diversity of the lines of evidence that lend them support but from the use made by the constituent strands of different ranges of background knowledge to interpret different dimensions of the archaeological record; they are compelling, taken together, insofar as it is implausible that they could all incorporate compensatory errors.

These features of archaeological practice suggest a general strategy of response to the metaphysical and epistemic anxieties born of antiobjectivist critiques that extends well beyond archaeology, offering further insight into the nature of the "options beyond" defended by Bernstein. There are certainly no such things as factual givens or context-neutral reasons that can serve as a transcendent grid capable of stabilizing the interpretation of unfamiliar cultural beliefs and practices or of grounding the adjudication of claims made by competing theories. Even so, the concepts we start with, near or distant, do not determine what we will find when we make the tack from source to subject. The orienting concepts we draw from familiar sources may be significantly reshaped, empirically and conceptually, by a series of vertical tacks, and their applications to new (subject) contexts are often sharply constrained by evidence brought

to bear by diagonal and horizontal tacks between source and subject. None of these tacks is rationally decisive on its own, but together they can call into question quite deeply held convictions about what is (or what must be) the case in unfamiliar contexts, even in archaeological contexts in which the subjects of inquiry are at best indirectly accessible. The cases I have considered make it clear that despite the vagaries of interpretation in contexts in which evidence is itself a contentious and unstable interpretive subject, it is not true that "anything goes."

Objectivists of a narrowly empiricist stripe presume that these empirical constraints reveal a unitary ground and source of legitimate (context-independent, objective) knowledge. Clearly things are not this simple; but the common error of relativist responses that cluster at the radical end of the continuum is either to ignore the role of empirical constraints altogether or to assimilate them to a seamless and self-contained network of belief, making those constraints an (arbitrary) artifact of the concepts that inform their interpretation as evidence. The insights of a mitigated objectivism have a great deal to offer in rebalancing the debate over objectivism and relativism, explaining how it is that rationally decisive judgments (if always defeasible) can be realized even in the absence of stable epistemic foundations. My thesis is that Bernstein's "options beyond"—the options he finds immanent in convergent lines of moderate relativist critique—depend on two loci of empirical constraint that are especially clear in archaeological tacking: constraints on the formulation of interpretive models that operate within source contexts, and constraints deployed in the process of testing the applicability of a model to subject contexts that may be incommensurably different from the sources on which it was based.

On Being "Empirical" but
Not "Narrowly Empiricist"

Post- and antiprocessual critiques of the 1980s and 1990s throw into sharp relief the tensions inherent in the New Archaeology that I have described in earlier chapters. In the essays included in this section I address the challenges posed by these critiques, elaborating on proposals about the status of archaeological evidence and strategies for stabilizing interpretive inference that I introduced in part III. My central thesis is that the very features of the archaeological record that are often cause for epistemic despair are among its greatest assets as a resource for investigating the cultural past. I refer here to the fragmentary nature of archaeological data and to the necessity, in making any use of it as evidence, of relying on background knowledge and auxiliary hypotheses, of ladening data with theory.

Taken together, the essays in this section present a model of how archaeologists make nuanced judgments about the relative credibility of diverse lines of evidence so that despite being richly constructed, empirical considerations can constrain reconstructive and explanatory claims about the cultural past. The inferential process that underlies these judgments has two components: it is a matter of systematically assessing the strength of the auxiliary assumptions (bridging principles, interpretive theory) used to interpret archaeological data and to formulate explanatory or test hypotheses; and it exploits the epistemic independence that (may) hold on two dimensions, between diverse lines of evidence and within any given line of evidence. In chapter 12 ("Red Herrings"), I sketch this model and argue that in practice, both processual and postprocessual archaeologists put it to good use when they move beyond critique and substantiate their claims about the potential limits of archaeological understanding. In the subsequent three chapters ("Bootstrapping," "Archaeological Evidence," and "Rethinking Unity"), I sharpen the conception of epistemic independence that underpins this response to the debate generated by the positivism of processual archaeology and

apply it to two quite different cases: the challenges to ideals of objectivity sometimes attributed to feminist archaeologists and the claims that historical archaeologists have made on behalf of hybrid (jointly archival and archaeological) forms of inquiry. Overextended appeals to the unifying power of explanatory hypotheses force consideration of what distinguishes compelling from spurious convergence; this question is the focus of the analysis I offer in the section's final essay (chapter 16), "Unification and Convergence in Archaeological Explanation," where I return to questions about the explanatory goals of archaeology that were central to chapter 4.

"Heavily Decomposing Red Herrings"

Middle Ground in the Anti-/Postprocessualism Wars

OPPOSITIONAL DEBATE

These are difficult times for philosophy in archaeology. The tenor of debate among those who take seriously questions about the aims and limitations of archaeological inquiry has become so acrimonious and so sharply polarized there is often very little constructive engagement of the issues raised.[1] Adversaries joust with such gross caricatures of opposing views that they routinely argue past one another, and then reinscribe in their own platforms the very contradictions they mean to transcend.[2] Not surprisingly, a recurrent theme in these engagements is the accusation that the critics and defenders of key positions have simply failed to see what is *really* important; their arguments turn on irrelevancies, and red herrings abound.[3]

Two such charges are especially intriguing, because they work so completely at cross purposes. Lewis Binford inveighs against the "big red herring" of overextended claims about the theory dependence of observations that he finds implicit in the relativist, anti-science positions he attributes to Hodder and, indeed, to all "Yippie" (postprocessual) archaeologists (1989: 35). In direct opposition to this pro-science (processualist) position, Shanks and Tilley call on Renfrew to "dispose of his heavily decomposing scientific red her-

ring": "stop wafting . . . in front of our noses [the] myth, mirage, obfuscation" of appeals to a reified and simplistic conception of scientific method (1989: 47). In what follows, I examine the concerns that lead some to take these red herrings seriously and others to dismiss them as inflammatory irrelevancies. I argue that the highly charged rhetoric typical of the debate between processualists and post- or antiprocessualists obscures considerable common ground between these positions.[4]

SCIENTIFIC METHOD VERSUS THEORY-LADENNESS

Where the red herring of scientific method is concerned, I have considerable sympathy for Shanks and Tilley's insistence (1989: 43) that the abstract scientific ideals invoked by Binford and by Renfrew should be problematized. The historical and sociological analyses of scientific practice inspired by the demise of positivism undermine the presumption that there is a unity of scientific method —a coherent body of "techniques, now well established . . . for the investigation of the natural world," as Renfrew puts it (1989b: 38)—that characterize all the disciplines we identify as scientific, and differentiate them clearly from nonscientific practice.[5] Indeed, Renfrew's parentheti-

cal acknowledgment that the techniques of science are "always evolving," as well as Binford's frequent description of science as a process of "learning how to learn" (1989: 230, 250, 487), suggests that the hallmark of the traditions of inquiry they recognize as scientific is precisely their flexibility, their adaptive responsiveness to diverse and changing conditions of practice.[6] This is not necessarily to endorse Feyerabend's argument that the only principle of practice that holds across the board is that any rule can be transgressed: "anything goes" (1988: 19). But it does suggest that Shanks and Tilley are right to object that appeals to science or "the scientific method" are, at best, unhelpful in determining how to proceed when a return to the innocence of empiricist ideals is no longer tenable.

At the same time, the position of anti-/postprocessualists regarding the red herring of paradigm dependence is exceedingly paradoxical. Shanks and Tilley have made good use of contextualist (specifically, Kuhnian) arguments to establish that even the identification of archaeological data, and certainly the construal of data as evidence, is inevitably mediated by interpretive theory. In a typical passage, Shanks and Tilley argue that "what makes the archaeological data speak to us, when we interpret it, when it makes sense, is the act of placing it in a specific context or set of contexts" (1987: 104). They go on to argue that there therefore is no foundational realm of fact that can serve as a final, autonomous basis for judging the truth or credibility of theory (111). In this spirit, Hodder once insisted that archaeologists simply "create facts" (1983a: 6; see also 1984a), while Shanks and Tilley conclude that there is "literally *nothing independent of theory* or propositions to test against" (1987: 111; emphasis in the original).

And yet, even as anti-/postprocessualists endorse a "radical pluralism" according to which "any interpretation of the past is multiple and constantly open to change, to re-evaluation" (Shanks and Tilley 1987: 109), they have distanced themselves from those forms of relativism that enforce an "anything goes" tolerance of all imaginable constructs. In response to critics who impugn them as relativists, Shanks and Tilley declare that they "don't accept any view of the past" (1989: 50). In fact, this is a recurrent theme in their work; in *Re-constructing Archaeology* (1987) they were clear on the point that if archaeology is to fulfill its po-

tential as a basis for critique of and active intervention in the present, the threat of an "anything goes" relativism must be resisted: "we cannot afford the essential irrationality of subjectivism or relativism as this would be cutting the very ground away from under our feet" (1987: 110). Given the tension that this declaration sets up with their constructivist arguments about evidence, it would seem incumbent on Shanks and Tilley to give some further account of how, exactly, archaeologists are to judge the relative credibility of evidential as well as of interpretive and explanatory claims; they need to explain how archaeologists are to be empirical rather than empiricist.[7] And yet, as their critics and even some of their fellow travelers point out, it is unavoidable that they, and anti-/postprocessualists generally, have failed to give any very satisfying account of how archaeologists can (or do) warrant discriminating judgments about the plausibility of competing claims about the past.

This is an issue Hodder has been concerned to address in the arguments he makes for "interpretive archaeology" (1991), an approach he has advocated since the mid-1980s when he began to move away from his strongest early arguments against processualism.[8] He objects that in their initial response to the failings of processual archaeology, anti-/postprocessual archaeologists remained too exclusively preoccupied with theoretical questions. They were primarily concerned with theorizing the internal, meaningful aspects of the cultural subject that processualists had left out of account, but they failed to come to terms with methodological questions about the nature and practice of the interpretive process required to bring these theoretical insights to bear on archaeological subjects. While this call for more sustained and constructive analysis of actual practice is welcome, the critical assessment on which it is based underestimates the centrality of epistemological concerns in anti-/postprocessual discussions since the early 1980s (see, for example, contributions to Hodder 1982b). And it has to be said that in setting the agenda for interpretive archaeology, Hodder himself offers few concrete suggestions as to how such issues might be addressed, beyond invoking philosophical hermeneutics as a promising source of insights about interpretive practice and endorsing a "guarded commitment to objectivity" (1991: 10).[9]

It might seem illegitimate to insist that Shanks and Tilley have an obligation to fill out the details of a methodological alternative to processualism, given their principled stand against all attempts to define, in abstract terms, "mechanistic procedures of so-called scientific or objective analysis" (1989: 2), and against any assumption that "predefined methods" (45) can ensure that archaeologists will not be "led to construct a false past" (L. Binford 1989: 39). However, accounts of method need not be prescriptive or arbitrary in the ways Shanks and Tilley find objectionable. There is a perfectly good sense in which the reflexive dimension of practice that they endorse can, and should, include articulating and critically appraising the provisional principles of method that are emerging in practice. Indeed, they seem to take this point when they insist on the need to identify "the most fruitful strategies" for "reading and writing" the past into the present (1989: 44).

The challenge that Shanks and Tilley face is to reconcile their claim that some accounts are better than others with their insistence that "the entire world is always already a vast field of interpretive networks" (1989: 2) and that objects of inquiry are always highly theorized. They set the terms of the problem themselves: if archaeology is to fulfill its critical mandate, they must explain how some types of theoretical construct can constrain the construction of others such that some are properly regarded as evidence (for some purposes, and at some moments in the process of inquiry)—"a network of resistances to theoretical appropriation" (44)—while others serve as tools of appropriation, as background assumptions and principles that mediate interpretation. This distinction will not ascribe permanent epistemic status to specific components of discourse; those claims that function as resistances, that are treated as constraints at one juncture, are always open to reassessment as interpretive or explanatory or generalizing constructs at another. So part of the task at hand is to explain how and why, under what conditions or with what warrant, the epistemic status of various kinds of constructs can change. This requires a nuanced account of how archaeological data—facts of the record—are constituted as evidence, how they come to be laden with theory such that they can have a critical bearing on claims about the cultural past and can, in turn, sustain what Shanks and Tilley call a "par-

ticular and contingent objectivity" (43). Whether or not this is properly termed a theory of archaeological testing rather than one component of a hermeneutic circle—whether or not it constitutes a mode of intellectual production that should be considered scientific—seems to me a genuine irrelevancy.

It is here that I see the convergence between the interests of processualists and anti-/postprocessualists. While polemical appeals to science as a model of practice are surely a red herring, they by no means exhaust the response of processualists to their critics. In particular, Binford's preoccupation with the "question of accountability" (1989: 34)—the question of how archaeological inferences are or can be justified (cf. 3, 10, and throughout)—is explicitly motivated by a concern to show that it is possible to sustain what *he* calls "relative objectivity" (230; with reference to 1982b), in face of the threat of cognitive anarchy that he finds implicit in the "open relativism" of anti-/postprocessualism (e.g., 1989: 34). Although Binford is vehement in denying that general questions about theory-ladenness have any relevance to practice (1989: 34), the middle-range practices of building and exploiting source-side resources that he advocates are, quite straightforwardly, strategies for securing, or rendering systematic, the inferences by which archaeological data are laden with theory.[10] Binford thereby provides many of the resources necessary for dealing effectively with the pressing problems of method—the problem of determining "the most fruitful strategy of inquiry" (Shanks and Tilley 1989: 44)—that confront anti-/postprocessualists such as Shanks and Tilley.

In making this argument I reject Binford's own disclaimers to the effect that "seeking middle-range research opportunities does not address itself to the bogeyman of paradigm dependence" (1989: 38). On the contrary, substantive work on the theory that ladens archaeological evidence is fundamental to any responsible treatment of problems of vicious circularity that concern processualists and their critics alike. I would also qualify Shanks and Tilley's claim that "vital philosophical and social questions of the theory dependence of data . . . are glossed over in the archaeological literature in general" (1989: 43). Because they reject processual analyses out of hand as dependent on a naive positivist conception of science, they fail to

see the relevance to their own work of the analyses of method developed by processualists like Binford. Finally, given the common desire to come to grips with the problem of how archaeological data can be both theory-laden and a source of resistance to theoretical appropriation, Binford is just wrong to claim that no one but he takes seriously the fundamental epistemological problem of establishing "how . . . we have confidence in or render secure the inferences and descriptions of the past offered by virtue of our study of artifacts," or the related methodological problem of how we go about "developing reliable means for inference justification" (1989: 10, 3). He underestimates the persistence of these concerns historically (see Grayson 1986; see also chapters 1–3 above) and he fails to recognize the constructive elements of anti-/postprocessual attempts to grapple with these problems.

COMMON GROUND

In the spirit of exploring this common ground, I here identify three points on which there is grudgingly consensus, and then sketch an account of how archaeological observations are constituted as evidence such that despite being richly theorized, they do routinely turn out differently than expected and can play (at least provisionally) a constraining role in the formulation and evaluation of knowledge claims about the cultural past.

First, all parties to the debate accept the anti-foundationalist point that neither data nor evidence are given, stable, or autonomous of theory. This is a central contention of anti-/postprocessual writers, and on the processualist side of the divide this contextualist thesis is explicitly endorsed by Binford and Sabloff in their discussion of paradigm dependence (1982), and by Renfrew when he observes that "post-positivist philosophers of science . . . agree that the material record can only be studied and data elicited by working within some kind of theoretical framework: the data can never be entirely free of the theoretical framework which produces them" (1989b: 39).[11]

Second, by extension, all recognize that the identification of archaeological data, as well as their constitution as evidence, depends on linking principles: source-side or background knowledge, middle-range theory, or mediating interpretive principles.

Finally, all agree that although archaeological data and evidence are interpretive constructs, the process of interpretation need not be viciously circular; the dependence on linking principles by no means guarantees that the resulting evidence will conform to expectations. Archaeologists can and routinely do make empirically grounded and conceptually reasoned judgments about the relative credibility of claims about the evidential significance of archaeological data; these are by no means certain, but neither are they entirely arbitrary. The problem is to give a systematic account of how researchers make such judgments.

Postpositivist philosophers and historians of science have been concerned with just this problem, resisting the excesses of social constructivist accounts of science as much as the logicism and foundationalism of positivist theories of science to which they were a response. Sociologically reductive accounts often preserve the categories of positivist analysis they mean to subvert, simply inverting its priorities—that is, privileging theory or contextual interests over observation and evidence (see, e.g., Galison 1987: 7–9). The result is a range of positions that offer, at best, "partial insights into the character of observation" (Galison 1987: 12) and are unable to make sense of the difficulty of doing science or of its successes; they run aground when faced with cases in which scientific practice shows little of the instability and arbitrariness of construction on which some of the more doctrinaire Strong Programme sociologists of science (among others) have insisted.[12]

In response to this impasse, an increasing number of historians and philosophers of science have reassessed what it means to say that observations are theory-laden. Such efforts are evident in philosophical work on experimental practice (e.g., as described by Galison 1988; Hacking 1988b, 1989) and in Shapere's analysis of the role played by prior information in determining what will count as an observation in physics (1982: 505). Shapere insists that although nothing can provide observation an "absolute guarantee" of efficacy (1985: 22, 36), it is simply not the case that observational beliefs are all (equally) doubtful or unstable. The explanation he gives for why that claim holds true is elaborated in important ways by Kosso (1988, 1992) and by Hacking (1983). Comparing these analyses with those emerging in archaeology at the intersection between contested

positions, I note some persistent similarities in the factors found to be crucial in stabilizing and warranting evidential claims. Specifically, I find those that fall into two categories—security and independence—especially relevant for understanding how evidence-constituting inferences are established in archaeological contexts.

In both archaeology and the experimental sciences analyzed by Hacking and Shapere, the key to stabilizing evidential claims is very often taken to be the security of the sources on which is based the imputed linkage between a surviving archaeological record and the antecedent contexts, conditions, events, or behaviors presumed responsible for it. But security is a complicated matter. On the one hand, what counts is security in the sense of "freedom from doubt" (Shapere 1985: 29), or entrenchment, in the source fields from which linking principles are drawn, a judgment that concerns both the credibility of the source field and the degree to which the appropriated theory is uncontested within the contexts in which this theory was originally developed and applied. But on the other hand, an important consideration in archaeological contexts is the *nature* of the imputed link: whether, or to what degree, the background knowledge in question establishes an exclusive and determinate connection between archaeological remains and the antecedent conditions or processes thought to have produced them.[13] The ideal of security in this sense is realized when the available background or source knowledge supports a biconditional linking principle to the effect that a surviving archaeological trace could have been produced by only one kind of antecedent condition, event, or behavior.

Biconditional security is, of course, the cornerstone of the deductivism once endorsed by Binford and still implicit, despite his subsequent disclaimers (1989: 17, 242, 261), in his tendency to privilege middle-range theory that promises unconditional, uniformitarian linking principles. Ironically, this ideal also figures in Hodder's appeals to universal principles of meaning constitution, as when he finds in Collingwood an implicit commitment to the view that "a universal grammar exists"—a set of "universal principles of meaning . . . followed by all of us as social actors"—ensuring that "each unique event has a significance which can be comprehended by all people at all times" (Hodder 1986: 124). The sug-

gestion that such reliable structural or cognitive principles might underwrite inferences from material remains to the intentional dimension of past human lives was a key component of Hodder's argument that archaeology can and should address questions about the insides of human action and cultural contexts. More closely controlled and qualified assessments of security in this sense—assessments that avoid appeals to abstract and problematic universals—figure in Shanks and Tilley's analyses of Swedish tombs and grave goods (Shanks and Tilley 1982, 1987; Tilley 1984). They are evident, for example, in the analysis of "structural homologies" operating across various categories of material associated with these tombs (Tilley 1984: 136), and in the arguments Shanks and Tilley give for attributing such homologies to structuring principles that underpin social relations and systems of control operating in the prehistoric communities that produced these tombs (1982: 150).

There is, finally, a third sense of security relevant to archaeological assessments of evidential claims: it has to do with the number and complexity of the linkages required to connect a body of archaeological material to those dimensions of the cultural past that are of particular interpretive or explanatory interest. Security of this sort is assessed in terms of something like the considerations of directness, immediacy, and amount of interpretation or degree of nesting of inferences described by Kosso when he amplifies Shapere's analysis of observation in physics (Kosso 1988: 455; Shapere 1982, 1985). In archaeological cases there can be no question of literally "interacting in an informationally correlated way" (Kosso 1988: 455) with the cultural past, as is relevant in discussions of experimental practice in physics and biology; direct measures of immediacy are inapplicable. Nevertheless, the length and complexity of the causal chain by which archaeological remains are produced—the number of interactions and of different kinds of factors involved—are clearly relevant analogs of the directness and degree of nesting (i.e., the amount of interpretation) that Kosso finds crucial to the credibility and objectivity of physically mediated observation. When interpretation depends on linking principles that postulate probable, or incompletely determining, antecedent causes—as is typical of cultural subjects—the possibility of error in a judgment of

evidential import increases exponentially as the number of such links expands. A concern to establish security in complex chains of inference seems to be the motivation for Schiffer's emphasis on the importance of delineating and closely documenting the range of interacting transform processes—cultural, natural, depositional—that work together to produce what survives as an archaeological record (e.g., 1983).

There are, then, (at least) three sorts of security at issue in archaeological assessments of evidential claims: security as a function of the entrenchment or *freedom from doubt* of the background knowledge about the linkages between archaeological data and the antecedents that produced them; security that derives from the *nature* of the linkages involved—specifically, the degree to which they are deterministic; and security that arises because of the *overall length and complexity* of the linkages involved.

In addition to stressing security, Binford has famously insisted on the importance of independence. Appeals to independence take at least two forms both in the discussions Binford published after 1982 and in the interpretations that anti-/postprocessualists have used to illustrate the fruitfulness of their alternative approaches to the archaeological record. I identified the first and perhaps the most straightforward sense of independence in chapter 7: it is an independence between the linking principles used to constitute archaeological data as evidence and the explanatory and interpretive models of the past on which this evidence is meant to bear. When Binford urges archaeologists to make use of background knowledge about "processes that are *in no sense dependent* for their characteristics or patterns of interaction upon interactions [that constitute the subject of the reconstructive hypothesis under evaluation]" (1982b: 135; emphasis in the original), he appeals to just the sort of independence that Hacking (1983: 183–185) and Kosso (1988: 456) find crucial in determining whether an observation can stand as evidence for or against a given test hypothesis in experimental contexts in biology and physics. It is an independence between the constituents and the conclusions of an inference that runs along what amounts to a vertical axis from elements of a given data base, via claims about how these data may or must have been produced, to conclusions about their significance as

evidence of some aspect of the cultural past. It is this sort of independence, as exploited in microbiology, physics, and astronomy, that leads Hacking to declare that although observations are clearly "loaded with theory," the theory involved often has no (viciously circular) connection with the subject under investigation or with current understanding of the relevant subject domains (1983: 185).

A second sort of independence is realized on a horizontal dimension when a number of *different* linking principles are used to constitute data as evidence of the cultural past. In some archaeological cases, this is analogous to the independence Hacking finds exploited by the makers and users of microscopes, where completely different physical processes—different interaction chains, and different bodies of ladening theory—are used to detect the same microscopic bodies or structural features of these bodies. Such independence serves to underwrite a localized miracle argument to the effect that it would be highly implausible that independent means of detection should converge if the body or structure under observation did not exist (Hacking 1983: 202; see chapter 5 above). As Kosso puts this point, an inductively confirming inference is credible when (or if) "the chances of these independent theories all independently manufacturing the same fictitious result is small enough to be rationally discounted" (1989: 247).[14]

Triangulation on a *single* aspect of an archaeological subject is often critically important in archaeology; Binford appeals to it when he argues the value of using multiple dating techniques or varying the descriptive categories in whose terms the analyses of patterning inherent in a given body of data are carried out (1989: 242). But in addition, horizontal independence may arise between lines of inference when diverse resources are used to constitute evidence of *distinct* aspects of a past context, cultural system, or series of events. On the assumption that these lines of evidence bear on a set of interacting events (or agents, or institutions, or conditions), the requirement that they yield a coherent model of the past context sets up a system of mutual constraints *among* vertically constituted lines of evidence. Independence in this extended sense seems to be what Binford has in mind when he urges archaeologists to use "alleged knowledge warranted with one set of theory-based arguments as the basis for assessing knowledge

that has been warranted or justified in terms of an intellectually independent argument," to set up "an interactive usage of our knowledge . . . to gain a different perspective on both sets of knowledge" (1989: 230).

In these various forms, considerations of horizontal independence can be as important in determining the credibility of any given line of evidence as are considerations of security (in any of the senses described), or the requirement that individual linking principles should be (vertically) independent from the broader claims they may be used to support or refute. When distinct lines of evidence fail to converge, aspects of the ladening theory that had been considered unproblematic may suddenly be thrown into question; they expose an "area of ambiguity," as Binford puts it (1989: 224, 230).[15] As Shanks and Tilley (1982) argue, the strategy of setting up lateral constraints can clarify ways in which the past context in question is different from, and often more complex than, entrenched assumptions had allowed. It is in fact dissonance between (independent) lines of inference and analysis that originally led anti-/postprocessualists to insist on the need to consider internal, ideational, or cognitive dimensions of the cultural past, a point that some processualists have accepted (e.g., Renfrew 1989a). When independently constituted lines of evidence *do* converge, they can provide much more compelling support for the model(s) of past systems or activities with which they are consistent than could any individual line of inference. As Tilley argues, referring to the analysis of parallel formal and temporal structures that emerge in a number of different lines of evidence related to Swedish megalithic tombs—the orientation and structure of tombs, the distribution of grave goods in association with them, the (divergent) elaboration of ceramic design both in association with tombs and settlement sites—it is the demonstration of "links between different aspects of the material-culture patterning" that "lends some credibility to the (interpretive) arguments presented" (1984: 144).

What emerges as common ground in the debates between processualists and post-/antiprocessualists is, first and foremost, a commitment to some form of mitigated objectivism. Although Shanks and Tilley reject all abstract, universalistic conceptions of objectivity, they do insist that the interesting question "is not *whether* objectivity exists" but "what it *is*," and they explicitly endorse what they describe as "a particular and contingent objectivity" (1989: 43; emphasis in the original). Indeed, at one point Shanks and Tilley conclude that it is meaningful to "speak of the *final primacy of objectivity*" (44; emphasis in the original). There are striking parallels here not only with Hodder's endorsement of a "guarded commitment to objectivity" (1991) but also, ironically, with Binford's postpositivist notion of "relative objectivity" and with Renfrew's argument, in critical response to Shanks and Tilley, that "it is not necessary to claim that the data must be in some absolute sense 'objective' . . . in order to propose their use in the evaluation of truth claims" (1989a: 36). By the late 1980s, even the strongest advocates of science in archaeology had abandoned claims to epistemic absolutes concerning the stability and autonomy of evidence, and their anti-/postprocessual critics had substantially qualified their early rejection of objectivity as an unavoidably incoherent and unobtainable ideal. All parties to the debate seem prepared to countenance objectivity, in mitigated form, as a regulative ideal that is crucial to archaeological practice.

Moreover, there is substantial convergence in how this mitigated objectivity is understood, at least in outline. Where evidence cannot be treated as a stable, foundational given, the factors that inform assessments of degrees of objectivity have to do with the inferences by which archaeological data are interpreted as evidence. Mitigated objectivity is achieved insofar as the ladening theory —the body of middle-range, linking principles— that archaeologists use to constitute archaeological data as evidence is itself secure in the various senses described, and these judgments of security are reinforced to the extent that lines of evidence are independent along vertical or horizontal dimensions. It is a fine irony, where independence is concerned, that what makes it possible for archaeological evidence to "resist theoretical appropriation" and thereby serve as a measure of "relative" or "particular and contingent" objectivity is precisely the *disunity* of the sciences on which archaeologists rely in the process of building or borrowing the resources they need in order to bring their data to bear (as evidence) on these theories (more of this in chapter 15).

I am intrigued by a further irony that would seem to reveal a final point of convergence between processual and anti-/postprocessual archaeology. Despite disclaiming any concern with the "red herring" of paradigm dependence, Binford does recommend that to control for the residual blinkering effects of such dependence, archaeologists should deliberately shift frameworks; they should bring into play "multiple perspectives" (1989: 486). It is important, he argues, to seek "some external frame of reference with respect to which we can appreciate [the] content [of our own paradigms.] . . . [A]nother paradigm is a good frame of reference, a different base from which to view experience" (486). With this statement, it would seem, Binford advocates just the sort of pluralism that Shanks and Tilley have tried to promote as a means of enhancing the potential objectivity of archaeological knowledge, in the newly qualified and fallibilistic sense endorsed by all parties to the debate. Far from being antithetical to scientific ideals, these qualifications make it clear that pluralism and theory-ladenness are essential to scientific practice. And under those conditions, the red herrings brandished on both sides of the current divide lose their rhetorical force.

Bootstrapping in the Un-natural Sciences—Archaeology, for Example

In its most general formulation, the central epistemological problem in archaeology is that which motivates Glymour to develop his bootstrapping account of confirmation: it is the problem of showing how charges of circularity can be met in contexts in which evidential grounds for evaluating theory are themselves theory-dependent. The specific form of circularity that threatens in archaeology is much like that described by Meehl in his discussion of bootstrapping strategies for using clinical data in psychoanalysis. There is always the danger that the unconscious themes an analyst is able to disembed, by virtue of training and theoretical sophistication, are arbitrary constructs.[1] This worry had been raised directly by Wilhelm Fliess in the summer of 1900 at a conference in Achensee, when he objected that Freud is a "'thought reader' who read[s] his own thoughts into the minds of his patients" (as quoted in Meehl 1983: 360). Time and again, when archaeologists become methodologically self-conscious the problem that occupies them is precisely this: archaeological data are so enigmatic and fragmentary that their identification as cultural and their interpretation as a record of the past risks collapsing into large-scale cultural mind reading in which the past is reconstructed in the image of a familiar present, or in the image of entrenched beliefs about unfamiliar (past and other) cultures.

In developing his bootstrapping account of confirmation, Glymour's aim is to show how evidence can bear on a theory in a discriminating, noncircular way even when that theory is used to establish the inferential link between evidence and test hypothesis. Evidence bootstrap-confirms a theory if "using the theory, we can deduce from the evidence an instance of hypothesis, i.e., an hypothesis comprising or instantiating the test theory, and the deduction is such that it does not guarantee that we would have gotten an instance of the hypothesis regardless of what the evidence might have been" (1980: 127). Glymour takes his cue from Newton (Glymour 1980: 207, 222–226): hypotheses are deduced from the phenomena, given linking principles derived from the same theory as the hypotheses under test. It is the *structure* of relations between these components of an encompassing theory that ensures that the argument from evidence is not circular.

Glymour goes on to argue that this strategy of inference should appear most explicitly in the developing and "un-natural" (social) sciences, where novel theories are being formulated or applied to new domains (1980: 172). Here, he says, there will be little in the way of developed "substantive principles about the bearing of evidence" (291) to obscure the essential bootstrapping structure of confirming arguments. In other respects, however,

Glymour's account makes it seem quite implausible that bootstrapping should be especially evident in the developing sciences. For example, it is an important structural requirement of bootstrap confirmation, as Glymour describes it, that test theories provide a determinate computation of values for all relevant variables; yet, as van Fraassen has observed, developing sciences can rarely meet such a condition (1983: 32–33). Such incapacity is particularly likely to be found in "un-natural" sciences; not only are social scientists often unable to specify relations among variables closely enough to allow calculation of their values from one another, but frequently they are uncertain what range of variables must be taken into account. It would thus seem that insofar as bootstrap strategies are employed in these contexts, they will necessarily diverge from Glymour's model in a number of respects.

I examine an example of archaeological testing that conforms to Glymour's model in broad outline; my aim is to specify how bootstrap strategies function when a theory is not just "becoming more testable" (van Fraassen 1983: 33) but is in the initial stages of development, or is undergoing extensive reformulation. There are three interdependent respects in which bootstrap practice departs from Glymour's ideal in such testing situations: testing is not strictly theory-contained, the theory-mediated inference from evidence to test hypothesis is not exclusively deductive, and structural considerations do not displace or take precedence over substantive considerations. My constructive thesis is that bootstrapping in developing and exploratory sciences is as much a process of theory construction as of theory testing.

ARCHAEOLOGICAL TESTING

Archaeology is a paradigmatically "un-natural" field in Glymour's sense, and it is one in which a preoccupation with establishing scientific modes of practice has sometimes obscured the bootstrapping nature of the forms of inference on which archaeologists typically depend, whatever methodological ideals hold sway. Whether practice is modeled on positivist, deductivist ideals—what Glymour describes as a "fantasy image of physics" (1980: 292)—or is unapologetically inductive, the use of archaeological data as evidence of a cultural past is inevitably mediated by an extended network of auxiliary hypotheses, some of which derive from the same general theory of cultural phenomena that underlies (that incorporates or entails) the explanatory hypotheses archaeologists are concerned to test. This interdependence illustrates Glymour's point that testing in this context, as in many others, is a three-place relation in which confirming arguments move from evidence to test hypothesis via auxiliaries. As such, it poses the kind of problem that Glymour claims can be circumvented by bootstrap testing, a methodology that he characterizes as "relevant wherever arguments about the possibility or impossibility of knowing something turn on questions of alleged circularity" (376).[2]

One influential family of responses to this problem is strikingly like Glymour's. The New Archaeologists insist that archaeological data can provide discriminating evidence for or against a test hypothesis so long as mediating theories establish a determinate relationship between the values of measurable (material) and hypothetical (cultural/behavioral) variables, and do not arbitrarily guarantee confirmation whatever the empirical results of inquiry. The ideal that animates much of this work, given the positivism of the New Archaeology, is a commitment to build a body of background theory capable of securing the deduction of hypotheses from the evidence, the hallmark of Glymour's bootstrapping model. This commitment to institute a form of bootstrap-testing methodology is also aligned with a reconceptualization of the cultural subject matter. New Archaeologists argue the case for treating human behavior and its material remains as the outcome of systemwide adaptive responses to material conditions of life. Insofar as this materialist ecosystem model serves New Archaeologists both as a general framework for interpreting archaeological data and as the source of test hypotheses about the cultural past, it raises all the problems of circularity that Glymour's model is intended to address.

In the classic examples of New Archaeology–inspired research, ecosystem commitments clearly inform the design and interpretation of empirical tests of local explanatory hypotheses. Consider, for example, the research program developed by Hill and by Longacre at two twelfth- to thirteenth-century pueblos, Carter Ranch and Broken K, in the Hay Hollow Valley (Hill 1966, 1968, 1970; Longacre 1964, 1966, 1968). The theoreti-

cal problem of interest in this area was to explain the widespread phenomena of population decrease and aggregation that took place throughout the U.S. Southwest immediately before and during the time at which the Carter Ranch and Broken K Pueblos were occupied (ca. 1100 to 1280 C.E.) and that resulted, after 1300 C.E., in abandonment of most of the region. The standard hypotheses about this dramatic collapse were unsupported; there was no evidence of invasion or violent internal conflict, such as would require aggregation in defensible villages, or of extensive disease, and there was no indication of catastrophic change in the environment on the scale of the cultural events to be explained (e.g., regionwide resource depletion or extensive drought).

Given their materialist commitments, New Archaeologists working in the U.S. Southwest were inclined to entertain some version of the last hypothesis, that of ecological collapse. And their systemic conception of culture suggested that relatively less dramatic changes in environmental conditions than had been envisioned might well have been the trigger that set off a sequence of local and restricted adjustments whose cumulative effect was the large-scale transformation of pueblo culture documented archaeologically. They thus attributed greater significance than had been traditional to paleoenvironmental evidence of a regionwide shift in the pattern, but not in the overall annual amount, of rainfall: gentle dispersed winter rainfall gave way to torrential summer storms of a sort that would have increased erosion and diminished the effective surface moisture. While this climatic shift would not have compromised agricultural production across the whole region, it would have begun to restrict maize production in the more marginal upland areas after 1100 C.E.; this change, in combination with population pressure, could have quickly created local shortfalls. The hypothesis Hill and Longacre entertained was that one of the few viable responses open to those who resisted returning to a fully mobile foraging subsistence pattern would have been the development, or increased exploitation, of social mechanisms for pooling regional resources that included intensified regional exchange, increased intersite cooperation, and eventually aggregation. The dramatic aggregation and decline of the population in succeeding generations would then be explicable as a culturally mediated response to gradual but significant changes in the environment, consistent with the encompassing ecosystem model of culture.

This hypothesis and, more generally, the ecosystem theory it instantiates led Hill and Longacre to focus on a number of variables that had not previously been analyzed or reconstructed in any detail: fine-grained shifts in patterns of resource exploitation that might reflect environmental pressure, internal intrasite and intra-assemblage variability that might indicate local change in the social structure and level of integration of prehistoric pueblo communities, and regional trade networks that suggest a system of redistribution that might have buffered those living in areas of shortfall. In connection with the first of these factors, Hill established that the occupants of Carter Ranch and Broken K, the two largest and latest sites in the Hay Hollow Valley, were under increasing resource pressure during the period immediately before abandonment; the faunal data and plant remains showed a continuous decline in dependence on wild plants and small game (e.g., Hill 1966: 26–28). Hill and Longacre were among the first to attempt to investigate the second factor (internal shifts in social organization) and their results here are most striking.[3]

In his investigations at Carter Ranch Pueblo, Longacre established a significant statistical association between ceramics painted with distinctive clusters of design elements and three separate sectors of the pueblo (1964, 1968: 98). He argued that this association could not be accounted for functionally or temporally. The stylistic differences do not correspond to activity areas or to different periods of occupation, but they might plausibly be explained as related to social differentiation within the pueblo community. His hypothesis was that by 1100 C.E., some 100 to 300 years earlier than postulated by ethnohistoric construction (Hill 1970: 74), the matrilocal residence system and associated matrilineal system of descent typical of contact period pueblos had already been established, but nothing like the level of social integration typical of later pueblos had been achieved; formerly autonomous and dispersed lineage units coexisted in single village settlements but retained their social distinctness. If established, this hypothesis is significant because it strongly suggests that aggregation was indeed a response to environmental pressure, not a func-

tion of independent shifts in dominant social norms.

Because neither the orienting theory nor the test hypothesis incorporates any specific, well-established principles about the relationship between the material and the social variables in question, Hill and Longacre depend heavily on ethnographic analogy to build linking arguments that bring archaeological data to bear on their hypothesis of internal social differentiation. These interpretive arguments run as follows: if, as in modern pueblos, women were the primary producers of ceramics and passed on design styles generationally, learning design styles primarily from their mothers, then a localization of ceramic design (such as Longacre identified at Carter Ranch) could be expected to occur if kinswomen lived in cross-generationally stable residential groups, as under a matrilocal residence system. Hill replicated Longacre's results in his analysis of the ceramic data from Broken K Pueblo (1966: 17, 21; he cites Longacre 1964), and then undertook to test for corroborating patterns of stylistic differentiation and distribution in other classes of artifacts typically associated with women's activities. The result was strong empirical confirmation of the test implications about intrasite variability that had been derived from the hypothesis that pueblos of the period comprised socially distinct residential units. The size of these subcomponents reinforced the hypothesis that these pueblos were an amalgam of village and homestead units that had previously been dispersed throughout the region, now coexisting next to the most stable supply of water in the valley in a final effort to survive in the area as sedentary agriculturalists. Hill's and Longacre's research thus not only confirms an explanatory hypothesis that was initially just a sketch but also further specifies its details along lines suggested by the encompassing theory. They cite these gains in content and specificity as test results that improve the empirical credibility of the theory much beyond a mere demonstration that the archaeological data conform to its expectations.

CONSTRUCTIVE BOOTSTRAPPING

At all levels of analysis Hill and Longacre construct confirming arguments that move from evidence to test hypothesis, not the reverse; this pro-cedure is unavoidable inasmuch as their arguments about evidential significance are, in part, constitutive of their test hypothesis. Moreover, confirmation of their test hypothesis depends explicitly on assumptions linking the evidence in question to the conditions postulated by the test hypothesis. To this extent, their arguments fit Glymour's model: their arguments are in principle "deductions from the phenomena" mediated by interpretive principles that are, ideally, determinate. It is also clear, however, that bootstrapping in this context is not a matter of using the resources of a single subject-specific theory to establish tests of its own empirical adequacy. Not only is the theory in question incomplete, but the range of conditions responsible for the production of an archaeological record is so great that even if it were complete and comprehensive, it could not be expected to specify relationships between all the variables that archaeologists must consider in constructing linking arguments. In the cases discussed here it was crucial to reconstruct certain noncultural variables—environmental conditions and material constraints on resource exploitation—which required an appeal to independent bodies of scientific theory, primarily paleobiology and ecology. Absolute dating of all kinds, as well as reconstructions of prehistoric technology and subsistence practices, routinely depends on collateral theory of this sort. Even when the variables in question are cultural, the relevant mediating theories, usually drawn from cultural anthropology, are notoriously incomplete in the areas of particular interest to archaeologists; they may identify the range of sociocultural variables that concern archaeologists but typically they do not specify relations between them and the material variables accessible to archaeologists. In assessing the evidential import of their data, archaeologists must appeal to background knowledge and ethnohistoric sources, as Hill and Longacre did, for an understanding of the sociocultural conditions that could have produced the record; this is information that might well be subsumed by general linking principles and be incorporated into a comprehensive theory of cultural theory if one were fully developed. But as things stand, bootstrap testing in a discipline like archaeology is not, and perhaps could not ever be, theory-contained in the manner required by Glymour's model.

This open-endedness is at once a source of dif-

ficulty and of strength. The difficulty is that arguments concerning the evidential import of archaeological data are bound to be inconclusive. Glymour's ideal of confirmation by deductive linking arguments may perhaps be approximated in archaeology when the hypothesis under test is exclusively concerned with the biophysical conditions responsible for the archaeological record (the paleoenvironment), or with human behaviors that are very tightly constrained by such conditions. These kinds of inferences are crucially important when they can be made; but in the vast majority of cases that interest archaeologists, especially those that concern social, cultural variables, they must rely on abductive forms of inference that are typically analogical (for a definition of *abduction,* see chapter 5, n. 15). In this regard, archaeological arguments of confirmation consistently depart form Glymour's deductive ideal.

The strength of such arguments, which the New Archaeologists were intent on exploiting, is that when they draw on resources external to the theory under test, they set up a system of internal constraints between different lines of supporting evidence. This promises not only a check on the accuracy of specific linking assumptions but, when consilience emerges, it may also dramatically improve the constructive support that any one type of test evidence can provide an hypothesis considered on its own.[4] When, for example, evidence interpreted in light of sources as diverse as bio-ecological theory, pueblo ethnography, and theories about cultural evolution all converge on expectations derived from the hypothesis that pueblo aggregation was a response to environmental stress, Hill's and Longacre's test data provide their theory particularly strong confirmation; it is implausible that such consilience could be an artifact of theoretical expectation.

It is important to recognize, however, that this strength derives from a convergence of substantive considerations of exactly the sort that Glymour insists are secondary and incapable of accounting for "the fine points of the distribution of praise and blame among hypotheses" (1980: 375). Faced with a lack of developed theoretical understanding in the relevant areas, Hill and Longacre resort not to structural considerations but rather to more tentative, ad hoc, and particularistic forms of substantive consideration to assess the credibility of their hypothesis and the significance of

the evidence they bring to bear on it. In their arguments from evidence, they use analogical inference to import empirical information about the nature of their evidence and how it might have been produced. It is hard to see how else they could have proceeded. How could a theory be developed that specifies the relations holding among component variables in the absence of substantive knowledge of the subject domain in question? It would seem that the structure of a theory and of inferences that bootstrap-confirm is unavoidably parasitic on substantive considerations of content. This suggests that Glymour's emphasis on the primacy of structural considerations is misplaced, even (or especially) for un-natural sciences at early stages of development.

It is also important to note that the use of analogical arguments to import substantive considerations has a constructive aspect that Glymour overlooks. For Longacre, the ethnographic data on pueblo ceramic production serve primarily as the source of fragmentary insights about links that might hold between his archaeological data and the social organization of prehistoric pueblo communities. In order to bring a wider range of archaeological material to bear on this hypothesis about social organization, Hill generalizes on that insight; he proposes a linking argument in which he appeals directly to the hypothesis that stylistic similarity at the level of the smallest units of design is an index of intensity of social interaction (1966: 17; see also the reconstruction of this argument in S. Plog 1980). The discovery that this principle anticipates and makes sense of patterning in a much wider range of artifact classes than originally were considered not only confirms the test hypothesis but may also reduce the uncertainty of the linking hypothesis itself; it suggests the existence of "ancestral" relations among hypotheses with regard to evidence (van Fraassen 1983) by which, contrary to Glymour's model, confirmation extends at least weakly to the conjuncts of a successful test hypothesis. More generally, it suggests that in Hill's and Longacre's hands, bootstrap confirmation is a process not just of testing a hypothesis that instantiates their developing theory but of building into this theory the resources it needs to raise itself confirmationally by its own bootstraps. Glymour's focus on structure obscures precisely the features of this process— the open-endedness, the reliance on analogy, and

the centrality of substantive considerations—that are essential to its constructive function.

I thus conclude that bootstrap confirmation in developing sciences is not only a reflexive, probative strategy for evaluating novel theories but also, and necessarily, a process of using empirical and theoretical knowledge established in a variety of contexts to build and refine such theories. The judgments researchers render concerning the bearing of evidence are therefore irreducibly a function of the background information that they have available and recognize as relevant. As such, these judgments constitute not simply an assessment of the credibility of discrete components of an encompassing theory but also an evaluation of how a given theory may most fruitfully be developed.

14

The Constitution of Archaeological Evidence

Gender Politics and Science

I begin with a digression that will situate my discussion of archaeological uses of evidence in the wider context of debate about the objectivity and value neutrality of archaeological understanding. My aim is to show that although archaeology is a thoroughly social and political enterprise, evidential constraints are not reducible to the interests of individual archaeologists or to the macro- and micropolitical dynamics of the contexts in which they operate. In fact, they are in some respects constitutive of political interests. The model of how evidential constraints operate on which I draw was introduced in chapters 12 and 13; my thesis is that although archaeological evidence is thoroughly laden with theory—although it is unavoidably a construct, open to question and revision—it can nonetheless impose decisive limitations on what can be claimed about past cultural systems, their internal dynamics, and their trajectories of development and transformation. I elaborate this model and illustrate it with examples drawn from the rapidly growing corpus of archaeological research on questions about women and gender, some of which is explicitly feminist in perspective.

From the outset, critics of scientific, processual archaeology have advocated feminist approaches, usually in the abstract and in prospect, as exactly the sort of politically engaged research they hope will displace the scientism and pretensions to value neutrality that they associate with the New Archaeology (Shanks and Tilley 1987: 246; Hodder 1986: 159–161, 1991: 7). It is striking, however, that they rarely made feminist problems a primary focus of their own research,[1] and that few of those who have pursued feminist lines of inquiry embrace the strongly constructivist, often ironic view of the research enterprise associated with post- and antiprocessual critique. Indeed, the feminist analysts typically make effective use of quite conventional appeals to evidential constraints to demonstrate the need for substantially rethinking explanatory and reconstructive models that leave women and gender out altogether or that depend on ethnocentric and androcentric presuppositions about gender relations. And in the process, they routinely produce results that diverge sharply from expectations, sometimes calling into question the presuppositions that informed their own reframing of questions and reinterpretation of the archaeological data. Central to this program of research is an interplay between evidential constraints and social, political factors that is poorly comprehended by positions articulated at either the objectivist or the antiobjectivist extremes that dominate current archaeological discussion, an

interplay that figures in parallel debates in other social sciences and in the sociology and philosophy of science (see chapter 11).

ARCHAEOLOGY AS POLITICS BY OTHER MEANS

It is by no means a new insight that archaeology is a deeply political enterprise. However pervasive and influential the rhetoric of (unmitigated) objectivity may be among professional archaeologists, the practice and products of archaeology do reflect the standpoint and interests of its makers. But even though this observation is by now a commonplace in the archaeological literature, it is still regarded with suspicion, if not outright hostility, by a great many archaeologists. It constitutes a profound challenge to the conviction—a central and defining tenet of North American archaeology—that the social and political contexts of inquiry are properly external to the process of inquiry and to its products. In general terms, as Rouse describes these ideals, it is assumed that "Knowledge acquires its epistemological status independent of the operations of power. . . . Power can influence our motivation to achieve knowledge [in specific areas] and can deflect us from such achievement, but it can play no constructive role in determining what knowledge is" (1987: 13, 14).

Archaeologists have long nourished the hope that if properly scientific modes of inquiry were adopted, they might secure a body of evidence that is autonomous of, and provides a decisive check on, the range of idiosyncratic and contextual interests that influence archaeological interpretation, either as a consequence of internal dynamics (the micropolitics of the discipline or the interests of individual practitioners) or as forces that impinge on the discipline from outside (external, sociopolitical factors). Despite the continuing influence of these ideals, however, there has been no shortage of critical analyses that demonstrate (with hindsight) how profoundly some of the best, most empirically sophisticated archaeological practice has reproduced manifestly nationalist, racist, and, on the most recent analyses, sexist understandings of the cultural past; confronting test hypotheses with evidence seems not to be proof against intrusive bias. These critiques take a number of forms. By way of a short and selective summary, I here distinguish five levels and types of critique that

have appeared in recent years. Later in the chapter I return to a detailed analysis of several examples of critical analysis that exposes sexist bias.

CRITIQUES OF ERASURE

First are the critiques that expose straightforward erasure, where the choice of research problem or the determination of significant sites or periods or cultural complexes systematically directs attention away from certain kinds of subjects—namely, those that might challenge the tenets of a dominant ideology or might be particularly relevant to the self-understanding of subordinate and oppressed groups. These include the critiques of colonial period archaeology in North America that have given rise to vigorous new areas of research: for example, the archaeology of slavery, sharecropping, and free black settlements in contexts where it had been assumed none existed, or where the great houses of prominent planters had been the exclusive focus of attention (Singleton 1985; Epperson 1990; Orser 1990, 1999; Yentsch 1994), and a range of studies that are now documenting the enormous diversity of those who populated the West (Wylie 1993a).[2] What gave rise to these new fields of interest was, in part, a concern that where archaeologists had failed to consider the material record of slavery and of poverty, of African American settlements and a highly diverse frontier, they had helped ensure that silence on these aspects of U.S. history would be enforced by a lack of relevant data. Critiques from South and Central America and from various parts of Africa make it clear that the typical preoccupations of first world and neocolonial research programs—such as discovery of the most primitive human and hominid remains (e.g., palaeoanthropology in the Rift Valley) and documentation of the now-eclipsed glories of ancient civilizations (in Mesoamerica and South America)—systematically obscure the history of oppression and colonization that is crucially relevant to contemporary indigenous and mestizo populations in these areas (Schmidt 1995; Patterson 1995b; Vargas Arenas 1995; Vargas Arenas and Sanoja 1990; Irele 1991, as cited by Vargas Arenas 1995). And since the late 1980s a rapidly expanding body of feminist critique documents how women and gender have been left out of account even when they are a crucial part of the story to be told (see below; Conkey and Spector 1984; Spector and Whelan 1989).

Even when marginal subjects are acknowledged and investigated as part of the subject domain of archaeology, they are often characterized in terms that legitimate a different kind of colonizing representation. A common second type of critique focuses attention not on erasure but on systematic, and manifestly interested (standpoint-specific), distortion in how various archaeological subjects are understood. Some critics have argued that this distortion is evident even in the new work on African American sites and heritage (e.g., Potter 1991). Most often such critiques challenge the presuppositions of long-established research programs. Renewed studies of early Spanish exploration and settlement in the Americas undermine conventional contrasts and stereotypes (Deagan 1990; Thomas 1991a; contributors to Thomas 1991b), and in an early discussion of "the image of the American Indian," Trigger (1980) traces the legacy of nineteenth-century evolutionary beliefs that compromises archaeological thinking about the complexity and diversity of Native American cultures. He subsequently extends this analysis to the presuppositions that lie behind a pervasively romantic view of early Native American responses to contact with Europeans, a view that was intended to correct earlier accounts but represents Native Americans as essentially tradition- and culture-bound. Such representations selectively deny these subjects a capacity for rational self-determination, obscuring the considerable diversity in the response of the First Nations to Europeans that, Trigger (1991) argues, the archaeological record of the period reveals in a number of ways.

Trigger's critique has been extended by Handsman (1989, 1990) and by Handsman and Richmond (1995), among others. They decry the dependence of North American archaeologists on Eurocentric models of community and settlement,[3] documenting how this failure to recognize native presence in anything but European-style settlements was crucial in legitimating a rhetoric of absence that has been used, throughout the long history of native dispossession, to justify the appropriation of native lands. In a similar vein, Hall documents the inherent racism of "archaeolog[ies] of the colonized . . . mostly practiced by the descendants of the colonizers" (1984: 45) in southern Africa, where the presumption of indigenous absence and an erasure of class conflict

have been reinforced by the dependence of archaeological analysis on reified, externally imposed concepts of tribal identity (see also D. Miller 1980). Feminist critiques of androcentrism in archaeological research often operate at this second level of analysis; they draw attention not just to the absence of any consideration of women and gender but also to the projection onto prehistory of presentist and ethnocentric assumptions about sexual divisions of labor and the status and roles of women in prehistory.[4]

In all these cases the imposition of prejudgments about what must have been the case in the cultural past determines not just what range of reconstructive models will be considered but also what sorts of data will be recovered and how they will be interpreted as evidence. At their most radical and pessimistic, the critics responsible for this second type of critique insist that the stereotypes, evaluative commitments, and "mythologies" (Thomas 1991a) that inform archaeological research are unavoidably self-perpetuating: they foreclose the collection or serious consideration of counterevidence that might call these presuppositions into question.

POLITICAL RESONANCE

At a more general level, a number of synthetic critiques have been advanced that delineate broad patterns of congruence or "resonance" (Patterson 1986a, 1986b) between the interests of large-scale geopolitical elites and entrenched archaeological research programs.[5] For example, in his compendious history of archaeological thought, Trigger (1989b) documents the entanglement of archaeology, in every context in which it has flourished, with nationalist programs of territorial expansion and cultural legitimation. At a less global scale, Patterson has argued that one can discern in the training and interpretive practices of North American archaeologists—in the discourse, the "content and form, level of exposition, and the chosen vehicles for publication" typical of the field (1986a: 21; see also 1986b)—two distinct communities whose views of the past resonate with the interests of the eastern establishment (that is, international capital and its allies) on the one hand, and with the core culture (midwestern, national capital and its power base) on the other.

At an even more general level are critiques of the enterprise of archaeology as a whole that indict its methodological and epistemic stance—its commitment to scientific ideals of objectivity—on the grounds that these effectively reinforce, rather than counter, the partiality of its makers. The British postprocessual critics of the positivism associated with the New Archaeology are among the most outspoken in this vein (see chapter 12). For example, Tilley has argued that "living in Western society of the 1980s is to be involved with and, in part, responsible for prevailing [grossly inequitable] social conditions" (1989: 105); under these conditions, the attempts made by archaeologists to maintain a stance of political neutrality and professional disengagement serve not to defuse the problem but to sustain and legitimate the existing order.

EXPLANATORY CRITIQUES

While the foregoing types of critique reveal, at various levels of analysis, systematic gaps, biases, and distortions in the results of archaeological inquiry that we should be prepared to question, for the most part they provide no detailed explanation of how these compromising effects are produced or why they persist. That is, they offer little account of the conditions under which, or the mechanisms by which, local and global political interests come to shape the content of archaeological understanding, generating the sorts of resonances and congruencies—the systematic silences and replication of stereotypes—that arise at the four different levels of analysis I have identified. A fifth form of critique, perhaps the least developed but one that is crucially important in its potential to provide these missing explanatory links, consists of analyses of how the internal conditions of archaeological practice—the micropolitics of archaeology conceived as a community and as a discipline articulated with a range of institutions—shape the direction and results of inquiry.

Several studies along these lines were reported in a landmark collection of essays, *The Socio-politics of Archaeology* (Gero, Lacy, and Blakey 1983); they illustrate how, for example, the structure of rewards institutionally entrenched in archaeology may reinforce a disproportionate interest in origins research and regional syntheses, much beyond the intellectual warrant for such research

(Wobst and Keene 1983). Feminist scrutiny of the discipline has resulted in a number of critical sociological analyses of familiar patterns of differential support, training, and advancement of women in the field, as well as of strong patterns of gender segregation in the areas in which women typically work,[6] but much of this equity research remains disconnected from questions about androcentric or sexist bias in the content of archaeological accounts. One study that does make this connection is Gero's analysis (1993) of the assumptions and conditions that have shaped Palaeoindian research on the earliest human populations in the Americas. Gero notes a strong pattern of gender segregation in the field. The predominantly male community of Palaeoindian researchers had focused almost exclusively on stereotypically male activities: specifically, large-scale mammoth and bison kill sites, technologically sophisticated hunting tool assemblages, and the replication of these tools and of the hunting and butchering practices they are thought to have facilitated. The women in the field have largely been displaced from these core research areas; they work on expedient blades, flake tools, and so-called domestic sites, and they have focused on edge-wear analysis. This pattern of segregation in the workplace is reinforced by gender bias in citation patterns. In the field of lithics analysis generally, Gero argues, women are much less frequently cited than their male colleagues, even when they do research that is more typical of men in the field, except when they publish with a male coauthor. Not surprisingly, their work on expedient blades and edge-wear patterns is almost completely ignored, even though these analyses provide evidence that Palaeoindians exploited a wide range of plant materials, presumably foraged as a complement to their diet of Pleistocene mammals.

At the very least, these disciplinary dynamics, these "social *relations* of palaeo research practice" (Gero 1993: 36; emphasis in the original), have reinforced an unfortunate incompleteness in entrenched accounts of Palaeoindian culture. More seriously, Gero charges, they substantially derail or, as she puts it, impose a limiting "en-railment" on the research program as a whole: "women's exclusion from Pleistocene lithic and faunal analysis . . . is intrinsic to, and necessary for, the bison-mammoth knowledge construct" (1993: 37). The central problematic of Palaeoindian research is

created by the fact that the technology, subsistence activities, social organization, mobility, and patterns of occupation of the landscape are characterized primarily in terms of male-associated hunting activities. It is this focus that generates the puzzles that dominate Palaeoindian research: how to explain or reconstruct what happened to the mammoth hunters when the mammoths went extinct. Did Palaeoindians disappear or die out, to be replaced by the small game– and plant-foraging groups that succeeded them? Did they effect a miraculous transformation of their entire form of life as the subsistence base changed? These questions only arise, Gero argues, if researchers ignore the evidence that Palaeoindians depended on a much more diversified set of subsistence strategies than acknowledged by standard "man the (mammoth/bison) hunter" models—precisely the evidence produced (largely) by women working on microblades and use-wear patterns. To overcome this incompleteness requires not just that practitioners take into account female-associated tools but, in addition, that they revalue women's work—the work of both contemporary women archaeologists and of Palaeoindian women in prehistoric contexts—and systematically rethink the ways Palaeoindian culture has been conceived as a subject of archaeological inquiry.[7]

Taken together, critiques at these five levels are understood by many to demonstrate more than just that archaeology is partial, in the sense that external interests and power relations may determine what questions will be taken up and what uses will be made of the results of inquiry. This admission would leave disciplinary practice and its products uncompromised by values, interests, and the social relations and material conditions of its operation. Rather, critiques of the kinds I have described are often seen to reveal sociopolitical dynamics that are intrinsic to disciplinary practice and are constitutive of its results at all levels. They show how external (noncognitive) factors determine what data will be collected and how they will be construed as evidence, what interpretive and explanatory hypotheses will be taken seriously and accepted (sometimes evidence notwithstanding), and what range of revisions or corrections will be considered when evidence resists being appropriated in terms of entrenched presuppositions. As Rouse has put this point with reference to general challenges to objectivism, such critiques make

it clear that "power does not merely impinge on science and scientific knowledge from without. Power relations permeate the most ordinary activities in scientific research. Scientific knowledge arises out of these power relations rather than in opposition to them" (1987: 24). It is this extension of sociopolitical critique, especially as attributed to feminists, that many archaeologists reject out of hand as a reductio ad absurdum of the central arguments of postprocessualism.

GENDER RESEARCH AND THEORETICAL AMBIVALENCE

Conkey and Spector made the first widely influential argument for feminist approaches to archaeological research in 1984, and a watershed collection of essays devoted to work in this area appeared seven years later (Gero and Conkey 1991), the outgrowth of a small working conference organized by Conkey and Gero in 1988.[8] In organizing this conference, Conkey and Gero approached a number of colleagues working in widely different areas of prehistoric archaeology and asked if they would be willing to explore the implications of taking gender as a focus for analysis in their various fields; even several years after the appearance of Conkey and Spector (1984) there was little feminist work in print or in process. Most of those approached had never considered such an approach and had no special interest in feminist initiatives, but they agreed to see what they could do. In effect, Gero and Conkey commissioned a series of pilot projects on gender that they hoped might demonstrate the potential of research along the lines proposed by Conkey and Spector in 1984. Their motivation was explicitly feminist: they sought to engage potentially sympathetic and influential colleagues in the investigation of new questions they thought should be asked concerning women and gender, questions they had come to see as important because of their own political commitments. Although a number of other contextual factors of a sociopolitical nature fed the subsequent groundswell of interest in work in this area, these feminist efforts to mobilize support for research on questions about gender and women in prehistory were a crucial catalyst for the considerable body of work that has since appeared.[9] In short, political interests have played

a key role in shaping the direction of this program of research.

Yet despite the political impetus that gave rise (directly and indirectly) to the diverse programs of research that now address feminist questions about women and gender,[10] the practice and the products of research in this area do not support or instantiate the strongest relativist claims sometimes attributed to postprocessualism. Far from displacing evidential considerations, a feminist standpoint, if anything, enhances a commitment to empirical rigor, especially in the critical inspection of sexist, androcentric presuppositions that have framed much otherwise exemplary research in the field. Indeed, the new research on gender frequently reflects a wariness of strong constructivist conclusions, and in this attitude feminist archaeologists are not alone. Feminist practitioners in a number of contexts have been alert to the relativist implications that are often presumed to follow from their own wide-ranging critiques of extant traditions of scientific practice and its claims to objectivity. Even those who recommend a postmodern stance as a resource for feminist research acknowledge the dilemma that it creates for feminists or for any who would use postmodern insights "in the interests of emancipation" (Lather 1991: 154). In this connection, and with special reference to feminist critiques of science, Harding argues the need to cultivate strategic "ambivalence"—to embrace both "successor science" projects, which use the tools of existing research traditions to expose their inherent androcentric bias, and the vision of alternatives embodied in postmodern disruption of these projects (1986: 195). Many who are less optimistic express concern that, at the very least, a postmodern stance has "both emancipatory and reactionary effects"; indeed, it may be "especially dangerous for the marginalized" (Lather 1991: 154). The worry is that deconstructive arguments intended to destabilize Enlightenment myths of objectivity and truth are themselves "merely an inversion of Western arrogance" (Mascia-Lees, Sharp, and Cohen 1989: 15); they are an inversion that serves the interests of those who have always benefited from gender, race, and class privilege: "The postmodern view that truth and knowledge are contingent and multiple may be seen to act as a truth claim itself, a claim that undermines the ontological status of the subject at the very time when women and non-Western peoples have begun to claim themselves as subject" (15).[11]

The tension between postmodern and emancipatory projects is evident in much feminist practice in the social and life sciences. On the one hand, feminist critics of science have exposed such pervasive androcentric bias that whatever their intentions, they seem to call into question not just "bad science" but much that passes for "good science," even exemplary science (see Longino and Doell 1983: 207–208; Longino 1990b: 3–15; Harding 1986; Wylie 1991: 38–44). Where this erodes confidence that scientific method is self-cleansing, a guarantor of objectivity, it is often presumed that feminist critics undermine any possibility of claiming greater credibility for their own insights in any but a purely political sense. And yet, the feminists responsible for these critiques are by no means prepared to concede that their accounts are just equal but different alternatives to those they challenge. Where women and gender have been characterized in stereotypically androcentric terms, or ignored in what purport to be humanly inclusive accounts of societies or cultural groups (e.g., in hunting-focused accounts of foraging societies; Slocum 1975 [1974]), historical epochs (e.g., the Renaissance that women did not have; Kelly-Gadol 1977), psychological processes (e.g., the "different voice" in moral reasoning documented by Gilligan 1982), or physiological and cognitive capacities (Fausto-Sterling 1985), the result has frequently been pervasive error and misrepresentation as measured by such standard criteria as empirical adequacy and internal coherence. Indeed, the claim made on behalf of research informed by a feminist angle of vision is often that it is simply *better* science in quite conventional terms (Fausto-Sterling 1985: 9). In a close analysis of exactly how and where androcentrism arises in biology (evolutionary theory and endocrinology), Longino and Doell argue that such critiques of science should not put feminists in the position of having to choose for or against science; we should not have to "turn our backs on science as a whole . . . or condemn it as an enterprise" (1983: 227). Their reason for cautioning against such simple, polarized responses is immediately relevant for understanding feminist practice in archaeology: "the structure of scientific knowledge and the operation of bias are much more complex than either of these responses suggests"

(208). Longino has since argued for the viability of a sophisticated "contextual empiricism" as a philosophical position (Longino 1990b: 215–232), as well as an option for feminist research practice—for "doing science as a feminist" (188)—that preserves a (mitigated) claim to objectivity.

It should not be surprising that the epistemological analysis offered by Longino and Doell and the research practice of many feminist scientists reflect a reticence to embrace a thoroughgoing constructivism about empirical inquiry. At its best, feminist research grows out of a commitment to understand, accurately and in detail, the institutions, attitudes, and practices that oppress women in a diversity of contexts and ways, *so that we can be effective in changing them*. And in this case its roots and inspiration lie in the varied experiences of constraint and dispossession that mark women's lives. Uncompromising constructivism and relativism trivialize these experiences; they deflect attention from questions about how and why they arise and from questions about the structures and conditions that constitute, for any who lack power, intransigent realities that impinge on their lives at every turn. In this respect, such positions embody what seems patently an ideology of the powerful. Certainly a central part of the activist experience of feminists who attempt to change oppressive conditions of life is the realization that effective intervention requires, first and foremost, a sound understanding of the forces we oppose. In short, a commitment to the emancipatory potential of feminism and a respect for the very real constraints we encounter in practice persistently force feminist researchers, theorists, and activists alike back from the extremes of both objectivism and relativism that emerge in abstract debate about the status of empirically grounded knowledge claims (see, e.g., Fraser and Nicholson 1988: 83; Wylie 1992a: 63–64).

These sorts of concerns, which are ubiquitous in discussions of the apolitical and even reactionary implications of (some) deconstructive and postmodern positions (Norris 1990), are not lost on the proponents of an explicitly political (postprocessual) archaeology. As I argued in chapter 12, the most outspoken critics of objectivist, processual archaeology can be seen to retreat from an uncompromising constructivism as soon as it becomes clear that such a position threatens to undermine their own social and intellectual agendas

as surely as it does those of the positivists they repudiate. Hodder qualifies his arguments from underdetermination with the striking observation that even though all facts are constructs, there does exist a real world—and, what is more, "the real world does constrain what we can say about it" (1986: 16); Shanks and Tilley declare themselves realists and invoke a dialectical relationship between object and subject that ensures that archaeological construction is not "free or creative in a fictional sense" (1987: 104). They make it clear that they are not prepared to embrace the view that all claims about the past must be considered equal (245), insisting that "the archaeological record itself" is a source of constraints that may "challenge what we say as being inadequate in one manner or another" (104).

This recurrent ambivalence about "anything goes" relativism among critics of naive objectivism—among postprocessual archaeologists as much as feminist critics of science—raises the question of how empirical (scientific) inquiry can be conceptualized so as to recognize, without contradiction, *both* that knowledge is constructed—it bears the marks of its makers—*and* that it is constrained, to a greater or lesser degree, by conditions that we confront as external realities not entirely of our own making. A fruitful point of departure is the grudging consensus identified in chapter 12. All parties to the current debate acknowledge that although archaeological data must be richly interpreted to stand as evidence, they do (sometimes) have a capacity to challenge and constrain what we claim about the past: they routinely turn out differently than expected; they generate puzzles, pose challenges, force revisions, and canalize reconstructive and explanatory thinking, sometimes raising doubts about even the most well-entrenched presuppositions.

THEORY-LADENNESS RECONSIDERED

A concern with just this nexus of problems can be discerned in the work of those (postpositivist) philosophers of science, including feminist philosophers of science, who have undertaken analyses of how observational and experimental results are stabilized such that, in practice, they often show less arbitrariness of construction than has been insisted on by some of the stronger sociological critics. I have in mind, for example, Longino and

Doell's analysis of the role played by background assumptions in traversing the distance between data, evidence, and hypotheses (1983: 208–210); Longino's subsequent analysis of ideals of objectivity (1990b: 62–82); Shapere's account of the role played by prior information in determining what will count as an observation in physics (1982: 505); and the substantial philosophical and historical literature that has grown up since the mid-1980s on experimental practice (see, e.g., Galison 1987: 7–9; 1988; 1989; Hacking 1988a, 1988b, 1989). On the model I outlined in chapter 12, the key to understanding how archaeological evidence can (sometimes) function as a semiautonomous constraint on claims about the cultural past is to recognize that archaeologists exploit an enormous diversity of evidence—not just different kinds of *archaeological* evidence, but evidence that depends on background knowledge derived from a number of different sources, that enters interpretation at different points, and that can be mutually constraining when it converges, or fails to converge, on a coherent account of a particular past context.

To summarize the earlier discussion, my thesis is that archaeological evidence derives its stability and autonomy from two sources: the *security* of the background knowledge invoked to establish a link between the surviving record and the past events or conditions that produced it and the epistemic *independence* of the evidence thus constituted. The kinds of security at issue here include the credibility of the background knowledge in the context from which it derives and the security of the inferences in which this knowledge is deployed: this last is a function of the nature of the linkages between surviving traces and antecedent causes (the degree to which they are unique or deterministic) and the directness and complexity of the inferential chain required to reconstruct the antecedents. And there are two dimensions on which independence is crucial: the vertical independence of background assumptions from test hypotheses (this is the independence captured in especially stringent terms by bootstrapping models of confirmation) and the horizontal independence from *one another* of linking hypotheses that arises when a number of different sources are used to establish the evidential import of archaeological data (independence in this sense obtains if no one set of linking principles entails the others

as a proper subset of itself, or is confirmed by the same evidence). Horizontal independence allows archaeologists to exploit a strategy of triangulation, setting up a system of mutual constraint among lines of evidence bearing on a common archaeological subject.

It is a significant irony that the role of these evidential constraints is nowhere clearer than in the new feminist work on gender, which is so often identified as precisely the sort of explicitly political research that leads inevitably to corrosive relativism. I will consider here a number of examples from contributions to the groundbreaking 1988 conference on gender research in archaeology that subsequently appeared in *Engendering Archaeology* (Gero and Conkey 1991).

Although, as I have indicated, most contributors to this conference remarked that they began with serious reservations about the approach urged on them by Gero and Conkey—they did not see how questions about gender, which had never arisen before, could bear on research in their fields or subfields—even the most skeptical found that attention to such questions brought to light striking instances of gender bias in existing archaeological research and opened up a range of constructive possibilities for inquiry that had been completely overlooked. One especially compelling critical analysis, developed by Patty Jo Watson and Kennedy (1991), exposes pervasive androcentrism in explanations of the emergence of agriculture in the eastern United States. Whatever the specific mechanisms or processes postulated, the main contenders all assume that women could not have been actively responsible for the development of cultigens even though they also assume that women were responsible for gathering plants (as well as small game) under earlier foraging adaptations, and were responsible for the cultivation of domesticates when horticulture was established. One model turns on the blatantly ad hoc proposal that shamans, who are consistently identified as male, were the instigators of this culture-transforming development; it was their knowledge of plants used for ritual purposes that informed the development of the cultigens on which Eastern Woodlands horticultural practices were based. In effect, women passively followed plants around when foraging, and then passively tended them when the plants were (re)introduced as cultigens by men (P. Watson and Kennedy 1991: 263–264).

The dominant alternative postulates a process of co-evolution by which horticulture emerged as an adaptive response to a transformation of the plant resources that occurred without the benefit of any deliberate human intervention; at most, human patterns of refuse disposal in "domestilocalities" unintentionally introduced artificial selection pressures that generated the varieties of indigenous plants that became cultigens. On this account the plants effectively "domesticate themselves," and women are, once again, represented as passively adapting to imposed change (262).[12]

Watson and Kennedy make much of the artificiality of both models. Why assume that shamans were men, or that dabbling for ritual purposes would be more likely to produce the knowledge and transformations of the resource base necessary for horticulture than the systematic exploitation of these resources as a primary means of subsistence? Why deny human agency altogether and represent the emergence of horticulture as an "automatic process" (1991: 262) when it seems that the most plausible ascription of agency (if any is to be made) must be to women (262–264)? Indeed, Watson and Kennedy observe that they are "leery of explanations that remove women from the one realm that is traditionally granted them, as soon as innovation or invention enters the picture" (264). The common and implicit basis for both theories is, they argue, a set of underlying assumptions, uncritically appropriated from popular culture and traditional anthropology, to the effect that women could not have been responsible for any major culture-transforming exercise of human agency.

In a constructive vein Hastorf, a contributor who works on pre-Hispanic sites in the central Andes, drew on several lines of evidence to establish that gendered divisions of labor and participation in the public, political life of the highland communities in question were profoundly altered through the period when the Inka extended their control in the region; the household structure and gender roles encountered in historical periods cannot be treated as a stable, traditional feature of Andean life that predates state formation (1991: 139). In a comparison of the density and distribution of palaeobotanical remains recovered from household compounds dating to the periods before and after the advent of Inka control, Hastorf found evidence within the sites that over time both

maize production and processing intensified and the degree to which female-associated processing activities were restricted to specific locations increased. In addition, she reports a striking comparison between the sexes of skeletal remains recovered from these sites and the results of a stable-isotope analysis of bone composition for evidence of variability in dietary intake. Although the lifetime dietary profiles of males and females are undifferentiated preceding the advent of Inka control in the region, Hastorf finds that they diverge sharply in the period when evidence of an Inka presence begins to appear. Specifically, males show higher rates of consumption of foods that have the isotope values Hastorf identifies with maize than do females. To interpret this result Hastorf turns to ethnohistoric records that document Inka practices of treating men as the heads of households and communities, drawing them into ritualized negotiations that involve the consumption of maize beer (*chicha*) and require them to serve on obligatory workforces away from their villages, for which they were compensated with maize and *chicha*. She concludes that through this transitional period, the newly imposed political structures of the Inka empire had forced a realignment of gender roles on local communities and households. Women "became the focus of [internal social and economic] tensions as they produced more beer while at the same time they were more restricted in their participation in the society" (152).

Parallel results are reported by Brumfiel (1991) in an analysis of changes in production patterns in the Valley of Mexico in the period when the Aztec state was establishing a tribute system in the region. Through analysis of the density and distribution of spindle whorls, she argues that fabric production, largely the responsibility of women (on ethnohistoric and documentary evidence), increased dramatically in outlying areas but decreased in the vicinity of the urban centers as the practice of extracting tribute payments in cloth developed. On further analysis, she found evidence of an inverse pattern of distribution and density in artifacts associated with the production of labor-intensive and transportable cooked food based on tortillas; the changing proportion of griddles to pots suggests that the preparation of griddle-cooked foods increased near the urban centers and decreased in outlying areas, where the less

demanding (and preferred) pot-cooked foods continued to predominate. She postulates, on this basis, that cloth may have been exacted directly as tribute in the hinterland, while populations living closer to the city center intensified their production of transportable food so that they could participate in the markets and "extradomestic institutions" then emerging in the Valley of Mexico that required a mobile labor force (Brumfiel 1991: 241). In either case, Brumfiel points out, the primary burden of meeting the tribute demands for cloth imposed by Aztec rule was shouldered by women and caused strategic realignments of their household labor. Where the Aztec state depended on tribute to maintain its political and economic hegemony, its emergence, like that of the Inka state studied by Hastorf, must be understood to have been dependent on a transformation that it caused in the way predominantly female domestic labor was organized and deployed.

Finally, several contributors consider assemblages of artistic material, some of them rich in images of women, and explore the implications of broadening the range of conceptions of gender relations that inform their interpretation. In a discussion of the British exhibition *The Art of Lepenski Vir,* Handsman challenges the notion that gender can be treated in essentialist terms, reassessing the ideology of gender difference and the presumption of a timeless, natural, and hierarchical opposition between men and women (1991: 360). He suggests several interpretive options that might be pursued in constructing "relational histories of inequality, power, ideology and control, and resistance and counter-discourse" where gender dynamics are concerned (338–339). In the process he points to a wide range of evidence—features of the images themselves and associations with architectural and artifactual material that might provide them context—that constitutes "clear signs" of complexities, contradictions, "plurality and conflict" (340, 343), undermining the simple story of natural opposition and complementarity told by the exhibit. In a similar vein, Conkey has developed an analysis of interpretations of Paleolithic art, especially images of females or purported female body parts, in which she shows how "the presentist gender paradigm has infused most reconstructions of Upper Palaeolithic 'artistic' life," yielding accounts in which "sexist twentieth-century notions of gender and

sexuality are read into the cultural traces of 'our ancestors'" with remarkable disingenuity (Conkey with Williams 1991: 121).[13] She concludes that whatever the importance of these images and objects, it is most unlikely that they were instances of either commodified pornography or high art, as produced in contemporary contexts.

EVIDENTIAL CONSTRAINTS IN PRACTICE

In all the cases discussed above, both critical and constructive results turn on the appraisal of evidential constraints. And in all cases, the evidence appraised plays a role that is to varying degrees autonomous and corrective of the expectations and presuppositions that laden it, that bring it into view or give it specific evidential import. Considerations operating on a number of dimensions have this capacity to constrain, as Kosso (1988) argues. Nevertheless, these multiple factors generate cases that fall along a rough continuum defined chiefly by degrees of independence in the first sense (the independence of linking hypotheses from claims or presuppositions about the subject past they help establish) and by the nature of the linkage invoked (the degree to which it is uniquely determining, establishing security in the second sense).

At one end of the continuum, the end that draws the attention of antiobjectivist critics, ascriptions of evidential significance are entirely determined by theoretical commitments, a set of precepts about the nature of the cultural subject, that are also embodied in the broader interpretive and explanatory claims that this evidence will be used to test or support. This predetermination is, in part, what Watson and Kennedy object to in explanations for the emergence of horticulture in the Eastern Woodlands. Sexist assumptions about the nature and capabilities of women underlie standard models of the horticultural transition (consistently reading women out of the account) and they infuse interpretations of the archaeological data used to evaluate these models, ensuring that these data will be seen as evidence for models that project onto the past a natural sexual division of labor in which women are consistently passive and associated with plants.

But even in these worst cases it is often possible, as Watson and Kennedy demonstrate, to establish grounds for questioning the assumptions

that frame both the favored hypotheses and the constitution of data as the evidence from which these hypotheses derive support. Two strategies for critique are evident in their analysis. The first is to exploit nonarchaeological resources, both conceptual and empirical, in an independent assessment of the framing assumptions. In this connection, Watson and Kennedy draw attention to a straightforward contradiction inherent in current theorizing about the emergence of horticulture in the Eastern Woodlands: women are persistently identified as the tenders of plants, whether wild or under cultivation, and yet are systematically denied any role in the transition from foraging to horticulture, whatever the cost of that denial in terms of theoretical elegance, plausibility, or explanatory power. To indicate just how high the cost may be, they draw on background (botanical) knowledge about the range and environmental requirements of the plant varieties that became domesticates to establish that they routinely appear in prehistoric contexts that were far from optimal (P. Watson and Kennedy 1991: 266). Watson and Kennedy argue that it is most implausible that these domesticates could have arisen under conditions of neglect, as suggested in the co-evolution model. Regarding the shaman hypothesis, their analysis is informed by an appreciation that since the 1970s, feminist anthropologists have documented enormous variability in the roles played by women and in the degrees to which women are active rather than passive, mobile rather than bound to a home base, and politically powerful rather than stereotypically dispossessed and victimized. This undermines any presupposition that women are inherently less capable of innovation, self-determination, and strategic manipulation of resources than their male counterparts and renders suspect any interpretation that depends on such an assumption, regardless of its archaeological implications. In this way Watson and Kennedy challenge the credibility of the interpretive principles used to bring archaeological evidence to bear on questions about the transition to horticulture, questioning the more fundamental framework assumptions that underlie the explanatory models of this transition that they find inadequate.

But in addition, even when circularity threatens—when linking principles are drawn from the same theory as underpins the test hypothesis—archaeological data can sometimes function as a locus of evidential constraint, thereby making possible a second strategy for critique. The predisposition to interpret archaeological data in terms of sexist assumptions about the nature and capabilities of women does not necessarily ensure (indeed, as Watson and Kennedy point out, it has not ensured) that the record will obligingly provide evidence that activities that are assumed to be male-associated will prove to have mediated the transition from a foraging to a horticultural way of life, however strong the expectation that they must have done so. In fact, most of the activities that the co-evolution model deems responsible for the creation of the "domestilocalities" in which cultigens emerged were women's activities, if the archaeological record of such sites is interpreted in light of the traditional assumptions about gender relations that Watson and Kennedy find presupposed by this account (1991: 262). If the interpretive assumptions in question constituted a more closely specified theory, the outlines of Glymour's bootstrapping inference might emerge in cases like these, complete with internal-to-theory independence between linking and test hypotheses (Glymour 1980; see chapter 13 above).

Straightforward circularity is generally not the central problem in archaeological interpretation, however. Given the state of knowledge in the relevant fields and the complexity of most archaeological subjects, it is almost unimaginable that a single encompassing theory could provide both the linking principles necessary to interpret archaeological data as evidence of the cultural past and a suite of hypotheses capable of explaining the events and conditions that this evidence brings into view. Usually the basis for ascribing evidential significance to archaeological data is some form of analogical inference that draws on diverse sources, most of which are understood in terms of highly localized theory. Here the worry is not overdetermination by an all-encompassing conceptual framework, but underdetermination due to a lack of generalizable knowledge about the conditions under which observed linkages between (archaeological) statics and (cultural, behavioral) dynamics may be projected onto past (or otherwise unobserved) contexts. The inferential distance that must be crossed, in all of Longino and Doell's senses (1983), remains considerable, and there are relatively sparse resources for helping to bridge it. As has been widely argued by both critics and ad-

vocates of analogical inference, there is a pressing need to strengthen the grounds for supposing that surviving traces are linked to antecedents in the same manner as observed in better-known contexts, as well as for eliminating alternatives when alternative linkages are known to be possible. That is, there is a need to establish the security of the inferences from present to past in both of the senses identified above.

Analogical inference is typically constructed and evaluated in terms two sets of evidential constraints that establish security in just these senses when effectively deployed (as outlined in chapter 9). These are constraints on what can be claimed about the analog, given background knowledge of the source contexts from which they are drawn, and constraints on the applicability of the analog to a specific subject context that derive from the archaeological record. For example, in associating women with the use of spindle whorls in weaving and with the use of griddles in food preparation, Brumfiel relies on a direct historic analogy, arguing that these artifacts are so extensively and stably associated with weaving/cooking and women in historically related ethnographic and ethnohistoric contexts that it is reasonable to assume that these associations held for prehistoric contexts as well. Similarly, archaeologists dealing with evidence of horticultural practice routinely postulate a division of labor in which women are assumed to have had primary responsibility for agricultural activities (Ehrenberg 1989: 77–141), but they base this assumption not on an appeal to the completeness of mapping between source and subject (which Brumfiel's case illustrates) but on the persistence of the association of women with horticulture across historically and ethnographically documented contexts, however different they may be in other respects.

In these cases, completeness of mapping and reliable correlation figure as evidence that a common determining structure[14] links a distinct type of artifactual material to specific functions, gender associations, or activity structures securely enough in present contexts to support an ascription of the same functions and associations to the archaeological subject. These interpretive claims can be as decisively undermined by a change in background knowledge about the sources of these analogs (e.g., evidence that the material : behav-

ioral linkage projected onto the past is not stable in source contexts) as by what archaeologists find in the record of the contexts onto which they are projected (e.g., evidence that a particular association or function could not have obtained in the context in question).[15] Conversely, where the linking principles based on background knowledge of source (or actualistic) contexts is uncontested and their credibility is independent of any of the hypotheses archaeologists want to evaluate against the evidence these principles help to establish, they can very effectively stabilize debate.

The power of the challenge posed by Brumfiel to extant models of the economic base of the Aztec empire depends on precisely this sort of stabilizing analogy. The association she posits between women and spindle whorls, pots, and griddles is not questioned by those she engages in debate and is independent of both the hypotheses she challenges and those she promotes. Given this provisional foundation, she brings into view new features of the structure of otherwise well-understood assemblages—formerly undocumented patterns of distribution and association among components of these assemblages—that standard models cannot account for, even when constituted and interpreted in terms that are shared by proponents of these models. She thus challenges not (just) the conceptual integrity or prior plausibility of conventional models (as Watson and Kennedy had done) but their empirical and explanatory adequacy as an account of the political economy of states that rose in pre-Columbian Mesoamerica and South America. Perhaps most important, Brumfiel identifies implausible assumptions about the stability of gender structures as the source of the inadequacies of these models; she argues for an alternative predicated on the thesis that gender relations and household divisions of labor are not only dynamic—genuinely historical and cultural, not natural—but are also crucial codeterminants of political and economic processes of state formation that had been treated as a public, male preserve. Her account is compelling inasmuch as she effectively fills some of the gaps and solves some of the puzzles that arise for extant theories because of their dependence on these assumptions.[16]

The limiting case on this continuum of theory-ladening inferences, the ideal of security in the

ascription of evidential significance to data described earlier, arises when archaeologists can draw on completely independent, nonethnographic sources that specify unique causal antecedents for components of the surviving record. Among the cases considered here, Hastorf's analysis of bone composition comes the closest to this ideal. If the background knowledge deployed in stable-isotope analysis is reliable (a question always open to critical reassessment), then it can establish, in chemical terms, what dietary intake would have been necessary to produce the reported composition of the bone marrow recovered from archaeological contexts. Where its results can be linked, through palaeobotanical analysis, to the consumption of specific plant and animal resources and through skeletal analysis to a pattern of sex-linked differences in consumption, isotope analysis can underwrite the inference of dietary profiles that is substantially independent and can provide a genuine test of interpretive or explanatory presuppositions about subsistence patterns or gender-structured social practices affecting the distribution of food. The independence and security of linking arguments based on background knowledge of this physical, chemical, bioecological sort are exploited in many other areas of gender research: in morphological analyses of skeletal remains that provide evidence of pathologies, physical stress, and fertility (Bentley 1996) and in materials analysis and reconstructions of prehistoric technology (e.g., in connection with ceramic production, Wright 1991; and architecture, Tringham 1991[17]), to name a few such examples. As this limiting ideal of (vertical) independence between linking principles and test hypotheses or framework assumptions is approximated, archaeologists secure a body of evidence that establishes provisionally stable parameters for all other interpretation and a stable (if never uncontestable) basis for piecemeal comparison between contending claims about the cultural past.

It is important to note, however, that the evidence provided by these sorts of linking principles has limited significance, taken on its own. Hastorf must rely on a number of collateral lines of evidence to establish that the anomalous shift in diet evident in male skeletons was due to increased consumption of maize beer, and to link the change in consumption to the advent of Inka-imposed systems of political control in the region and to a restructuring of gender relations at the level of the household. This reliance on multiple lines of evidence is an important feature of archaeological reasoning that cuts across the considerations of (vertical) independence and of security I have described. In fact, evidential significance is rarely ascribed to items taken in isolation. Context is crucial and is defined in a number of ways; if it is relevantly cultural, rather than geological or ecological, it may be characterized by associations among artifacts or features that are recovered together in undisturbed deposits, that have close spatial or temporal proximity, or that show technological, formal, or stylistic affinity even if widely dispersed.

When elements of the archaeological record can be assumed to bear on a particular past context in one of these senses and, most important, when these elements are ascribed significance on the basis of diverse linking principles (i.e., principles derived from independent bodies of background knowledge), then a network of horizontal constraints may come into play *between* distinct (vertical) lines of interpretive inference that can vastly increase their individual and collective credibility. Each vertical linkage between data, evidence, and hypothesis may be compelling individually—each may be secure in the relevant senses, and independence between linking principles and test hypotheses may ensure against vicious circularity—but if the linking principles determining evidential significance are independent of one another in the second sense, it becomes possible to triangulate on a postulated set of conditions or events. And if diverse evidential strands all converge on a given hypothesis about the past, they can provide that hypothesis compelling support, to the degree that it is implausible that such convergence could be the result of compensatory error in all the lines of inference establishing its evidential support (Kosso 1988: 456; Hacking 1983: 183–185). Most often the problem in archaeology is not to adjudicate between a number of equally plausible, well-supported, explanatory alternatives but to find one account, one reconstructive or explanatory hypothesis, that is consistent with all the lines of evidence that are constructed using diverse resources.

While Hastorf most explicitly exploits the con-

straints imposed by a requirement of convergence across horizontally independent lines of evidence, it is clear that Brumfiel relies on horizontal independence as well. When she identifies an anomalous distribution of artifacts related to cloth production over time and space, and then reassesses the evidence related to different sorts of food processing, the new (unexpected) convergence she documents provides her own account with especially strong support precisely because nothing in the linking principles ensures such convergence; the evidence could have turned out otherwise. More significant still are cases in which independently constituted lines of interpretation fail to converge. Even when each line of evidence relevant to a particular account of the past enjoys strong collateral support taken on its own (i.e., each is secure), undetected error may become evident when one line of evidence persistently runs counter to the others, when dissonance emerges among lines of interpretation. The failure to converge on a coherent account clearly indicates an error somewhere in the system of background knowledge—the auxiliary assumptions and linking principles—however well-entrenched they may be.

In cases of extreme dissonance, which are approximated by the interpretations of artistic images and traditions considered by Handsman (1991) and by Conkey (with Williams 1991), a persistent failure to converge may call into question the efficacy of *any* interpretive constitution of the data as evidence in a particular area.[18] These authors conclude that many familiar and influential interpretive options must be abandoned, given the lack of convergence between the interpretive claims based on material identified as art and reconstructions based on other forms of evidence that bring into view the larger cultural contexts in which the artistic tradition occurs. Indeed, all indications are that the prehistoric cultures they consider must have been so profoundly different from any with which we are familiar that the images constituting their artistic record cannot be assumed to have any transculturally stable meaning; they cannot be taken as evidence of many, or indeed any, of the range of activities, beliefs, or sensibilities that we associate with art. Such discontinuity may suggest that there is no determinate fact of the matter where the symbolic import of gender imagery is concerned; or, as Conkey

suggests, it may require us to acknowledge that in such cases we simply are not and may never be in a position to determine what the fact of the matter is. But even in these most enigmatic cases the data often do effectively resist the imposition of favored interpretations, thereby undermining a number of formerly plausible claims about the past. Thus, dissonance among lines of interpretation may make clear what we *cannot* claim in connection with a particular past; it may force a reconsideration of fundamental assumptions about the nature of the subject domain—about art and artistic production—and about the limits or prospects for success in investigating it. Paradoxically, the fragmentary nature of the archaeological record is at the same time its strength in setting up such evidential constraints, even in establishing the limits of inquiry.

The explicitly feminist initiatives that have emerged in archaeology make clear the centrality of values, interests, and sociopolitical standpoint to archaeological practice, and for this they are sometime decried as "just political" (Wylie 1990). At the same time, however, they illustrate how a range of empirical and conceptual resources can be used to critically evaluate not only conventional interpretations of the cultural past but also the assumptions that inform them, assumptions that are sometimes so deeply entrenched in our thinking as to be invisible. The strategies that feminists use to mobilize these resources are common in archaeological practice. When successful, they sometimes put us in a position to say we have discovered a fact about the world, or have shown a formerly plausible claim to be simply false; the critical analysis by Watson and Kennedy and the constructive proposals of Hastorf and of Brumfiel are examples in point. In other cases the outcomes of inquiry are more equivocal. As Handsman and as Conkey (with Williams) illustrate, sustained investigation may call into question basic assumptions about the accessibility, or even the existence, of certain facts about a given subject domain. In short, some objects of knowledge and epistemic situations do sustain a moderate objectivist and realist stance, while others do not; they are textlike in their interpretive openness, and it may never be appropriate to claim evidential security for descriptive or explanatory claims about them.

The conclusion I draw is that we should resist the pressure to adopt a general epistemic stance as appropriate to all evidential claims and all reconstructive or explanatory claims warranted by a particular disciplines. Any question about the status of evidence and the relationship between evidential and sociopolitical interests in the construction of knowledge—whether we should be relativists or objectivists—must be settled locally, in light of what we come to know about the nature of specific subject matters and about the resources we have for their investigation.

Rethinking Unity as a "Working Hypothesis" for Philosophy of Science

How Archaeologists Exploit the Disunities of Science

As compelling as they once were, and as influential as they continue to be in many contexts of practice, theses of the global unity of science have been decisively challenged in all their standard formulations: methodological, epistemic, and metaphysical. It cannot be assumed as a normative ideal or even as a "working hypothesis" (Oppenheim and Putnam 1958) that the sciences presuppose an orderly world, that they are united by the goal of systematically describing and explaining this order, and that they rely on a distinctively scientific method that, successfully applied, produces domain-specific results that converge on a single coherent and comprehensive system of knowledge.

A question immediately arises: What follows from these arguments against unity, given that they represent not just the culmination of critical debate about a particularly influential view of science, but a challenge to assumptions that have very largely defined what it is to do philosophy of science? In this chapter I consider the implications of disunity at two levels. I am concerned, first (and primarily), to delineate the scope of arguments against global unity theses. However much the weight of critical argument tells against old-style global unity theses, it is important not to lose sight of the fact that ideals of epistemic and methodological unity remain a powerful force in many sciences (Morrison 1995; Wayne 1996),[1] and that

local and contingent unifying strategies are crucial to most scientific inquiry. I argue, with reference to the practice of historical archaeologists, that the interfield and intertheory connections necessary to support evidential claims represent a significant if perplexing unifying force, even if they do not support global unity theses. They establish a robust network of cross-connections that binds the sciences together. At the same time, however, the epistemic leverage they provide depends on significant and pervasive disunity in the sciences. In the final section, I briefly consider some meta-implications of this argument for philosophy of science and for science studies more generally.

THE UNITY OF SCIENCE AS A WORKING HYPOTHESIS

METHODOLOGICAL UNITY THESES

Although claims of methodological unity were the cornerstone of expansionist programs in philosophy and in science in the nineteenth and the early twentieth century, they received only cursory attention from such powerful advocates for unified theories of science as Oppenheim and Putnam, who by 1958 were declaring that this genre of unity thesis "appear[ed] doubtful" (1958: 5).[2] Indeed, such great nineteenth-century systematiz-

ers as Mill and Whewell took considerable care to catalogue the diversity of methods developed by (successful) sciences and were divided in their assessment of whether or how these could be characterized in unitary terms.[3] Recent reexaminations show that even such a stalwart of the Vienna Circle as Otto Neurath was more interested in the coordination of scientific methods than in methodological unity per se (Cat, Cartwright, and Chang 1996; Cartwright and Cat 1996). In short, both classical and logical positivists were equivocal in their endorsement of methodological unity theses. As Hacking remarks of unity theses of all kinds, two quite distinct senses of unity are at issue: unity qua "singleness" and unity in a looser, contingent sense that he describes as "harmonious integration" (1996: 41).

Twenty years after the appearance of Oppenheim and Putnam's declaration of support for unity theses, Suppes (1984 [1978]) reinforced their caution about the methodological variants of these theses. He declared claims about methodological unity unsustainable in any interesting form; if formulated in terms general enough to cover all scientific practice, they are likely to be trivial and to obscure more than they illuminate of the real complexity of scientific practice. They are, moreover, irrelevant; it might have been important to articulate a clear-cut definition of what counts as scientific method when science itself was in need of a philosophical defense, but by the late 1970s, Suppes argued, that was no longer necessary. It was time to turn our attention to "a patient examination of the many ways in which different sciences differ in language, subject matter, and method, as well as [to] synoptic views of the ways in which they are alike" (1984 [1978]: 125). Of recent disunity theorists, Dupré is most uncompromising in pressing this point; he argues that the quest for "general criteria of scientificity" (1993: 229) is largely irrelevant to current unity debates. Where methods are concerned, "science is [at best] a family resemblance concept" (1993: 242; see also 1995); and where judgments of scientific credibility are at issue, the most promising and realistic strategy is to apply the standards of a flexible virtue epistemology on a case-by-case basis.

Debate on these issues is by no means closed. Certainly, some advocate more closely delimited methodological unity theses, although they usually focus on particular features of scientific inquiry (e.g., models of explanation, confirmation, testing, and belief revision), and some argue strenuously against disunity critics on the grounds that if they are right, meaningful distinctions between science and pseudo-science are irrevocably compromised and corrosive relativism is unavoidable (see Stump 1991 on challenges from John Worral and Harvey Siegl). More modestly, Ereshefsky argues that despite his disunifying ambitions, Dupré's catalogue of epistemic virtues captures a "fairly stable core" of nontrivial but global features of scientific methodology (Ereshefsky 1995: 156).[4] In the end, however, what emerges is a decisive rout of theses of methodological unity that are global in scope and that posit the "singleness" of scientific method (to use Hacking's term). If they are characterized with any specificity, methodological strategies and standards do seem to be highly variable across the sciences, and they clearly evolve: they are responsive to the empirical conditions of practice (to subject domain) and to the interests of investigators. This appreciation of the complexity and diversity of scientific practice is reinforced as philosophers of science naturalize their practice and attend to the specifics of practice in an increasingly wide range of fields.

EPISTEMIC AND ONTOLOGICAL UNITY THESES

The unity theses that Oppenheim and Putnam endorse have to do with the content of science and its subject domain(s) rather than its methodology; though I refer to these as epistemic and ontological unity theses, they did not.[5] At their most ambitious, the advocates of these theses postulate a hierarchy of microreductions that integrate all the sciences into one coherent system; the language and, more to the point, the laws and theories—in short, the content—of each science should (ultimately) derive from, or supervene on, those of successively more basic sciences until finally reaching a "unique lowest level," a foundational science of elementary particles (Oppenheim and Putnam 1958: 9). In this they assume that an orderly and unitary structure of part:whole relations holds between the objects studied by sciences at each level; reduction is accomplished if it can be shown that a science at one level involves the study of objects that can be "decompos[ed] into things belonging to the next lowest level" (9). Oppenheim and Put-

nam unambiguously state that in the late 1950s actual science displayed no such ideal unity, although they note a number of "unifying trends" that warrant systematic investigation; ultimately, they insist, the "unity of science" is a hypothesis that "can only be justified on empirical grounds" (12). In this tentative form it has served as the central "organizing principle" for a great deal of philosophical work on science in the last forty years.

In practice the interest in global epistemic reduction has fragmented into localized debates about the likelihood that microreductions will be realized between pairs of sciences: physicalist or materialist reductions of psychology to neuroscience, biochemical reductions of genetics, the "quantum takeover" in physics that has been contested by Cartwright (1995). And in virtually all such cases, the prospects for reduction remain at least contentious and certainly distant. Even paradigmatic examples of apparently successful unification prove unexpectedly complex, providing at best equivocal support for epistemic and ontological unity theses. For example, Morrison (1992) argues that the unification effected by Maxwell's electromagnetic theory, and more recently by electroweak theory, is "structural rather than substantial" (1995: 369); unity is accomplished at a theoretical level by extending a powerful mathematical formalism to diverse phenomena, but key elements of the constituent theories are left either uninterpreted or unreduced and little ground is provided for claiming that any deeper (ontological) unity in nature has been discovered (1995: 372).[6] In Morrison's view, the conjoined theories do not establish a part:whole relation between the forces or entities they posit; in the case of Maxwell's theory, physical interpretation of the unifying mathematical model remained a fundamental difficulty, while electromagnetic and weak forces remain distinct in electroweak theory. Here theoretical unity coexists with ontological disunity (371); indeed, in the case of electroweak theory, Morrison argues that "unity is achieved at the price of introducing an element of disunity" (369).

In a recent discussion of the "special" sciences (specifically economics), Kincaid makes the complementary argument that even if we accept some form of metaphysical unity thesis it does not follow that we ("real human agents") can or should make epistemic unity our central objective. The entities and events studied by social scientists may all be dependent on or indeed constituted by their physical realization, but it does not follow that physical theories at the "lower level" should be granted explanatory primacy (Kincaid 1997: 3); higher-level theories in the special sciences may well describe the causal dynamics of sociopolitical, cultural, or economic systems that cannot be strictly derived from physical theories. Insofar as the special sciences prove capable of establishing interesting (counterfactual-supporting) generalizations—and such capability must be considered an open, empirical question—it seems unlikely that these will map onto physical descriptions of the objects and events they systematize. As Fodor put it in 1974, "what is interesting about monetary exchanges [for example] is surely not their commonalities under *physical* description" (1974: 103–104; emphasis in the original).[7]

Dupré extends this line of argument, noting disjunctions between the theories produced not only by distinct branches of science but within them as well (a distinction made by Davies 1996). Biologists actively debate divergent classificatory schemas, all of which may be said to cut nature at its joints but reflect different selections of joints. These are distinguished not by concern with different levels of reality that fit neatly together when parts are reassembled into wholes, but by an interest, pragmatic or scholarly, in different aspects of a complex reality and its diverse causes: "Evolution, the source of biological diversity, is itself a diverse set of processes. There is no reason to expect that it will give rise to any unique and privileged set of categories suited to the varied sorts of inquiries and interests that we bring to the study of biological organisms" (Dupré 1996b: 443). It is no accident, Dupré concludes, that epistemic disunity seems to be the rule, rather than the exception. Reduction projects founder on the diversity and disorder of nature, which poses a fundamental (empirical) challenge to the metaphysical as well as the epistemic components of Oppenheim and Putnam's "working hypothesis."

One sympathetic critic objects that if Dupré is seriously committed to pluralism, he must allow that essentialist categories may yet prove viable in some areas (Ereshefsky 1995); another who is less sympathetic argues that Dupré puts too much weight on the state of disarray in which he finds contemporary biology: perhaps "our epistemic situation now is most like that within late sixteenth-

century astronomy where much integrative conceptual and empirical work lay in the future and the endorsement of a pluralistic realism would have been at best premature" (R. Wilson 1996: 312). In response, Dupré reasserts a point acknowledged at the outset by Oppenheim and Putnam and made repeatedly by disunifiers. The question of whether unity theses are viable is empirical and certainly remains open; it is, indeed, "hazardous to read a philosophical position off the current state of science" (Dupré 1996b: 441).[8] Certainly no one can claim to offer arguments that decisively settle the case for or against unity theses, given that these are prospective and to some degree normative, as well as empirical. Nevertheless, at this juncture the weight of evidence and argument counts strongly against any form of global "singleness" theses. If anything, methodological and theoretical disunity seems to proliferate rather than diminish as the sciences mature and specialize. And, as often as not, this growth leads to a recognition of greater complexity rather than of simplicity in the ontology of the subject domains that scientists investigate. The more we learn about the specifics of scientific inquiry, the more tenuous seems the rationale for taking any form of global unity thesis as the point of departure for philosophical analysis. The real challenge is to determine to what extent disunity prevails, in what different forms, and for what reasons.

INTEGRATION AND UNIFICATION

By no means does the above brief for taking disunity seriously displace all questions about unity in more contingent and localized senses. As unifiers and disunifiers alike acknowledge, unifying connections within and between the sciences are a crucial feature of much research practice. Fine-grained studies of inter- and intrafield relations bring into focus a complex network of interdependencies—counterparts to the methodological, epistemic, and metaphysical unity postulated by traditional unity theses—that do not fit reductionist models but nonetheless bind the sciences together "by much more subtle routes" (Kincaid 1997: 6).

Perhaps the most tangible evidence of such cross-field connections is in the examples of "interfield theories" analyzed in the late 1970s by Darden and Maull (1977), in the emergence of "cross-disciplinary research clusters" described by Bechtel (1988) and by Abrahamsen (1987), and in the expanded range of interfield problem-solving strategies that have subsequently been identified by Darden (1991) and by Galison (1996). In the cases considered by these analysts, questions arise concerning aspects of the subject domain studied by one field that can be addressed adequately only by engaging the resources of another. The interaction generated between fields often results not in a reductive assimilation of one field (or theory) to the other, or in a simple borrowing of information, technology, or explanatory models that leaves each essentially unchanged, but in the formation of substantially new theories and research programs concerned with relations between phenomena that cut across the traditional domains of neighboring fields (Darden and Maull 1977: 50). The cases that pose the most telling challenge to traditional unity theses are those, originally described by Darden and Maull, in which interacting fields are linked in just the ways that should support microreduction—their subject domains stand in a part:whole relation to one another—but what emerges is a semiautonomous theory. Typically these emergent theories concern aspects of the entities studied by one field that have not been its focal concern but are relevant for understanding the wholes studied by another field. In addition, Darden and Maull consider interfield theories that are formed to account for a range of other structural:functional and causal relations between phenomena at the same level of organization that are studied by distinct fields.[9] Bechtel and Abrahamsen expanded on this account of interfield relations by considering instances of horizontal integration that result when a number of fields concerned with overlapping problems form loosely coordinated "disciplinary research clusters" (Bechtel 1988: 110; Abrahamsen 1987). In some cases these clusters bring together practitioners who study the interactions between distinct phenomena studied by different fields; in others they concern what are recognizably the same phenomena studied from different field-specific perspectives. But despite the assumption of some form of local ontological unity, what emerges are conjoint bodies of theory and research practice that are integrated to varying degrees but fall well short of content reduction.

The technology-induced emergence of trading

zones recently discussed by Galison (1996) is a somewhat different but related interfield formation that arises from methodological rather than theoretical integration. Here the technology of computer simulation establishes unifying connections between fields, connections embodied in "strategies of practice" that depend on no presumption of ontological unity (however local) and yield no substantial theoretical integration, much less theory reduction (Galison 1996: 157). But they do represent "a new cluster of skills . . . a new mode of producing scientific knowledge that was rich enough to coordinate highly diverse subject matters" (119). As the pioneers and advocates of these computer applications refined their technical practice, they found themselves marginalized in their home fields and increasingly drawn into a delocalized trading zone (155); they developed a language and a style of inquiry that took on a life of its own.[10] It was this creole that gave rise to a reconceptualization of the subject domains of contiguous fields. Computer simulation technologies may have been introduced as a tool that could help diverse fields solve internally defined problems, but "bit by bit (byte by byte) . . . the computer came to stand . . . for nature itself" (157). There emerged a body of practice that, like interfield theories, is not strictly the product of any one existing field; to varying degrees and in different ways it transformed and integrated the research of distinct disciplines but did not generate an autonomous new field or reduce any one existing field to another.[11]

When Darden and Maull first described interfield theories they were concerned that these had been ignored because the mandate set by Oppenheim and Putnam's working hypothesis focused philosophical attention on just one kind of interfield relationship, that of derivational microreductions. They proposed a new working hypothesis, one that conceptualizes unity in science as "a complex network of relationships between fields effected by interfield theories" (Darden and Maull 1977: 60). Even this remains too restrictive, however.[12] Bechtel's and Abrahamsen's research clusters represent a looser interfield coordination of theory, and Galison's trading zones quite another (primarily methodological) interfield formation. In addition, there are innumerable other more mundane and "work-a-day" connections (Abrahamsen 1987: 356; see also Darden 1991); these sustain durable networks of relationships between

fields but do not supplement the donor or recipient fields substantially enough to warrant the formation of an interfield theory or cross-disciplinary research cluster, and they are not contentious enough to generate a semiautonomous trading zone. They are exchanges that proceed relatively quietly, establishing themselves as a stable and ubiquitous form of discipline bridging (Abrahamsen 1987: 356).

At the relatively broad and transformational end of this spectrum of interactions, one field may appropriate the orienting theory or domain-defining metaphors—and sometimes with them the problematic—of another field, but remain a theoretically and methodologically (as well as institutionally) autonomous endeavor. Psycholinguistics is an example, considered in some detail by Abrahamsen (1987), in which the balance between influence and assimilation is renegotiated on an ongoing basis. The diffusion of structuralist approaches through the social sciences, described in another connection by Pettit (1975), is a case in which a linguistic metaphor and, selectively, some aspects of linguistic theory and linguistic methods of analysis were extended to a wide range of fields dealing with cultural subjects that could reasonably be conceived as meaning bearing in various senses (see chapter 8 for further discussion of structuralist analysis in archaeology). Archaeology is a field whose recent history has been shaped by a succession of experiments with different metaphorical and theoretical constructions of its cultural-material subject domain: a reductive eco-materialism that privileges the environmental determinants of cultural behavior; various forms of historical materialism, structuralism, and poststructuralism; and, recently, a family of evolutionist approaches on which cultural phenomena are conceived as part of the extended human phenotype, to be explained in terms of selection pressures. In addition, however, borrowings of more limited scope are essential even in fields with less permeable boundaries, whose subject domains and problematics are distinctly their own. These borrowings include the transfer of explanatory models, empirical results, and research technologies (skills and instruments) from one field to another, where they are used to develop field-specific explanatory theories and to establish the evidential basis necessary for evaluating these theories.[13]

Dupré considers these to be elements of the "densely connected network" that binds various sciences together, but dismisses them as irrelevant to the debate about unity; they merely establish that "no form of knowledge production can be entirely isolated from all the others" and this, he says, is "too banal an observation to glorify with the title 'unity of science'" (1993: 227). He is certainly right that such interfield connections provide little support for the kind of global, "singleness" unity thesis he contests.[14] But if Dupré's critique of these theses is taken as a point of departure, such "banal" interactions are crucial for understanding the relationships that productively integrate and coordinate the actual practice of science. These low-level, unexceptional connections often involve just the kind of paradoxical juxtaposition, even interdependence, of unity and disunity on which Morrison (1995: 369) remarks; they preserve disunities in many areas while at the same time building localized bridges, trading zones, and points of integration between fields. In the case I consider below, historical archaeologists make use of integrative connections between fields to establish an evidential basis for building and testing claims about the past, but the epistemic advantage this affords depends on their ability to systematically exploit the disunities that persist on many levels among scientific fields and theories.

LOCALIZED UNITY: HISTORICAL ARCHAEOLOGY

CONJOINT USES OF EVIDENCE

Historical archaeology emerged as a distinct field only in the last third of the twentieth century.[15] In North American contexts its proponents have struggled vociferously to establish its credibility and define its identity in opposition to two powerful parent disciplines: (real) archaeology and (real) history. Prehistoric archaeologists have been inclined to treat historical archaeology as shallow, literally and figuratively, and historians dismiss it as a hopelessly thin source of insight about the past. Historical archaeologists, for their part, insist that much damage has been done by arrogant prehistorians who, enlisted by contract firms, government agencies, and university field schools, assume that historic sites pose no interesting challenges of their own, as well as by insular historians who insist that there is nothing to be learned from kitchen middens and cellar pits that cannot be better learned from the documentary record. It is striking, however, that as intent as historical archaeologists have been on defining the boundaries of their new field, they consistently emphasize the need for, and value of, substantial interfield connections. A recurrent theme in these debates is an insistence that when events and conditions of life of historic periods are at issue, vastly more can be achieved by making conjoint use of the evidential, methodological, and theoretical resources of archaeology and documentary history than can be achieved by either field working in isolation from the other.[16]

The argument here is not just that archaeological inquiry provides supplementary detail about the past, useful for animating museum displays but of only marginal relevance to the bigger picture historians construct on the basis of documentary research. In resisting the imperialism of history, historical archaeologists sometimes insist that they offer substantially different, potentially transformative insights about the recent past. The gritty details of the archaeological record bear witness to "the inarticulate" (Ascher 1974: 11), the "endless silent majority who did not leave us written projections of their minds" (Glassie 1977: 29), whose dispossession extended well beyond the alienation of their labor to the production of what Glassie describes as "superficial and elitist . . . tale[s] of viciousness"—"myth[s] for the contemporary power structure" (1977: 29). Historical archaeology promises not just to fill in missing information about those who are largely invisible in the narratives of text-based history, but to counter the "inevitable elitism" (29) of traditional history. While this assertion sells short the insights afforded by radical history (e.g., "history from below"; Sharpe 1991) and ignores the conservatism of much historical archaeology, it does draw attention to the transformative potential of the field, a potential that has been realized in a number of areas in which historical archaeologists have been active since the 1970s.[17]

Sometimes these claims about the corrective powers of historical archaeology are generalized in epistemologically interesting ways. The discipline-bridging position of the new field is represented as a resource rather than a liability, on the

grounds that the credibility of claims about the historical past is substantially improved if they are supported by both documentary and historical evidence. Here historical archaeologists appropriate and extend strategies of response that mediate between the more extreme positions defined in the long-running debate in North American archaeology about the status of archaeological evidence described in earlier chapters. They resist, on the one hand, the constructivism of uncompromising antipositivists who insist that if all evidential claims are theory-laden, then any appeal to archaeological evidence is viciously circular. And on the other, they seem disinclined to follow the lead of unreconstructed positivists who define their subject in terms compatible with the conviction that certain ranges of auxiliaries, usually those established by the most successful of the physical sciences, can secure a surrogate foundation of stable (if not given) evidence.[18] As many others have done, historical archaeologists do their best to assess the security of the sources on which they rely to address the questions they find significant (not just tractable).[19]

In this mediating spirit, many archaeologists exploit the fact that strong constructivist arguments presuppose a degree of unity in science that simply does not exist. Circularity is an inescapable problem only if one assumes a seamless integration of all the various fields and theories on which archaeologists rely when constructing models of the past and when interpreting their data as evidence for or against these models.[20] In practice, when archaeologists exploit the dimensions of epistemic independence described in previous chapters (chapters 12, 13, and 14), they make good use of disunities that (contingently) ensure a disjunction between the background assumptions drawn from different sources and the hypotheses they test against evidence interpreted in light of these assumptions. Consider, for example, the possibility that an archaeologist might use radiocarbon dating and various types of materials analysis to test the plausibility of a hypothesis about trade connections, perhaps a hypothesis inspired by structuralist analysis of the grammar of design traditions evident in the burial goods, elite ceramics, and architecture of two distant and otherwise distinct prehistoric communities.[21] The test in question would be designed to establish whether the material thought to have been traded into one

context could have originated in the other (e.g., whether it is contemporaneous and whether it is made of materials or by means of technologies possessed by the source culture). In such a case, there is sufficient disjunction between, on the one hand, the linguistics and sociocultural anthropology from which assumptions framing the test hypothesis are drawn and, on the other hand, the chemistry and physics necessary to establish the source and dates of the archaeological material that the resulting evidence could not be expected to converge in support of the structuralist hypothesis about trade relations linking the cultural traditions; its convergence is not plausibly an artifact of the interpretive principles used to bring archaeological data to bear on the test hypothesis.[22]

In this spirit, Leone and Potter argue that if we "abandon the conceit that the documentary record was created for us," and the underlying premise that interpretation of the archaeological record is dependent on the documentary record, it becomes possible to exploit these records as "two *independent* sources of evidence" (1988: 14; emphasis in the original). A process of "analytical byplay" between documentary and archaeological data, of working "back and forth, from one to the other," suggests that each can be used "to extend the meaning of the other" (14). The crucial methodological corollary is that if two sources are indeed independent, then a failure to converge can be counted on to expose weakness in the constituent chains of reasoning that may not be evident when the security of each is considered on its own; each line of evidence can be used as a check on the other.

CAUSAL, INFERENTIAL, AND DISCIPLINARY INDEPENDENCE

Although I am sympathetic to these claims on behalf of historical archaeology, they conflate several different senses of independence between lines of evidence, not all of which are epistemically relevant or coincident with the disciplinary boundaries between history and archaeology. There are at least three kinds of (horizontal) independence at issue here: causal, inferential, and disciplinary independence. I disentangle them initially with reference to the examples of microscope development and use that Hacking considers (1983: 186–209), and that I discussed in chapter 12 (see also

Wylie 1999b); the parallels with archaeological practice are instructive, particularly if that practice is conceptualized as a matter of "indirect observation" of the cultural past (e.g., J. Fritz 1972). On Hacking's account, the makers and users of microscopes exploit a number of different physical (causal) processes of interaction (or signal production) between the target and the receiving instrument. The great value of the proliferation of microscopes that exploit these different causal processes (e.g., acoustic as opposed to optical microscopes) is that they allow for a triangulation of signals, correcting and enhancing the information any one microscope could provide about the entities they enable us to observe: "we believe what we see [through microscopes] largely because quite different physical systems provide the same picture" (Hacking 1983: xiii). Triangulation depends on crucial unities between fields and domains, but the judgment that some lines of evidence are (horizontally) independent in an epistemically relevant sense depends on causal and theoretical disunities.

The first and most obvious dimension of independence in Hacking's examples is that which distinguishes the different physical systems—the causal processes or mechanisms—that produce the traces detectable by different kinds of microscope. It has to be assumed that these causal pathways all emanate from, or interact in the production of, an ontologically unified subject: the entity or events that the microscope is meant to detect. At the same time, however, triangulation depends on the plausibility of the assumption that these different trace-generating systems are causally independent, in that they do not interact in such a way as to ensure an artificial congruence in the signals they transmit.

Independence in a second sense holds between the bodies of background knowledge, the auxiliaries, that are deployed in inferentially reconstructing the pathways by which signals are transmitted and received. The transfer to one field of empirical or theoretical results established in another—the basis for constructing any one line of evidence—depends on a limited assumption of ontological unity and of theoretical congruity between the source and target fields. That is, it is assumed that the causal processes exploited by microscopes are relevantly the same in the (export destination) contexts where they support mediated observa-

tion as in the source contexts where these causal processes are a primary object of study. Under these conditions, the knowledge of these causal systems developed by one science can serve as the basis for auxiliaries in another. But if horizontal independence is to be established, it must also be assumed that the background knowledge concerning causally distinct processes is epistemically independent. Here the crucial forms of inferential independence are those which ensure that coincidence in the images produced by different instruments is not an artifact of the instruments themselves or of the auxiliaries that inform our interpretation of the traces they enable us to detect. Triangulation thus depends on theoretical disunities between the different ranges of auxiliaries on which microscopists rely to make observations of the same entity or process; these include disunities of content, domain-defining presuppositions, and traditions of research practice that mitigate against an arbitrary congruence between lines of evidence constructed using different instruments.

One indication of such inferential (epistemic) independence may be that the background theories on which microscope makers rely have been developed by institutionally distinct disciplines. This disciplinary disunity is a third sense of independence that figures in archaeological contexts, with particular prominence in historical archaeology. Although these three senses of independence—causal, theoretical, and disciplinary—are often treated as one, it cannot be assumed that they will coincide. The same process of signal transmission might be detected, or interpreted, using very different bodies of background theory while nonetheless carrying the same distortion through different channels. Alternatively, bodies of background theory that are drawn from different disciplines and that seem distinct in content may share enough in the way of common assumptions—perhaps a consequence of the kinds of trade between fields described by Darden, Bechtel, Abrahamsen, and others—that persistent, compensating errors arise in the detection and interpretation of signals even when they are generated by causally independent processes and interpreted using apparently distinct bodies of theory.

The kind of (horizontal) independence that archaeologists invoke is assumed, ideally, to incorporate all three of these dimensions of inde-

pendence: causal independence is assumed to be aligned with an independence in the content of background theory that is marked, in turn, by the distinctness of its disciplinary sources. The most obvious archaeological examples of this ideal are cases in which different methods of physical dating are applied to material from a single archaeological context. Consider the use of tree ring counts and measures of radiocarbon decay, magnetic orientation, and the internal evolution of stylistic traditions to determine (respectively) absolute cutting, burning, and deposition dates and tradition-specific production dates. The disciplines that supply the relevant technologies of detection are certainly institutionally autonomous, and the content of their theories is substantially independent; it is unlikely that the assumptions that might produce error in the reconstruction of a date using principles from physics will be the same as those that might bias a date based on background knowledge from botany or sociocultural studies of stylistic change.[23] Finally, this independence in the content of the auxiliaries and in their disciplinary origins is especially compelling because it is assumed to reflect a genuine causal independence between the chemical, biological, and social processes that generated and transmitted the distinct kinds of material trace exploited by different dating techniques.

The case of historical archaeology makes clear, however, just how complex and uncertain the argument for epistemically significant independence between textual and archaeological sources can be. In some respects and in some instances the archaeological record can reasonably be assumed to be independent of the documentary record in all the senses described here. It may be entirely plausible that the contents of trash pits and various kinds of official documentary history are produced by such different means and for such different purposes that they can be regarded as causally independent, even though they derive from (and therefore serve as evidence of) the same community or set of historical events. Moreover, to effectively use such different kinds of material as a record of the (same) past it may be necessary to rely on interpretive techniques and bodies of background knowledge that derive from distinct research traditions and depend on fundamentally different skills and presuppositions—specifically, those necessary for the interpretation of documen-

tary records as opposed to the analysis of material culture.[24] In such cases historical and archaeological lines of evidence may be expected to provide a check on one another: the disunity of their sources confers epistemic advantage on their conjoint use.

But in many cases, these assumptions of independence cannot be made, and none can be assumed to be indicative of the others. The disposal of trash may reflect the same principles of decorum as writing for the public record, and both lines of evidence may systematically obscure precisely the underlying contradictions that are reflected in the silences of elitist history that historical archaeologists mean to correct. Indeed, there may be greater causal independence between different types of documentary record—for example, between legal statutes and personal diaries—than between certain kinds of archaeological and documentary record: public architecture and speeches made by the heads of state, for example. In addition, however resolute archaeologists and historians have been in maintaining the boundaries between their disciplines, they are almost certainly subject to many common influences and often rely on similar interpretive resources; they are affected by a range of bridging and integrating forces that persistently undermine the institutional disunities they guard so jealously. Thus there is no reason to believe that the politics structuring the debate about how to mark the quincentennial of Columbus's voyage would have had a fundamentally different impact on historians than on archaeologists studying the operations of various colonial powers in the Americas (see, e.g., Trouillot 1995: 108–153). Similarly, it is implausible that the systematically distorting romanticism about First Nations cultures critiqued by Trigger (1991) would have shaped the archaeological interpretations he considers but not the accounts developed by historians of the dynamics of contact. Historians and archaeologists often interpret the different records with which they deal in strikingly similar ways; consequently, they may consistently overlook or misinterpret aspects of their subject that seem incongruous (unpalatable or unrecognizable) to those using a common stock of background assumptions. The emergence of closely parallel feminist critiques in both fields makes it clear that the practice of deploying different kinds of evidence, even in deliberate conjunction, is not

in itself proof against pervasive androcentrism or sexism. Appearances of disciplinary and theoretical disunity may be deceiving.

Questions about the conceptual, causal, and disciplinary independence of distinct lines of evidence must be treated as empirically open and must be assessed on a case-by-case basis. Disciplinary boundaries may not cut the world at its joints where different orders of causal production are concerned, and they may not insulate neighboring disciplines from the influence of assumptions that are capable of inducing compensatory errors in seemingly independent lines of evidence. It follows that to determine epistemically relevant independence, two lines of inquiry are necessary: one to establish the extent to which the processes responsible for ostensibly different records are, in fact, *causally independent* of one another and another to determine the extent to which the background theories concerning these processes—the interpretive principles used to read these records —are *conceptually independent*. While questions of causal independence can be addressed only by first-order empirical research, questions of conceptual independence require a program of second-order, metascientific investigation that is both philosophical and empirical (specifically, sociological and historical); confounding presuppositions that are deeply embedded in disciplinary traditions may come to light only through systematic study of the various kinds and degrees of interaction that bind apparently distinct fields together. Whenever archaeologists assess the transferability and the (likely) independence of the auxiliaries they borrow, they make judgments about the reach across disciplinary boundaries of crosscutting interests, shared assumptions, and common theoretical models and methodologies. If such judgments are to bear this epistemic weight, they must be grounded in a detailed understanding of the diverse patterns of integration, trade, coordination, and differentiation that both unify and fragment the scientific enterprise.

METAPHILOSOPHICAL IMPLICATIONS

Although philosophers and colleagues in neighboring fields of science studies have largely abandoned global unity theses *about* the sciences, metaversions of these theses often underpin our own practice. The relations between history, philosophy, and sociology of science continue to be structured by claims that privilege a particular methodology as the only way to properly study science. Whether the approach in question is that of exact philosophy or informal conceptual analysis, technical or social history, sociometrics or ethnography, the assumption lying just below the surface is often that as a subject for investigation, science falls within the ambit of a specific discipline whose methodology is uniquely appropriate to its study. To be sure, this confident imperialism has been sharply contested in recent years. A number of sociologists now urge a strategy of "alternation" between diverse standpoints and methods for studying science, while philosophers have long negotiated an uneasy alliance with historians of science and some are now intent on socializing and humanizing, as well as naturalizing, the philosophical study of science. My claim, however, is that if unity theses are called into question as the working hypothesis that frames philosophical science studies, two meta-consequences follow that require a substantial extension of these initiatives.

First, the working hypothesis that frames our research must be redefined. We must finally set aside the polarized options defined by debate over global unity and disunity theses; neither is tenable and both obscure important features of research practice. Although unity cannot be presupposed, the scientific disciplines are unevenly and contingently interdependent in any number of ways that are crucial to their practice and success as a family of enterprises. If we are to understand the sciences, we must attend to the diverse networks of interaction responsible both for the proliferation and for the integration of distinct bodies of theory and research traditions. Doing so serves not just a philosophical interest but, more specifically, a normative and practical concern to clarify concepts, such as that of evidential independence, which are methodologically central to the various practices of science.

Reorientation along these lines requires, second, a commitment to methodological pluralism that substantially undermines the boundaries that persist between various fields of science studies. As the case of historical archaeology makes clear, epistemically salient notions of evidential independence cannot be explicated in strictly philosophical terms. Some kinds and degrees of interfield integration are necessary conditions for the

effective transfer of expertise and theory between fields. At the same time, the epistemic significance of appeals to diverse (horizontally independent) lines of evidence depends on the persistence of substantial ontological, epistemic, and institutional disunities between the sciences. To determine whether epistemically relevant independence holds in any particular case requires that we closely examine not just conceptual connections that may hold between fields but also the histories of discipline formation and the social, institutional dynamics that bind these fields (uneasily) together.[25]

Unification and Convergence in Archaeological Explanation

In the mid-1990s something of a watershed was reached in philosophical theorizing about explanation. While questions about explanation have always been central to philosophy of science, with the widely touted demise of positivism they assumed the status of paradigm-disrupting anomalies, and since the early 1970s a number of widely divergent approaches to understanding explanation have been continuously in play. After 1988 there appeared a spate of syntheses, overviews, and collections in which some of the central contributors, most visibly Wesley Salmon and Kitcher, undertook to bring order to this proliferation of positions. The upshot is a tripartite categorization of philosophical theories about explanation: epistemic, ontic, and erotetic.[1]

Epistemic theories of explanation offer a top-down account according to which explanations are distinguished by the way they organize what we know about the world, not any specific content or type of claim about the world. These include the original Hempel-Oppenheim (deductive-nomological) covering law models of explanation, the statistical and inductive variants of these models that were formulated through the 1970s, and information-theoretic accounts. They also include the unificationist models originally proposed by Friedman in 1974 and by Kitcher in 1976. On the unificationist account explanation is conceptual-ized as a function of the systematizing power of theory, though not mediated by a particular argument structure: a theory is explanatory when it "effects a significant *unification* in what we have to accept" (Friedman 1974: 14; emphasis in the original): "science increases our understanding of the world by reducing the number of independent phenomena we have to accept as ultimate or given," thereby rendering the world more "comprehensible" (14–15; see also Kitcher 1989: 432; 1981). Where Friedman's account ran into difficulties differentiating the units of basic or "brute" phenomena that are more or less successfully unified, Kitcher has moved to an "argument pattern" account of explanatory unification. He describes explanation as increasing scientific understanding "by showing how to derive descriptions of many phenomena using the same patterns of derivation again and again" (Kitcher 1989: 432). The central intuition here is that successful explanations allow the generation of as many conclusions as possible from as few premises as possible.[2]

By contrast to epistemic theories, *ontic accounts of explanation* represent a bottom-up approach; explanations are characterized in terms of their content. It may be required, for example, that they be grounded in an understanding of causal or other relations of dependence that obtain in the external world.[3] On the causalist account that Salmon has

advocated since the late 1970s,[4] explanations are understood to "reveal the mechanisms, causal or otherwise, that produce the facts we are trying to explain": "to explain is to expose the inner workings, to lay bare the hidden mechanisms, to open the black boxes nature presents to us" (W. Salmon 1989: 121, 134). Salmon thus insists that what counts as an explanation "depends on the kinds of mechanisms—causal or noncausal—that are [actually] operative in our world" (149–150), and cannot be settled a priori. On some ontic theories explanation may be grounded in an understanding of "worldly relations other than causation" (Ruben 1993b: 12)—for example, various forms of structural dependence and determination, identity, supervenience, and event (and entity) composition that "give significant structure to the world of events" (Kim 1974: 52; 1993) but are not strictly causal.

Pragmatic or erotetic theories of explanations are a third family; they characterize explanations not by appeal to any specific feature of content or form but rather as answers to "why" questions; explanations are accounts that satisfy the curiosity or puzzlement of a particular inquirer under given circumstances. As part of his program of formulating a viable (constructive) empiricism, in 1980 van Fraassen argued for just such a deflationary view of explanation: he reaffirmed the empiricist thesis that the systematization of observables, not the explanatory modeling of causes, is the primary aim of science and argued that what counts as an explanation is a function of the pragmatic circumstances of question asking, constrained only by the requirement that the content of answers given be scientifically acceptable (van Fraassen 1980; see also Lloyd and Anderson 1993). Others pursue the projects of distinguishing different types of explanation-eliciting questions and elaborating a fine-grained account of the pragmatics of answer giving (e.g., Bromberger 1966; Garfinkel 1981). In the interest of reconciling subjective and objective accounts of explanation, Railton (1981, 1989) proposes a distinction between an "ideal explanatory text" and "explanatory information." The ideal explanatory text constitutes the framework of complete, ideal understanding within which choices may be made to foreground different selections of explanatory information, depending on the circumstances under which an explanatory question is raised

(see also W. Salmon's discussion of Railton, 1989: 154–166).

There seems to be general agreement, at least among the synthesizers whose categorization has normalized debate, that the leading contenders among the theories of explanation on offer are particular versions of ontic and epistemic theories: namely, Salmon's causalist theory and Kitcher's unificationism. While Kitcher holds that causalist theories depend on metaphysically contentious claims about causal processes that are best understood in terms of the unifying power of our schemas—"objective dependencies among phenomena are all generated from our efforts at organization" (1993: 172)—Salmon suggests that there may be room for rapprochement, building on Railton's proposals. Perhaps unificationist and causalist accounts represent different but compatible strategies for understanding "the same facts," while pragmatic approaches "determine which way of 'reading' is appropriate in any given explanatory context" (W. Salmon 1989: 185).

I will argue that while a healthy pluralism is desirable, especially given the diversity of explanatory practices typical of the sciences (not to mention ordinary life), Salmon's conciliatory move may be premature. My thesis is that the powers of unification emphasized by Kitcher are dependent on the understanding of underlying mechanisms, dispositions, constitutions, and dependencies central to explanation on a causalist account. This case can be made through analysis of Kitcher's account of the conditions under which apparent improvements in unifying power may be judged spurious. But to clarify what is at issue here I consider, in some detail, an archaeological case in which debate about the merits of an ambitious and highly controversial explanatory account has unfolded along lines defined by precisely the intuitions that divide Salmon and Kitcher. Here the credibility of a powerfully unifying argument pattern—whether or not it should be accepted as a plausible explanation—depends fundamentally on the plausibility of its claims about the conditions actually responsible for the *explanandum* and not on an elaboration of its unificationist virtues. I first describe this case, and then consider its implications for the newly normalized philosophical debate about explanation.

RENFREW'S GRAND SYNTHESIS

One of the most ambitious and perplexing explanatory theories currently under discussion in archaeology is an account of contemporary linguistic diversity advanced by Renfrew in the late 1980s: a subsistence-driven demic-diffusion model of the long-term, large-scale cultural processes that he believes must explain the existence and distribution of linguistic macrofamilies. As originally developed in *Archaeology and Language: The Puzzle of Indo-European Origins* (1987), Renfrew's focus was the long-standing problem of explaining the "remarkable relations that link nearly all the European languages, many of the languages spoken in India and Pakistan, and some of those in the lands between" (1989b: 106).[5] His thesis was that these widely distributed linguistic affinities should be explained as a consequence of the Neolithic revolution. As agricultural subsistence technologies diffused across Europe in the early Neolithic (approximately 8000 B.P.), the populations using these technologies carried with them a common stem language, Proto-Indo-European, which inexorably displaced the diverse local languages of existing foraging societies. Renfrew describes this process as one of subsistence-driven demic-diffusion because, on his account, the mechanisms responsible for the linguistic diffusion of Proto-Indo-European were demographic pressures operating on the expanding population of agriculturalists, reinforced by what he describes as the inherent superiority of agricultural technologies. Renfrew has since argued that processes of demic-diffusion may explain much of the confusing pattern of language distribution in the contemporary world as a whole (1992b: 12) and may be supported by emerging patterns of genetic affinity among human populations. In both local (Indo-European) and global form, the demic-diffusion model is to be recommended, on Renfrew's account, because it holds out the promise of a "remarkable potential synthesis between archaeology and historical linguistics [and] . . . an emerging discipline which we might call 'historical genetics'" (1992a: 445–446).

To make the case for the demic-diffusion model, Renfrew develops a typology of the cultural processes by which a language may come to be spoken in a region and rejects the main alternatives to his preferred demic-diffusion hypothesis of linguistic replacement. He argues that the simplest model, that of initial colonization and continuous (local) linguistic development, is patently implausible. The first populations to enter Europe would have introduced a common stem language much too early (between 35,000 and 12,000 B.P.) to account for contemporary linguistic affinities. Processes of linguistic divergence (analogous to genetic drift) and of linguistic convergence (comparable to gene flow) would have generated a much more highly fragmented, locally diverse linguistic picture if they had operated continuously since initial colonization. Renfrew thus concludes that some intervening episode of linguistic recolonization must have occurred, introducing a proximate stem language to the region recently enough that contemporary Indo-European languages would still bear the marks of a common origin.

Having thus eliminated the explanatory models that posit initial colonization and continuous development, Renfrew's chief concern is to demonstrate that demic-diffusion is the most plausible of the linguistic replacement models available. In particular he is intent on establishing the inadequacy of a widely accepted alternative explanation of how Proto-Indo-European was introduced to the region in which Indo-European languages are now spoken: by a Kurdic invasion. On this account, the protolanguage was carried into the region by mounted warriors emanating from north of the Black Sea (western Russia) "somewhere between the late Neolithic period and the beginning of the Bronze Age," some 5,000 to 6,000 years ago (Renfrew 1989b: 108). On Renfrew's topology, this is an elite dominance model. As a family, such models postulate a process of linguistic replacement by which a relatively small, well-organized external force displaces an internal elite and imposes its language on the local population. While the Kurdic invasion hypothesis fits the time frame for linguistic replacement required by standard linguistic reconstructions of Proto-Indo-European, Renfrew insists that such a model is unsustainable both conceptually and empirically.

Although elite dominance models vary considerably in the originating homelands they postulate, the trajectory of the invasion or migration and the mechanisms responsible for population displacement and consequent linguistic replacement

all are flawed, Renfrew argues, by a shared assumption that something like our contemporary "separation of the non-urban world into distinct *ethnē*" can be projected thousands of years into prehistory. All assume a static three-way identification between linguistic communities, social units (ethnic identities or populations), and archaeological cultures. Renfrew objects that "a strongly developed ethnicity is not, in fact, a universal among human societies" (1988: 438), and was not likely to have obtained 5,000 to 6,000 years ago.[6] Moreover, the Kurdic invasion hypothesis makes specific assumptions about the technology and social organization of the invading population that are "a travesty of archaeological interpretation" (438). The military advantage of the Kurgan warriors remains hypothetical. The model offers no plausible account for why "hordes of mounted warriors [would] have moved west at the end of the Neolithic, subjugating the inhabitants of Europe and imposing the proto-Indo-European language on them" (Renfrew 1989b: 110). And there is no evidence that the societies of either the invaders or the populations invaded were centrally organized or socially stratified in ways Renfrew considers a necessary condition for the sort of conquest that could have brought about wholesale linguistic replacement (110). In short, there is scant evidence that the conditions necessary for an episode of elite dominance could have obtained in the period in question, even if there was large-scale movement of population.

Given the inadequacy of these competitors, Renfrew presents the case for some form of demic-diffusion (or demography-subsistence) model, according to which a large number of people bearing the required stem language diffuse slowly into a given territory and displace the old population (and its languages) not by force of arms but by introducing a "new exploitative technology" (Renfrew 1988: 439) that confers on the incoming population a decisive adaptive advantage. In Europe, Renfrew observes, the Neolithic revolution represents just such a process: "if one surveys European prehistory there is an event wide-ranging and radical enough in its effect to be a candidate, and that event does indeed fall squarely into the subsistence category: the coming of farming" (1989b: 110). The effect of this bold conjecture is to push the requisite episode of linguistic recolonization much further back into prehistory than histori-

cal linguists had considered plausible.[7] On Renfrew's account the diffusion of Proto-Indo-European should be understood as a consequence of the Neolithic transition, which occurred 8,000 years ago (6500–6000 B.C.E.)—some 3,000 years earlier than the appearance of the Kurdic invaders who, on the main rival explanation, spread Proto-Indo-European from the northern steppes in the shift from the Neolithic to the Bronze Age.

The specifics of Renfrew's demic-diffusion model are adapted from an influential account of the Neolithic revolution published by Ammerman and Cavalli-Sforza (1973, 1979), and later elaborated by Cavalli-Sforza in much more ambitious and controversial terms (e.g., 1997; see also Cavalli-Sforza, Piazza, and Mountain 1988, 1990; P. Ross 1991; and critical discussion by Bateman et al. 1990). Ammerman and Cavalli-Sforza proposed that the wheat and barley, goat and sheep agricultural complex, which had been traced back to central Anatolia where the prototypes of the domesticates later found throughout Europe existed in the wild, was carried into Europe in the seventh millennium B.C.E. by relatively small, incremental movements of farmers and their offspring. A crucial feature of this model is the assumption that the population density that farming could support had the potential to increase the population associated with a foraging economy by as much as a factor of fifty. Ammerman and Cavalli-Sforza estimate that this population pressure would have forced each generation of farmers to seek new territory at a rate of approximately 1 km a year (18 km in any direction per generation, where generations are estimated at twenty-five years each). On this "wave-of-advance" model, Renfrew argues, farming would have been carried across Europe in about 1,500 years—approximately the time frame suggested by archaeological evidence (1989b: 111).[8] The inexorable nature of this advance is due both to population pressure (the demographic component of the model) and to the adaptive advantage that agricultural subsistence practices and technology would have given the incoming population (the subsistence component). What Renfrew adds is that this slowly, steadily diffusing population of farmers carried with them not just agricultural technology but also their language, and that this language displaced other local sister languages to become the common linguistic foundation out of which contemporary Indo-

European languages emerged by processes of local divergence from one another. Secondary processes—processes of linguistic replacement or convergence caused by later episodes of elite dominance and ongoing contact through trading links and proximity—would then have redistributed these descendent languages and established an overlay of later lexical and structural commonalities.

In the original formulation of this wave-of-advance model, Ammerman and Cavalli-Sforza appealed not just to convergent patterns in the archaeological and linguistic evidence but also to congruencies with the distribution of gene frequencies in European populations, specifically in the distribution of blood types and antigens. For example, they make much of the fact that the frequency of the Rhesus negative factor is significantly higher among the Basque population, a linguistic isolate, than the surrounding European population, and that other genetic affinities correspond to linguistic affinities; they find in these data crucial support for the hypothesis that the Neolithization of Europe involved population diffusion and replacement. Although this is an aspect of Ammerman and Cavalli-Sforza's model that Renfrew does not invoke (see his criticisms, 1992a: 463–465), he does hold out hope that a more refined analysis of genetic markers may be an important source of collateral evidence for the population movements he postulates in connection with his demic-diffusion model of linguistic replacement.

Despite strong critical reactions to every aspect of this original model, Renfrew now argues that it can usefully be generalized to many other areas of the world. Some linguists propose the existence of a few broad macrofamilies that reduce the bewildering diversity of contemporary languages—some 5,000 to 10,000 distinct languages, depending on how they are individuated (Renfrew 1992a: 449)—to between seventeen and twenty linguistic phyla, excluding six or seven isolates and various pidgins and creoles of recent origin (Renfrew 1992b: 13; see also Ruhlen 1987). At the same time, Cavalli-Sforza argues that there is broad congruence between these linguistic families and the genetic affinities now being documented among contemporary human populations (1997; Cavalli-Sforza, Piazza, and Mountain 1988, 1990).

The case Renfrew makes for extending the demic-diffusion model to other linguistic macrofamilies closely parallels his original arguments for the Indo-European hypothesis. The affinities between the languages that constitute these macrofamilies cannot be explained by relatively simple models postulating a single episode of initial colonization followed by local processes of linguistic change.[9] Over 12,000 or more years, linguistic divergence would have generated a plethora of local languages whose connection to an original protolanguage would probably no longer be evident. To account for contemporary affinities, this diversity must have been reduced in many regions by episodes of recolonization like that postulated for the region in which Indo-European languages are now spoken. Renfrew argues that "much of the world's [contemporary] linguistic map" must have been shaped by large-scale linguistic replacement realized roughly between 7000 and 3000 B.C.E. (1992b: 39), a period in which waves of agricultural advance can be documented for many of the regions in question.

Renfrew's global thesis is, then, that the demic-diffusion model can be extended to roughly a third of the macrofamilies thus far identified by linguists (1992b: 24).[10] The broad outlines of contemporary linguistic families were established by the end of the Neolithic, and subsequent episodes of elite dominance (including colonial expansions of the last five centuries) have served primarily to complicate rather than fundamentally alter this picture. While Renfrew remains cautious about appeals to parallels between genetic and linguistic affinities, here, as in the case of Indo-European, he is hopeful that new techniques for molecular analysis will refine the existing phonetic dendrograms and put reconstructions of common genetic stock on a more secure footing (1992a: 467). The really significant genetic contributions to Renfrew's synthesis will come when these techniques are successfully applied to the surviving skeletal remains of ancestral populations.

RESERVATIONS AND QUESTIONS

CONVERGENCE ARGUMENTS AND UNIFICATION

An unmistakable sense of excitement accompanies this grand synthesis. Here we stand, on Renfrew's telling, heirs to decades—indeed, to a century or more—of intensive programs of research

in at least three independent fields that all bear on a common set of explanatory problems: archaeological work on the origins and spread of modern humans and on the rise of farming in various parts of the world, linguistic investigation of the distribution of contemporary languages and their affinities, and biogenetic studies of human populations. These lines of inquiry have now progressed to a point where each is in a position to map the large-scale distribution of linguistic, cultural, and genetic phenomena and to propose general explanatory models of how diversity in their realm might have arisen. What most intrigues Renfrew about the demic-diffusion model is the new synthesis it promises of these diverse lines of inquiry, not only in the Indo-European case but potentially across a number of different cultural and linguistic regions. The theme that figures most prominently in his advocacy of the demic-diffusion model is the remarkable nature of the "convergence" (1992b: 12, 1989b: 114)—the "congruence," the "mutual compatibility," the "curious parallel[s]" (1992a: 449)—that this model brings into view and makes intelligible.

A related theme, especially prominent in discussions published in the early 1990s, is Renfrew's conviction that any model-building exercise should be guided by a "principle of parsimony" (1992b: 16–17). Although the demic-diffusion synthesis is still very much a conjecture, he insists that it has "the merit . . . of offering a relatively simple account in historical terms for the distribution of languages of the world" (23). Indeed, Renfrew argues that "it is the function of models to simplify and make intelligible, so that despite the scepticism of some, it is no reproach to my explanations that they are simple, and offer simpler outcomes than are seen in reality among the data" (55). In these statements, Renfrew articulates a conception of the nature and aims of scientific explanation strikingly similar to the intuitions that Kitcher, among others, describes as central to unificationist theories of explanation.[11] He treats explanation as serving primarily to systematize as many and as diverse a range of phenomena, using as few premises and as limited a store of "argument pattern[s]" or "ways of thinking," as possible (Kitcher 1989).

Consistent with the central tenets of unificationism, Renfrew repeatedly defends the value of idealizations. His arguments here resonate with Kitcher's observation that a key step in developing an explanatory theory is always to formulate an idealized description of the *explanandum,* shifting the focus of explanatory inquiry from the question of why a particular object behaves as it does to that of why "ideal objects of this general type exhibit these properties" (Kitcher 1989: 453). With its strategy of unification by means of idealization, Renfrew's approach to explaining the distribution of Indo-European languages and other macro-language families is very much top-down—an example (if it succeeds) of theoretical explanation that proceeds by appeal to general principles, showing how particular *explananda* "fit into the universal scheme of things" (W. Salmon 1989: 183) or, at least, fit into larger and encompassing structures. It is specifically not the point of such explanations to provide a detailed account of the mechanisms or processes by which a given outcome is produced—the "underlying micro-structure of what they endeavor to explain" (W. Salmon 1989: 184)—as would be typical of the bottom-up, causalist approach that Salmon advocates. In evaluating prospective explanations, on this account, it is crucial that idealizations be formulated and selected with an eye to their scope of application (albeit subject to a proviso discussed below). And here again, Renfrew's intuitions about the significance of the synthesis afforded by the demic-diffusion model seems to be exactly those central to Kitcher's unificationism, especially where its extension to language families other than Indo-European is concerned.

But when pressed on the question of why simplifying idealization is desirable, Renfrew notes not only that it enlarges the scope of a model, allowing for a broader synthesis of disparate phenomena within its domain and across formerly distinct domains, but also that such unifying power enhances the credibility of the model. It is an indication that the model successfully captures what he describes as "an intelligible mechanism by which a basic process can be understood" (Renfrew 1989b: 463). This formulation is consistent with the unificationist intuition that basic-ness just is a matter of providing broad unification (Kitcher 1989: 487, 496–497), but Renfrew later adds a much stronger claim: if the demic-diffusion model proves applicable to a number of non-Indo-European language families—to "much of the world's language map" (1992b: 39)—then its

credibility will improve in the original domain of its application, as an explanation for the spread of Indo-European languages. Such extensions are understood not just to expand the unifying breadth of the model but to reinforce its claims about the causal efficacy of the mechanism invoked; if it seems likely that linguistic replacement was accomplished by means of demic-diffusion in other contexts, then it is all the more plausible that it could have been responsible for the spread of a stem language in the Indo-European case. Here Renfrew shifts from a claim about the explanatory power of the model, conceived first and foremost as a function of its capacity to bring diverse phenomena under a common pattern of argument, to a claim about the evidential support that accrues to the model (now construed in causalist terms) when a number of independent lines of evidence converge on its central claims. This latter line of response sits uneasily with the unificationist themes that dominate Renfrew's defenses of his model; he suggests that powers of unification are a virtue of explanation in part because they provide reason to believe the model's ontological and causal claims. Although invoking these causalist considerations introduces a considerable tension into his own arguments, in doing so Renfrew makes use of a pattern of justificatory argument that is ubiquitous in archaeology: namely, that it would be highly implausible, given the independence of these various lines of evidence, if the mechanisms postulated (abductively) to explain them did not actually exist and operate as proposed.

Despite recurrent epistemic pessimism about the prospects for making effective use of fragmentary, ephemeral archaeological data as evidence, I argue elsewhere that the strategies archaeologists have developed for exploiting a range of background knowledge can be very effective in establishing networks of evidential constraint (see chapters 12–15). The interpretive ladening of data with theory—their (inductive) constitution as evidence—is a complicated business, but the complications that arise from worries about circularity cut in both directions. Given pervasive disunity among the sciences on which archaeologists must rely to establish empirical claims about the temporal depth and contemporaneity of Neolithic sites, dietary profiles, prehistoric demography (especially changes in fertility), subsistence practices, social organization, and patterns of cultural con-

tact and diffusion—the claims central to the debate about the demic-diffusion model and its explanatory power as an account of the distribution of Proto-Indo-European—there is no guarantee that all the relevant lines of evidence will converge on one explanatory model rather than another (or, for that matter, on any of the models under consideration). When they do, archaeologists (sometimes) have grounds for confidence that they know, within a specifiable range of error, how old the record is, what plant resources a prehistoric community exploited, how resources were distributed, and perhaps how the community was organized productively and reproductively; they can establish that particular events and conditions, and not others, actually (or likely) did obtain in a particular past context as described. As I have suggested, the principle at work here is that of a modest "piecemeal" or "local" realism (see, respectively, R. Miller 1987; Wimsatt 1987: 23–24; see also chapter 5 above): to varying degrees it would be a miracle if each of these lines of evidence, given their independence from one another, incorporated compensating errors capable of producing a spurious convergence.[12] In these cases, the power of an explanatory hypothesis to induce convergence among disparate (inductively constituted) lines of evidence establishes its credibility as an account of the causal conditions (broadly construed) responsible for the surviving record.

Despite my sympathy for convergence arguments in this evidential sense, Renfrew's use of them gives me pause. Their appearance in some of his defensive arguments for the demic-diffusion model seems incongruous at best. They mark a significant slippage in what he means by convergence that allows him to shift from a primary emphasis on the explanatory power of the demic-diffusion model—a preoccupation with what this model can do, as an explanation, *for the archaeological and linguistic (and, prospectively, genetic) phenomena* in the wide range of locales where various macrofamily languages are now spoken—to a concern with what these phenomena can do, as evidence, *for the model* construed as an account of mechanisms and processes that actually produced these phenomena and their intriguing patterns of distribution and affinity. Renfrew's appeal to large-scale (quite literally global) consilience forces the question of *when* the convergence of evidence is compelling and when not.

It is by no means clear that Renfrew's synthesis of historical linguistics, archaeology, and historical genetics, and its extension to a range of linguistic macrofamilies, fills the evidential role he claims for it. That is, it is by no means clear that this unifying power establishes grounds for believing that the processes of linguistic diffusion and replacement posited by the model actually occurred and have the kind of realist, causalist explanatory power I have been claiming for the homelier reconstructions that find support in the unexpected convergence of diverse lines of archaeological evidence. In fact, a central point of contention in the debate about Renfrew's syntheses focuses precisely on what relationship holds between his highly abstract and simplified ("parsimonious") explanatory model and the reconstructions of local sequences of cultural transition it subsumes. In what follows I summarize the key lines of criticism brought against Renfrew's equivocal use of convergence arguments, in the process drawing out their implications for the philosophical debate about the nature and ground of explanatory power.

FOUR OBJECTIONS

First, much depends on how the linguistic *explanandum* is characterized. Renfrew's assessment that some form of linguistic replacement model is required turns on his claim that the current distribution of languages is too simple to be explained by initial colonization and the subsequent (local) differentiation of daughter languages. In fact, the global synthesis assumes the credibility of the macrofamily *constructs,* and these are themselves quite contentious in some respects. If they were to be rejected or substantially reformulated, Renfrew might well find himself in the awkward position of providing an elaborately unifying explanation for a nonpattern. As one pair of critics put it, referring to Renfrew's safest case, "a linguist would have expected the author to stress the fact that Indo-European is a construct, not a demonstrable reality" (Zvelebil and Zvelebil 1988: 575). In effect, a construct of this sort is already a substantially simplifying unification that prefigures the quest for a unifying explanation.[13] And in this case evidential nepotism threatens (to use Kosso's term, 1989); vertical independence is compromised to the extent that the linguistic evidence of

macrofamily affinities presupposes Renfrew's favored explanatory hypothesis.

Second, Renfrew's argument that demic diffusion is the most plausible linguistic replacement hypothesis depends on the claim that all serious competitors have been considered and are inadequate. Not surprisingly, a number of critics object that even if the existence of Indo-European or other macrofamilies is accepted, it does not follow that *an* event, a single unitary process of similar scale, must be invoked to explain this outcome (see Zvelebil and Zvelebil 1988). One such critic insists that Renfrew underestimates just how continuously dynamic language can be in small-scale, nonliterate societies; significant linguistic change can occur "without radical change in the material particulars of life and with an amount of change in the human gene pool so small as to be for all practical purposes undetectable" (Ehret 1988: 571). One implication of this potential for rapid local change is that language replacement at the time of the Neolithic transition may be too early to account for contemporary affinities among Indo-European languages: "it is by no means certain that after 8,000 years the languages introduced by the first farmers in Europe could even be recognized as having a common origin" (Sherratt 1988: 459). At the very least, several intermediate steps must be postulated for intervening time periods (mainly the Bronze Age) in which it *is* plausible that processes of linguistic convergence, the formation of common trading languages, and lesser episodes of invasion and subjugation (associated with the secondary products revolution) would have occurred. As a result, hypotheses that postulate messier, more localized processes of continuous development once more become attractive.

In this case, contra Renfrew, models of convergence through interaction or the formation of creoles and a lingua franca may well have the resources to explain contemporary language distributions without invoking the large-scale diffusion of a protolanguage ancestral to those that now appear similar (Sherratt and Sherratt 1988). Perhaps the more local (but widespread) movements of people and cultural traits documented for the Bronze Age did constitute migrations and diffusions of cultural influence capable of accounting for contemporary linguistic affinities even if they do not constitute an episode of elite dominance

(Anthony 1996; Anthony and Wailes 1988).[14] The general point is that "by starting from the premise of unity, we simply stack the deck" (Barker 1989: 448). It is not at all obvious that Renfrew's demic-diffusion model is the only one capable of explaining the existence of contemporary linguistic macrofamilies, if we are prepared to question that initial premise and consider less tidy models of prehistory. His claim that it is the only adequate option on offer reflects an implicit metaphysical commitment to the view that causes must match effects in scale, and that it must be possible to discern a causal hierarchy in which the messy, multicomponent factors distinctive of local contexts must ultimately depend on (or be reducible to) a small set of simple, "basic" causal processes.[15] The act of making this assumption determines in advance what range of explanatory hypotheses can be considered, establishing a reference class defined by the key characteristics of the hypothesis Renfrew himself favors. Again, epistemic independence is compromised when the evidence a hypothesis is designed to unify is then cited as its main source of empirical support.

Third, a number of archaeological critics have objected that even if Renfrew is granted his arguments for preferring hypotheses that postulate a single, fundamental replacement process, it is by no means clear that the demic-diffusion model has the resources to explain the existence of Indo-European, or indeed other macrofamily languages. Renfrew helps himself to a number of assumptions—about the causal efficacy of the (subsistence) mechanisms and (demographic) catalyst central to this model—that his critics challenge. For example, why should we assume that early Neolithic farmers have such a decisive adaptive advantage over foragers that they will inevitably displace them? In many locales, both in Europe and elsewhere, there is evidence that farming did not automatically or completely displace foraging and gathering-hunting modes of subsistence; sometimes foragers and farmers coexisted for a very long time, and often those who made use of cultigens relied on a mixed subsistence strategy. Moreover, when farming did ultimately prevail, it was often through a much slower and more uneven process than Renfrew's model envisions.[16] In particular, given local continuities in cultural traditions through the Neolithic transition, it seems that farming *technology* often diffused on its own; the methods and tools of farming were taken up, piecemeal and syncretically, by indigenous foragers who did not necessarily find themselves displaced as a population, and did not necessarily adopt other cultural practices associated with farming.[17] This pattern leaves open the question of whether, and to what extent, the language of the original farmers diffused with their farming technology.[18]

A related criticism focuses on the demic component of the model, drawing attention to the fact that the proposed catalyst for diffusion—population pressure—is not an automatic corollary to the advent of farming. The fiftyfold increase cited by Renfrew (and by Ammerman and Cavalli-Sforza) is a *potential* figure, but "it cannot be assumed that such potential had a profound impact in the Neolithic[.] . . . Neolithic farmers faced many social, technological and environmental handicaps in Europe which might have reduced their reproduction capacity" (Zvelebil and Zvelebil 1988: 579). Indeed, in many areas the health status of early farming populations seems to have been poorer than that of their Mesolithic counterparts, and their population densities were not different enough from those of foragers for demographic pressure to have functioned as the sort of catalyst required by Renfrew's demic-diffusion model. Reflecting on these and related problems, a number of archaeological critics conclude that Renfrew's updated and expanded formulation of the wave-of-advance model remains, in its specifics, "an improbable hypothesis for most parts of the continent" (Zvelebil and Zvelebil 1988: 579), and similar objections have been directed against the global synthesis. In short, collateral evidence is lacking for key elements of Renfrew's hypothesis, construed in causalist terms.

These considerations lead, in turn, to a fourth (and final) critical point that raises directly the philosophical issues that concern me here. A number of Renfrew's critics object that his model is inadequate as a matter of principle because it is not properly grounded in, or congruent with, lower-level, local reconstructions of the transitional processes responsible for the Neolithic revolution. Generalizing on this concern, they question the wisdom of his commitment to idealization and synthesis; perhaps the unifying power Renfrew so values is not, in fact, a virtue that should be given priority over all else. One critic

pointedly describes the dangers of "excessively *a priori*" models as they arise in Renfrew's case: "Whilst the 'wave-of-advance' model has a beguiling simplicity, it probably misrepresents the reality of the process so profoundly that it may not be useful to keep it, albeit hedged around with the increasing number of ifs and buts about regional 'acculturation' and 'Neolithisation,' as our central notion for what was going on" (Barker 1988: 449). A relatively sympathetic commentator observes that "any enquiry which claims to be scientific or even merely systematic has to be shaped by models of some kind, whether these are explicit or not[,] . . . [but serious problems can arise when models are] generated and shaped by mathematical criteria of elegance rather than by abstraction from the data" (Coleman 1988: 451).[19]

In short, Renfrew's critics raise serious questions about both the inherent plausibility and the archaeological applicability of his demic-diffusion model, suggesting that the "grand synthesis" may be spurious. They object that many of the instances the model is meant to cover do not conform to its expectations, that the mechanisms he posits to account for linguistic replacement are causally inefficacious even if they were instantiated in the contexts where they are supposed to have operated, and that the messier processes described by alternative models are not as obviously incapable of producing the outcomes to be explained as Renfrew had supposed, although they are more complicated and less powerfully unifying. Taken together, these critics counter Renfrew's appeal to the unifying power of his model with demands that it should (also) meet the conditions of adequacy central to an ontic (causalist) conception explanation. They require Renfrew to provide an evidentially well-supported account of the mechanisms by which the Neolithic revolution brought about linguistic replacement in the specific locales covered by his demic-diffusion synthesis, and they are suspicious of appeals to the virtues of simplicity and unifying power as grounds in themselves for accepting Renfrew's synthesis, unless causalist conditions of adequacy are met.

CAUSALIST AND UNIFICATIONIST CRITERIA OF ADEQUACY

Renfrew's response to these objections takes two forms. In some contexts he seems prepared to take seriously the causalist intuitions that underlie his critics' objections, but argues that the demic-diffusion model is an idealization intended to capture, at a high level of abstraction, "primary processes" that operate at a very large scale. It is no reproach to such a model that it fails to capture the details of the Neolithic transition in specific locales: "The ultimate explanation for the present distribution of Indo-European languages will be a more complicated one than I have presented . . . but second-order (mainly later) processes can only be correctly interpreted if they are seen within a frame of reference which is approximately valid for the primary processes" (Renfrew 1988: 466).[20]

Perhaps the demic-diffusion model is meant to describe the structure and mode of operation of underlying primary processes on the understanding that complementary models will provide a detailed account of mediating secondary processes by which they were realized in particular locales. Or perhaps the explanatory power of claims about such primary processes lies in their ability to delineate broad *categories* of mechanism or process that may have taken quite different forms in specific instances. On either approach the demic-diffusion model provides an idealization of causal factors, as a causalist would say it must to have explanatory power, but draws attention to emergent properties of these factors or to processes that operate at a different scale than those of interest to Renfrew's more particularist critics. On this reading his model may best be construed as provisional, an example of the various types of "false models" that, on Wimsatt's account, "act as a starting point in a series of models of increasing complexity and realism"; they "suggest . . . alternative lines for the explanation of the phenomena," or provide a "template that captures larger or otherwise more obvious effects," thereby making possible more accurate modeling of smaller-scale, local phenomena (1987: 30–31).[21]

In other contexts, however, Renfrew sidesteps the objections raised by causalist critics, insisting on an ontologically thin reading of the claims he makes about "basic processes" and claiming noncausalist virtues for his proposed explanation. He reasserts the principle that models necessarily simplify and idealize in the interests of establishing a powerful, wide-ranging "generalizable" synthesis (Renfrew 1988: 463). It should not be held against them that they do not accurately describe

all (or any) particular instances in their domain, that they "offer simpler outcomes than are seen in reality among the data" (Renfrew 1992b: 55). Presumably, then, Renfrew's model should be held accountable *not* to individual instances but to aggregate outcomes characterized in appropriately general terms; it is not necessary that any or all local Neolithic transitions follow a particular pattern, only that they should result in an overall spread of farming that correlates, in the area affected, with the distribution of contemporary language families like Indo-European. At one point Renfrew goes so far as to insist that the wave-of-advance postulated by Ammerman and Cavalli-Sforza model does not, in fact, make empirical assertions about actual Neolithic processes of transition; the 1 km a year rate of advance is a "factual assertion about the mathematics of the model: it is not an assertion of fixed rates of change" (Renfrew 1988: 463). In this case, rather than being provisionally false like Wimsatt's "false models" (1987), perhaps the inaccuracies of Renfrew's demic-diffusion model are intentional, like Cartwright's laws (in *How the Laws of Physics Lie,* 1984): his synthesis does not assume or establish grounds for ontological commitment to claims about underlying ("basic") causal processes, just grounds for accepting the model as a formal heuristic—a unifying argument pattern—that serves to systematize, with sufficient accuracy for specific purposes, the aggregate inputs and outputs of large-scale, long-term cultural processes. The significant question then becomes whether the features Renfrew has subtracted or added or smoothed in his idealization make too large a difference in outcome for the idealization to be acceptable.[22] To assess Renfrew's demic-diffusion model in these terms, it would be necessary to specify more clearly what ends unification is meant to serve in the cases the model covers.

THE PROSPECTS FOR RAPPROCHEMENT

If consistently maintained, Renfrew's second strategy of response may seem to defuse the objections of his critics. His objectives are just different from theirs. What he offers is a powerful unification of diverse phenomena under a single, elegant (simple) explanatory model, an argument pattern that can be repeated again and again in explaining the linguistic features of a wide range of cultural contexts (within and beyond the Indo-European case). This unifying power has considerable appeal, though it comes at the cost of adequacy to local details and cannot be expected to account for why or how the phenomena subsumed by the model should manifest the patterns that allow their unification.[23] In this case it would seem that Renfrew and his critics are simply arguing at cross-purposes. Perhaps Salmon's parable of rapprochement is relevant here. He describes a wager laid by a physicist colleague that the balloon held by a young boy on an airplane would move toward the front of the cabin at takeoff, rather than toward the rear. The physicist won the bet but, Salmon notes, two explanations could equally be given to account for the phenomena: one, taking the form of a causal/mechanical explanation, would cite the behavior of expanding and jostling molecules and the other, exemplifying a unificationist approach, would appeal to the general Einsteinian principle that establishes an equivalence between the effects of acceleration and the effects of a gravitational field (W. Salmon 1989: 183). Salmon argues that "both of these explanations are legitimate and . . . each is illuminating in its own way" (184). He therefore urges a "rapprochement between the two approaches to scientific explanation that have been in conflict for at least three decades," mediated by an assessment of the pragmatic considerations that determine the circumstances under which each of these modes of explanation is appropriate (185).

I believe, however, that there is more at stake than simply a judicious decision to focus on different aspects of the subject domain and the (ideal) explanatory text that it supports. The causalist objections raised by Renfrew's critics should be telling for Renfrew even if he were to adopt a consistently unificationist stance. I will first indicate why this is the case with reference to Renfrew's synthesis and Kitcher's account of unificationism, and then conclude with a more general philosophical observation about models of explanation and their relationship to arguments of confirmation that depend on the convergence of diverse lines of evidence.

Although a staunch advocate for the "church of unification," Kitcher is careful to counter the possibility that the principle of explanatory unification, if unchecked, "could run riot over the deliverances of experience" (1989: 489), opening the

way to explanatory accounts whose superior unifying power is realized by arbitrarily fusing or embedding patterns, or by embracing implausible beliefs whose only recommendation is that they effect unification. He insists, in this connection, on the "proviso" (491) that explanatory unification must be "conditional on principles that govern the modification of language and that rule on the acceptability of the proposed beliefs" (489). Any modifications to the existing knowledge base or language that a new theory proposes—the introduction or subtraction of beliefs to the knowledge base (K) and of predicates to the language (L)—must be justified on grounds that are, in effect, independent of any appeal to the unifying power of the theory and its modifications. If the dispute about the merits of Renfrew's synthesis is set in the larger context of theoretical and methodological debate within archaeology, it becomes clear that his critics are drawing attention to a number of ways in which Renfrew has not met Kitcher's proviso.

Renfrew's critics are frequently concerned *not just* that his passion for synthesis and simplicity obscures a number of complexities that are important if you have a taste for causal models or otherwise prefer to focus on the specifics of a given prehistoric period and locale; their complaint is not against idealization as such. Rather, they object that Renfrew is highly selective in granting priority to a small range of factors—specifically subsistence-technological and demographic factors—that, they insist, taken on their own cannot account for the phenomena in question. One such critic argues that there is a pressing need to "put aside the question of 'origins' that has dominated the subject [of Indo-European] for a hundred years" (Barker 1988: 449); in this spirit, he urges the importance of coming to terms with the vagaries of modeling the *social* processes that mediated the response of human communities to the ecological factors, the biological desiderata of reproduction, and the technological and subsistence innovations associated with farming that Renfrew privileges as key catalysts and basic causal processes.[24] The counterexamples introduced by such critics—for example, local transitions where farming was adopted only very slowly, was not associated with any major increase in population density, and did not involve wholesale

replacement of local populations or cultures—serve to foreground the role and effects of precisely the sorts of social, symbolic, and cultural factors that Renfrew systematically discounts.

This objection has particular significance when considered in light of the intense debate among North American archaeologists about explanatory goals and criteria of explanatory adequacy since the late 1960s (see chapter 4). Renfrew maintains a broad allegiance to the central tenets of processual archaeology: specifically, its commitment to an eco-materialist conception of the cultural subject and the conviction that if the technological and adaptive dimensions of these systems are granted causal primacy, it will be possible to set archaeological interpretation on a firm scientific footing; all aspects of cultural systems will be explicable in terms of those (material, eco-environmental) aspects of the cultural past that can be most reliably reconstructed. Despite trenchant criticisms of these methodological and theoretical commitments, Lewis Binford continues to insist on a quite uncompromising and reductive form of this thesis: "institutions and cultural forms [which presumably include Renfrew's 'basic processes'] must be thought of as having a life *independent of their participants;* they are the conditioners of the participants' behavior" (1983: 221; emphasis added). Given this understanding of the causal structure of cultural systems, Binford urges that archaeologists focus on "the macroforces that condition and modify lifeways in contexts unappreciated by the participants within complex thermodynamic systems" (1986: 474). The internal dynamics of cultural systems—social relations and structures, ideational factors, the entire ethnographic lifeworld of human agents—are thus ruled out of account as irrelevant to archaeological explanation. On eco-materialist principles, they are assumed to have no causal efficacy at the level of large-scale system dynamics; to use Renfrew's term, they can be treated as (epiphenomenal) "secondary" factors and processes. Although Renfrew distances himself from Binford's more extreme statements—he is, after all, concerned to make sense of linguistic affinities and is a prominent advocate of "cognitive archaeology" (Renfrew 1993a)—he does presuppose something like Binford's distinction between internal or ethnographic, context- and agent-specific factors (com-

ponents of secondary processes) and emergent system-level dynamics (primary processes). And, among all the systemic processes that might play a role, he accords technological, demographic, and subsistence-related factors special causal efficacy in his explanation of Indo-European and other linguistic macrofamilies.

Those engaged in the debate with Renfrew are by no means among the most radical critics of processual archaeology, but their substantive objections to his model raise serious challenges to the more reductive and functionalist elements of its eco-materialist conception of culture. Matters are far from settled; many aspects of the processualist paradigm fruitfully persist alongside a diversity of anti- or postprocessual approaches. Nevertheless, it seems fair to say that Renfrew's critics engage the resources of a knowledge base (K') that has been significantly modified by arguments establishing that however difficult the task may be of reconstructing the internal social dynamics and ethnographic dimensions of past cultural systems, archaeologists cannot assume their explanatory and causal irrelevance at either a local or a systemic level, whatever the methodological advantages of such an assumption. Critics of processual archaeology routinely point out that there is much greater variability in the archaeological record than can be accounted for in adaptive-functionalist or eco-reductive terms (see, e.g., Hodder 1982b, 1986; chapters 4 and 7 above), and they appeal to collateral ethnohistoric evidence to establish, in general terms, the limitations of explanatory idealizations that privilege these factors.

These broad theoretical concerns are central to the debate about the adequacy of Renfrew's demic-diffusion model. Various sorts of social factors and internal dynamics are specifically what his critics insist are relevant for understanding how and why farming advanced in the (particular) way it did in various contexts; they are also relevant for determining whether, in fact, farming's advance could have been responsible for the processes of linguistic replacement that Renfrew considers necessary to explain contemporary linguistic macrofamilies. The force of their objections is that Renfrew has not rebutted the collateral arguments that call into question his processual assumptions about the culture-trans-forming powers of technological advantage and demographic pressure. That is, he has not provided grounds for resisting the shift from K to K'—from a restricted eco-materialism to a conceptual framework that includes consideration of social, historical factors—apart from his repeated assertions that if the K-beliefs constitutive of the demic-diffusion hypothesis are retained, they promise powerful cross-context and cross-field unification. The situation is inversely analogous to that faced by the continental drift hypothesis in the early days of its elaboration, as described by Kitcher; it was beside the point to "expand the inventory of the advantages of unification" until objections to the very possibility of continents drifting had been addressed (1989: 492).

Notice, however, what Kitcher's proviso requires of Renfrew for an effective rebuttal to these objections. To counter concerns about his failure to modify key (processual) beliefs about the cultural subject, Renfrew must provide independent (nonunificationist) grounds for believing both that the complex of subsistence, technological, and demographic factors he postulates did actually obtain in the contexts in question and that they had the causal capacity (broadly construed) to bring about large-scale linguistic replacement as the primary processes responsible for establishing Proto-Indo-European in the regions where its daughter languages are now spoken. That is, he must establish that the sociocultural factors complicating this picture in most locales are causally dependent (or irrelevant), so far as this crucial transition is concerned. And he must show that wholesale linguistic replacement as early as 8000 B.P., in the case of Proto-Indo-European, can account for the contemporary linguistic macrofamilies he means to explain without recourse to explanatory models that grant a central role to secondary (local) processes of continuous linguistic development. Kitcher's proviso, like Renfrew's critics, thus requires systematic evaluation of the claims Renfrew makes about the causal powers and capacities of the various factors cited by the demic-diffusion model. Indeed, at every level the debate over Renfrew's demic-diffusion synthesis turns on judgments about the credibility of precisely the sorts of claims central to an ontic, if not specifically causal, model of explanation. Far from being purely heuristic, assumptions about the causal ef-

ficacy of demographic and technological/subsistence factors inform Renfrew's judgments about how to idealize in the first place; they underwrite his assessment that social, internal factors had negligible effects at a systemic level. Most critiques of his model make these assumptions explicit and call them into question. And, in the end, the acceptability of Renfrew's nonmodification of processual beliefs will depend on whether such causalist claims can be sustained empirically, even on a consistently unificationist view of the aims of explanation.

I suspect that the pivotal role played by such causal claims (and the need to establish their credibility) is not unique to Renfrew's model or to archaeology. I propose, more generally, that ontic considerations of a broadly causal sort routinely reenter the picture with Kitcher's proviso. Whenever appeals to unification are conditional on independent principles governing belief modification (Kitcher 1989: 489), as often as not the principles in question will specify conditions under which it is reasonable to believe that specific causal mechanisms, or other (structural) relations of dependence and determination, actually exist and have the powers or liabilities attributed to them. By extension, Renfrew's critics challenge not just his commitment to processual ideals or his (inconsistently maintained) unificationist view of explanation, but also the use he makes of convergent lines of evidence to support the claims about causal mechanisms and processes central to his demic-diffusion account of the spread of Proto-Indo-European. Renfrew's appeal to the unifying power of his model as a source of evidence as well as explanatory power is problematic inasmuch as, at a number of junctures, the independence of the evidence he invokes from his test hypothesis is compromised. The capacity of the model to integrate, under one argument pattern, a range of archaeological and historical-linguistic phenomena is the primary reason for positing demic-diffusion as the mechanism responsible for the linguistic outcomes that require explanation, but Renfrew provides little evidence that this mechanism was (or could have been) responsible for the spread of Proto-Indo-European independent of that which suggested the model in the first place. By contrast, his critics provide considerable evidence that it could not be responsible, or was not, in a number of specific locales. Perhaps the

appeal to convergent evidence carries with it a requirement beyond epistemic independence of the various kinds discussed in previous chapters. A model's ability to fit multiple lines of evidence (unifying and, in this sense, explaining them) is not in itself grounds for concluding that its ontological and causal claims should be accepted; if the dangers of reification are to be avoided, there must be evidence for the existence and operation of the entities or mechanisms posited that is independent of the outcomes that the model was designed to explain.

I conclude with a jointly philosophical and archaeological observation. It is no rebuke to ontic theorists that it is "a purely contingent truth," on their view of explanation, that the independent causal structure of the world includes a limited number of basic mechanisms, rendering "unification . . . at best a contingent commitment of the tracing of causal structure" (Kitcher 1989: 497). Although it is too early to tell how the debate between archaeological processualists and anti- or postprocessualists will turn out, I believe that we are witnessing here, at bottom, a dispute about whether the cultural subject domain studied by archaeologists is structured by a sufficiently small number of basic mechanisms to support a rigorous unificationism of the sort endorsed by Renfrew. Several decades of work under the aegis of the (positivist) New Archaeology have left us with the growing realization that, as a matter of contingent (if explanatorily unfortunate) fact, the cultural worlds studied by archaeologists are sufficiently complex that they require an expanded store of argument patterns, many of which are not widely applicable. It remains an open and empirical question what kinds of mechanisms or processes shape the cultural formations that archaeologists hope to reconstruct and explain, but all indications are that simplifying, reductive models are unequal to the task of understanding these historically and dimensionally complex systems.[25]

The strategies archaeologists are using to sort out the scope and plausibility of claims about basic mechanisms (which are not at all specific to archaeology) turn on the judicious use of (evidential) convergence arguments to assess the plausibility of claims about the causal processes, structures, and relations of dependence responsible for

prehistoric cultural forms and their archaeological record. The philosophical lesson here is that the viability of a unificationist program (and its associated methodological principles) is contingent on facts about the world—specifically, facts about the nature of the generative mechanisms and structures of dependency that actually inhere (or not) in the subject domains under investigation. And determining these facts of the matter requires a variety of evidence, selected with an eye to countering not only the threat of circularity but also a tendency to reify those hypothetical constructs that seem equal to the task of integrating the bewildering complexity of evidence that is the archaeological record.

Issues of Accountability

Ethical Dilemmas in Archaeological Practice

The (Trans)formation of Disciplinary Identity

From its inception as a museum- and university-based discipline early in the twentieth century, one of the defining features of North American archaeology has been its identification as a scientific enterprise. This is evident not only in the programmatic literature and, indeed, in the training and practice, institutional location, and funding base of most North American archaeology but also in the bylaws and statements on ethics adopted by the major archaeological societies from the 1970s on. In some cases—for example, that of the Society for American Archaeology (SAA)—these policies have undergone several rounds of revision as archaeologists grapple with profound changes in the contexts in which they are trained and employed and in the ethical conflicts they face. The emphasis on responsible scientific practice has long been a central commitment of the SAA, and of many other archaeological societies, but what that means for the activities of individual archaeologists has been substantially reformulated over the years.

Two developments are of particular significance in this connection.[1] One is a pressure to professionalize that has grown as an increasing number of archaeologists have found themselves involved in culture resource management (CRM), employed by government agencies responsible for heritage protection and as consultants to industry,

rather than in academic research. From the mid-1950s on, a vocal contingent within the SAA has argued the need to codify professional, scientific standards of practice, specifying "who an archaeologist was and what that person was qualified to do" (McGimsey 1995: 11). In addition, since the mid-1970s archaeologists in all sectors have been increasingly active in advocacy and conservation efforts, as they respond to the accelerating destruction of archaeological resources by construction, agriculture, and other land-development projects on the one hand and by vandals and looters of antiquities on the other. Here, a commitment to scientific goals provides the justification for archaeological conservation policies and salvage efforts; archaeological sites and artifacts are to be valued and protected because they are an irreplaceable resource for understanding the cultural past.

In the same period, growing challenges from quite different directions have put this disciplinary identity—this alignment of scientific goals with an emerging professionalism and a conservationist ethic—under enormous strain. The ideal of professional disengagement from commercial (nonscientific) interests in the archaeological record is proving increasingly hard to realize, given the dramatic expansion of the antiquities market and the pervasive, often indirect and unintentional, entanglement of professional archaeology

with commercial interests in archaeological resources. At the same time, nonarchaeological interest groups, most especially Native Americans (the First Nations), object that they are not served by scientific exploitation of the record. Within the SAA these challenges have precipitated a new round of review of existing ethics policies, throwing into relief a fundamental tension in the ethics stance developed by the SAA and sister organizations: North American archaeologists identify themselves both as primary users of archaeological resources (i.e., as scientific practitioners and professionals whose central aim is to exploit the archaeological record in a particular way) and as advocates for and protectors of these resources.

My central aim in this chapter is to develop an analysis of issues pertaining to disciplinary identity that underlie current ethics debates in North American archaeology. I first consider ways in which the scientific ideals that came to dominate the field (described in previous chapters) informed the ethics statements developed by a number of North American archaeological societies, and then discuss some of the conflicts over questions of accountability that are now pushing the limits of this disciplinary identity; I conclude with a brief account of the response to these challenges now emerging in the SAA. The debates I discuss are complex and very much in process; what I offer by way of analysis must remain tentative, though I hope it will serve to demonstrate how closely interconnected are the epistemic goals and the ethical, political commitments that define the discipline.

SCIENCE, COMMUNITY STANDARDS, AND CONSERVATION

In the first decades of the twentieth century, members of the cohort of North American archaeologists instrumental in professionalizing the field were intent, above all, on clearly distinguishing their enterprise from the "woefully haphazard and uncoordinated" forms of practice associated with an antiquarian interest in the archaeological record (Dixon 1913: 563). By the end of World War I, they confidently declared that being an archaeologist no longer "meant being a mere collector of curious and expensive objects once used by man" (Wissler 1917: 100); a narrow preoccupation with the objects themselves was no longer acceptable (Dixon 1913: 565).[2] Any exploitation of the archae-

ological record must be designed to answer key questions about culture history and cultural development, and for that end the "mere finding of things" (Wissler 1917: 100) would never suffice; archaeologists must adopt "saner and more truly scientific methods" (Dixon 1913: 565). These themes have since been repeated at a number of critical junctures (see chapters 2–4). The conviction that "archaeology is anthropology or it is nothing," as the point was later put (e.g., Willey and Phillips 1958: 2, quoted in L. Binford 1962: 217), and that archaeological practice must be rigorously scientific, reappears in the 1930s shortly after the SAA was established, in the period when federal works programs in the United States stimulated the first massive expansion of archaeological training and employment. They are the subtext of methodological debates that arose in the 1950s, when a rapidly growing post–World War II cohort of archaeologists was struggling to bring order to the vast array of sites and materials that were by then available for analysis.[3] And they were reasserted in uncompromising terms in the 1960s and 1970s by the proponents of the New Archaeology, in the period when the scale of professional archaeological practice was again expanding dramatically in response to the legislation protecting archaeological sites and resources that gave rise to the CRM industry.[4] Although the 1980s and 1990s saw sustained criticism of the New Archaeology, broadly scientific ideals continue to dominate North American archaeology. On this view, archaeology *proper* is concerned not with the recovery of archaeological material as an end in itself, or with archaeological objects as such, but with systematic investigation of the archaeological record as a source of evidence, a scientific resource.

In the extended process through which this disciplinary identity has taken shape—itself a matter of negotiating the boundaries between archaeological and nonscientific or nonprofessional interests in the archaeological record—these defining scientific, anthropological ideals have been juxtaposed with a number of other emerging concerns and responsibilities. The first of these, the pressure to professionalize, became an explicit focus of debate within the SAA by 1954, when the postwar expansion of graduate training and employment in archaeology led some members to urge the society to establish a system by which archaeologists employed in increasingly diverse settings

could be held accountable for minimal levels of training and standards of practice, subject to a common code of professional conduct. But as McGimsey noted in a retrospective account of these debates, there was a strong countervailing sentiment that the SAA should not undertake to "define the difference between professional and non-professional (amateur) archaeologists" (1995: 11). This commitment to inclusiveness continues to be a strong influence in the SAA. Avocational practitioners are recognized not only by the SAA but also by many of its sister organizations (e.g., the Archaeological Institute of America and the Society for Historical Archaeology) as playing a central role in the field; in many cases, they are as well-trained and as committed to anthropological goals as those who do archaeology for a living.

When a position on standards and ethics was adopted by the SAA in 1961, it took the form of a report drafted by the SAA's Committee on Ethics and Standards titled "Four Statements for Archaeology" (Champe et al. 1961). These statements define the central objectives of the field of archaeology and provide general guidelines for practice and training, but they avoid any detailed specification of professional credentials and responsibilities. Archaeology is characterized, in the first statement, as "a branch of the science of anthropology . . . concerned with the reconstruction of past human life and culture" (Champe et al. 1961: 137; elsewhere, a "scholarly discipline," 138) whose "primary data lie in material objects and their relationships." In a second statement on "methods in archaeology," the committee stressed that the value of these objects lies in "their status as documents, and is not intrinsic." In this spirit, it censured "disregard of proper archaeological methods" (137) in collecting, recording, and reporting these data—such behavior was grounds for expulsion from the society. In all aspects of their practice, members of the SAA were to "aim at preserving all recoverable information" (137) so it would be available for further study. And in a carefully worded closing statement on "training in archaeology," the committee observes that although, in the past, some leading archaeologists had acquired the skills necessary for competent practice "without formal training," they had nonetheless "spent years in the study of archaeology as a science"; the guidelines close with a description of the kinds of formal training and supervised field experience that were then appropriate for "persons planning to enter archaeology as a career" (138).

The message of these statements is clear and conforms closely to the central themes of the programmatic debate that had begun fifty years earlier: whether career professionals or skilled avocational practitioners, archaeologists are distinguished by their commitment to scientific goals and standards of practice. The SAA thus condemns, as a violation of the responsibilities associated with these commitments, any form of "uncontrolled excavation by persons who have not been trained in the basic techniques of field archaeology and scholarship," as well as the "willful destruction, distortion, or concealment of the data of archaeology" and the "buying and selling of artifacts" (Champe et al. 1961: 138, 137). The rationale for censuring this last activity explicitly affirms the centrality of a commitment to scientific goals; the commercial trade in artifacts is prohibited "*inasmuch as* [it] . . . usually results in the loss of context and cultural associations" (137; emphasis added) and therefore compromises the value of archaeological material for scientific, anthropological purposes.

A decade later the question of professional standards was reopened by members of the SAA and some of the major (governmental) employers of archaeologists who shared a concern that as the demand for archaeological expertise in cultural resource management expanded, "the field [was growing] so large that certain segments of the profession were almost unknown to other segments, and . . . the nonacademically associated members of the community were not subject to any form of peer review (other than that of the market)" (McGimsey 1995: 11). In 1974 the SAA formed a Committee on Professional Archaeology, which recommended that the society establish a register of members certified as professional archaeologists and adopt a code of conduct specifying the standards that should govern their training, performance, and managerial practice in various areas of professional activity. Although the SAA membership voted to support the development of such a register and code of conduct, the executive board rejected the proposal on the grounds that it threatened to put the society at risk legally and financially; moreover, such mechanisms of self-regulation would entrench a distinction between

professional and nonprofessional members that would change the character of the SAA. In 1976 members of the Committee on Professional Archaeology established an autonomous Society for Professional Archaeologists (SOPA). Under the rubric of SOPA they established a formal code of ethics, along with a register of professional archaeologists and a formal grievance procedure for reviewing violations of professional standards (McGimsey 1995: 12–13; see also Woodall 1993 [1990]; W. Green 1995).

In the SOPA code of ethics, archaeology is defined, first and foremost, as a profession: "the privilege of professional practice requires professional morality and professional responsibility, as well as professional competence, on the part of each practitioner" (SOPA 1991: 7). The responsibilities of professional archaeologists and the criteria of competence that they must meet are set out in considerable detail and with reference to clients and employers, employees, colleagues, students, the public at large, and the field as a whole (7–8). Regarding "standards of research performance" (9–10), the SOPA code describes the "research archaeologist" as having a responsibility to "design and conduct projects that will add to our understanding of past cultures and/or that will develop better theories, methods, or techniques for interpreting the archaeological record" (9); several clauses in this section mandate that the work of professional archaeologists be informed by a "scientific plan of research," conform to scientific standards of excavation and recording, and be reported to "colleagues and other interested persons" (9–10). But the emphasis throughout is on *professional* responsibilities. An engagement in the scientific enterprise of archaeology—"stay[ing] informed and knowledgeable" about developments in relevant aspects of the field, contributing to the larger goals of scientific archaeology, and respecting scientific standards of research design and practice—underpins but does not exhaust the standards of conduct specified for professional archaeologists.[5]

The SAA subsequently developed a statement on the objectives of the society that was incorporated into its bylaws in the 1980s (SAA 1995). This updated policy specifies no standards of practice but otherwise reiterates many of the central themes of the 1961 "Statements." It emphasizes the orienting commitments that all archaeologists should share: to promote "the archaeology of the American continents," including archaeological research, publication, education, and public interest in archaeology (e.g., as embodied in regional or local archaeological societies). Although these "objectives" include no definition of what constitutes archaeology as a field comparable to that which was central to the earlier "statements," several clauses make it clear that the kind of archaeological practice the SAA advocates and represents is that which contributes to the scientific understanding of past culture: the society as a whole is to "operate for exclusively scientific and educational purposes" (SAA 1995: 17, article II.8) and is committed to "promote and support all legislative, regulatory, and voluntary programs that forbid and discourage all activities that result in the *loss of scientific knowledge and of access to sites and artifacts*" (17; emphasis added). Most striking, given the history of debate that led to the formation of SOPA, these objectives explicitly declare that all archaeologists are united by a shared commitment to the goals of scientific inquiry: the society itself is to "serve as a bond among those interested in American Archaeology, *both professionals and nonprofessionals,* and to aid in directing their efforts *into scientific channels*" (17, article II.4; emphasis added).

It is also significant that the SAA's bylaws include a new theme that did not figure in the 1961 guidelines. The second of nine objectives is to "advocate and . . . aid in the conservation of archaeological resources."[6] The addition of conservationist interests reflects a second development —a crisis of unprecedented proportions, as Lipe described it in 1974—that has been as important in shaping the disciplinary identity of North American archaeology as the pressures to professionalize. Lipe began his influential article, "A Conservation Model for American Archaeology," by observing that "all of us in the archaeological profession are aware of the present crisis in American archaeology precipitated by the growing rate at which sites are being destroyed by [human] activities—construction, vandalism, and looting of antiquities for the market" (1974: 213). At that time, there was already a growing literature projecting that at current rates of development and exploitation, the majority of archaeological resources might well be irrevocably destroyed within a generation.[7] Given this assessment, Lipe argues that

"if our field is to last more than a few more decades," archaeologists must move away from an "exploitative model of utilization of archaeological resources" and embrace a "resource conservation model" (214). He acknowledges that doing so would require a substantial reassessment of disciplinary priorities. Archaeologists would have to make it their primary goal to identify, protect, and conserve archaeological resources "for maximum longevity" rather than to exploit them for immediate (scientific) purposes; they should make every effort to develop nondestructive techniques for documenting archaeological resources, excavating only when there are no other means of addressing crucial research problems and, ideally, when there are no prospects for protecting a site; and they must be actively involved in public education, as well as in planning and resource management "whenever land surface alterations are involved" (223).

Although Lipe is clear about the need to reorient research practice, the rationale he gives for embracing conservationist values remains scientific. The archaeological record is a scientific and anthropological resource that must not be squandered; conservation is desirable not as an end in itself but as a means of ensuring that future archaeologists, who may be in a position to make more effective use of this resource, have sites and materials with which to work.[8] An imperative to pursue scientific, anthropological goals in the short run must be weighed against a longer-term responsibility to ensure that archaeological resources are available in the future. Even so, Lipe insists in a statement that anticipates later proposals for an ethic of stewardship, "a focus on resource conservation leads us to a position of responsibility for the whole resource base" (1974: 214).

This conservation ethic has implications for a number of other aspects of disciplinary identity that are reflected in archaeological statements on ethics. On the one hand, it reinforces the commitment of the SAA to maintain close ties with avocational practitioners and the interested public. From the time that North American archaeologists confronted the crisis described by Lipe, they have acknowledged that conservationist goals will not be realized unless professional archaeologists can engage nonprofessionals in the enterprise of documenting and protecting archaeological sites and materials. On the other hand, the commit-ment to conservation throws into relief a different contrast between archaeologists, whether professional or avocational, and a particular type of non-archaeologist who now takes the place of the nineteenth-century antiquarian as a foil for definitions of the discipline—namely, looters, traders, dealers, "acquisitors" (Pendergast 1991), collectors, and others whose primary interest is in the artifacts themselves, specifically their commercial value. While in its 1961 "statements" the SAA explicitly censured only direct involvement in the antiquities market—"the buying and selling of artifacts" (Champe et al. 1961: 137)—the "objectives" later drafted for the SAA bylaws define a broader commitment to "discourage commercialism in archaeology and to work for its elimination" (SAA 1995: 17, article II.7). In 1991 the SAA adopted an editorial policy for its journals, *Latin American Antiquity* and *American Antiquity,* that further strengthens this opposition to commercial exploitation of the record: "Neither journal will knowingly publish manuscripts that rely on archaeological, ethnographic, or historic-period objects that have been obtained without systematic descriptions of their context; that have been recovered in such a manner as to cause the unscientific destruction of sites or monuments; or that have been exported in violation of the national laws of their country of origin" (SAA 1992: 751).

One aspect of the SAA's editorial policy, the requirement that archaeologists respect legal restrictions on the export and trade of antiquities, is now a standard component of statements on ethics endorsed by archaeological societies in North America.[9] Most support the UNESCO Convention on Cultural Property and condemn practices that violate local laws. The SOPA code of ethics calls on professional archaeologists not only to avoid any form of illegal activity themselves but to refrain from making "exaggerated, misleading, or unwarranted statements about archaeological matters that might induce others to engage in unethical or illegal activity" (SOPA 1991: 7). Substantially stronger statements appear in the ethics statements of two other societies that also represent North American archaeologists, the Society for Historical Archaeology (SHA) and the Archaeological Institute of America (AIA).[10] Both give conservation first priority, although they are also committed to supporting and promoting archaeological research.[11] The AIA is "dedicated to . . . the

protection and preservation of the world's archaeological resources and the information they contain," as well as to the promotion of research and publication (AIA 1991: 285), and the SHA lists the support of research third in an initial statement of objectives that begins with conservation and the "preservation . . . of archaeological resources" (SHA 1992: 36). Consistent with this conservationist orientation, the AIA not only condemns the illegal trade in antiquities but also urges its members to "refrain from activities that enhance the commercial value of such objects" (AIA 1991: 285), while the SHA condemns "the collecting, hoarding, exchanging, buying or selling of archaeological artifacts and research data, *for the purpose of personal satisfaction or financial gain*" (SHA 1992: 36; emphasis added). Both societies also stipulate that their meetings and publications are not to be used as forums for presenting material destined for the market.[12] What the editorial policy of the SAA adds is the requirement that material published in its journals must not have been recovered in such a way as to have caused unscientific destruction of the archaeological record. Although archaeologists differ in their response to and interpretation of these principles, a recent survey of "attitudes and values in archaeological practice" establishes that most North American archaeologists do subscribe to a conservation ethic, broadly defined; the values associated with stewardship have become "a strongly embedded value" (Zimmer, Wilk, and Pyburn 1995: 12).

By the late 1980s, there thus had emerged in North American archaeology a disciplinary identity in which scientific, professional, and conservationist goals are treated as interdependent or even mutually constitutive. Professional archaeologists may not be exclusively dedicated to scholarly interests, but their responsibilities include a commitment to ensure that their work produces information about archaeological resources that supports the goals of scientific archaeology. Although the advocates of a conservation ethic insist that archaeologists may have to forgo some attractive research opportunities in the interest of conserving scarce resources for future use, they do not call into question the long-range goals of archaeological science (Lipe 1996). Indeed it seems widely assumed that although archaeological material should not be treated as having intrinsic

value, to use the language of the 1961 SAA "Statements" (Champe et al. 1961: 137), the systematic, scientific investigation of the archaeological record is intrinsically valuable, serving a nonparochial interest in expanding our knowledge of past cultures. Scientific, scholarly goals are thus assumed to take precedence over the interests of any who exploit archaeological resources for personal gain or for the benefit of a small number of interested parties—any whose use does not enhance, or threatens to diminish, the common store of what is presumed to be humanly significant (scientific) understanding of the past. Looters and commercial salvors, dealers and private collectors, are condemned not just for failing to contribute such knowledge but for destroying the foundations necessary to build it as they seek "personal satisfaction or financial gain" (SHA 1992: 36).

Given this conceptualization of disciplinary goals, North American (anthropological) archaeologists define themselves as authoritative experts on a resource of great public significance that they are best fitted to document, appraise, and exploit. There is considerable tension implicit in adopting a stance as protectors of a scarce and valuable resource while at the same time advocating interests that make archaeologists primary users of that resource. This tension is unlikely to be contentious only so long as two presuppositions can be maintained:

1. Archaeological practice can be clearly distinguished from nonscientific and, increasingly, nonprofessional uses of the record.

2. The scientific goals central to archaeological inquiry can be presumed to yield an understanding of the cultural past that is a common good, that serves humanity or society as a whole.

In recent years it is precisely these assumptions, and the priorities they establish among disciplinary goals, that are being challenged by critics both within and outside the discipline. In what follows I consider two broad categories of challenge—those posed by commercial interests and by descendant communities—that are straining to the limit the disciplinary identity of scientific, anthropological archaeology that underpins the ethics commitments endorsed by the SAA and many of its sister organizations.

PROFESSIONAL VERSUS COMMERCIAL INTERESTS

The conditions that now undermine the sharp oppositional definition of scientific archaeology include, ironically, the very pressures to professionalize that were brought into play by a commitment to conserve and protect archaeological resources. Because of the enormous expansion of employment in CRM, a majority of archaeologists now work for undeniably commercial interests in a variety of settings. And at many junctures they find their commitments to the larger goals of scientific archaeology compromised by the requirements of running a business and meeting the demands of employers and of government regulations.[13] In addition, to the shock and horror of many, it has become increasingly clear that even the purest of academic, scientific research all too often plays into the hands of the market for antiquities. This market has expanded dramatically, especially since the 1980s (see, e.g., Pendergast 1991; Kaiser 1990, 1991), and it is responsible for such massive looting and commercial salvage that these threats to archaeological resources are routinely cited as among the most significant we now face (S. Harrington 1991). As reported by Vitelli, U.S. Customs estimates that "the dollar value of the traffic in smuggled artifacts is second only to that of the traffic in drugs" (1984: 144). Four cases bring into sharp focus the ethical difficulties created or exacerbated by these developments.

CONTENTIOUS CASES

DONNAN AND *NATIONAL GEOGRAPHIC*

An exchange between Donnan and a freelance science writer, Alexander, that began in *Science* in 1990 illustrates how sharply contested are the boundaries between archaeological and commercial interests in the material record. That year Donnan had published, in *National Geographic,* an article describing a spectacular series of royal burials at Sipán, a site representing the Moche culture of coastal Peru at its height (ca. 400–600 C.E.); two years earlier he had published, also in *National Geographic,* an analysis of warrior-priest imagery found on richly decorated Moche grave goods. Both were illustrated with glorious color photographs of Moche artifacts, and the earlier article

prominently featured material held in private collections, some of which was undeniably looted (Donnan 1988: 551–552, and 1990; Alva and Donnan 1993: 27–41; see also Kirkpatrick 1992). Alexander (1990) interviewed the main Lima-based collector, Enrique Poli, who had acquired some of the most spectacular pieces and made no pretense of how it was recovered. Indeed, Poli gloated publicly that Donnan's interest and the *National Geographic* story had confirmed just how important his collection was.

Evidently, Donnan had long maintained connections with private collectors of Moche art. In his 1988 article he draws on the resources of a photographic archive of Moche art held in both private and public collections that he had developed over the previous twenty years (Donnan 1988: 551; see also 1990: 23). By juxtaposing material from this larger assemblage with excavated material—particularly material recovered by Alva, a Peruvian archaeologist who worked on the remaining undisturbed tombs at Sipán (Alva 1990, 1995)—Donnan was able to develop a comparative analysis of the elaborate imagery of Moche ceramics and metalwork that made possible the identification of religious-political roles and leaders in Moche society and the reconstruction, in broad outline, of Moche technology, social and political organization, systems of belief, and ritual practice (Donnan 1988, 1990; Alva and Donnan 1993). Donnan concludes his response to Alexander, "If I had known now what a crucial difference the information [recovered from privately held collections] would make in our ability to accurately reconstruct this ancient society, I would have gone about recording it with even deeper resolve" (1991: 251). As Alexander describes the dilemma posed by Donnan's work, his analysis and publication of looted, privately held material raises the question of whether "archaeologists [should] make use of looted data to increase the body of knowledge, even if that means tacitly justifying looting" or whether, instead, they should "take the high road, shunning all looted objects perhaps at the expense of knowledge lost forever" (1990: 1074).

Donnan responded to Alexander's article with a letter to the editor that was printed in the next issue of *Science* (1991). He was, he said, dismayed by the accusation that what he had done had aided and abetted the international art market whose

lust for Moche antiquities was clearly the main catalyst for the systematic destruction of these coastal sites (Donnan 1991: 498). He protested that he abhors looting and sharply rejected the suggestion that the decision to record looted data should be treated as "the low and unscrupulous road," in contrast with the "high road" of dealing only with scientifically excavated material (498). He described a set of guidelines that he and the editors of the National Geographic had drawn up, specifying what sorts of material could be published in the journal, when he proposed to include photographs of privately held Moche antiquities in his 1988 article. These guidelines require that he respect the UNESCO Convention on Cultural Property and local laws of export and patrimony. The material he published in 1988, while looted, had not been illegally exported from Peru and therefore was not in violation of the UNESCO convention. Moreover, it was held in a "Peruvian collection . . . officially registered with the National Institute of Culture in Peru," in accordance with federal heritage laws (498).

Beyond the defense that he had broken no laws, Donnan offers a positive rationale for making use of looted data that depends on two lines of argument. In his rebuttal to Alexander he formulates what I will call a salvage principle, arguing that archaeologists who refuse to work with looted data abrogate a primary responsibility to document and preserve whatever information survives of the archaeological record that will make a difference to our understanding of the cultural past: "It is tragic that looting takes place, and I know of no archaeologist who does not decry the loss of critical information that results. But to stand by when it is possible to make at least some record of whatever information can still be salvaged simply compounds the loss" (Donnan 1991: 498). In addition, in a statement quoted by Alexander, Donnan insists that professional publication has little impact on the market for antiquities; therefore, prohibitions against publishing looted data are a futile gesture: "Not recording what we can is not going to help. . . . Ninety-nine out of 100 people from hacqueros to collectors wouldn't even know if an archaeologist stopped publishing" (quoted in Alexander 1990: 1075). This is a contentious claim; but even if it were accepted as true for the Moche case, it seems most immediately aimed at critics who object that the publication of looted data causes direct harm, enhancing the value of antiquities and stimulating the market for them. It does not so clearly address the concern Alexander raises about indirect harms: that such publication may tacitly legitimate looting (Alexander 1990: 1075), reinforcing complacency about looting and perhaps compromising the credibility of archaeologists (like Donnan himself) who take a public stand against the commercialization of archaeological resources.[14]

Perhaps Donnan would respond to this objection by citing his own record of activism against looting. According to Kirkpatrick, Donnan played a crucial role in arranging for the protection of the surviving tombs at Sipán and in supporting their excavation by Alva under armed guard (Kirkpatrick 1992; see also Alva 1995); Donnan's 1990 publication in the National Geographic is paired with a report by Alva (1990) on the results of these excavations. In addition, Donnan is described by Kirkpatrick as having made it a priority to educate local communities about the significance of nearby sites and to engage them in the project of protecting archaeological resources.[15] He also played a central role in mounting a high-profile exhibit of Moche culture that includes prominent documentation of the damage done to Moche sites by looters and condemns the antiquities trade that fuels this destruction (Alva and Donnan 1993). Presumably, Donnan believes that these active strategies of opposition are more effective in mobilizing public opinion and (it is hoped) action against commercial exploitation of the archaeological record than a passive refusal to publish looted data; and presumably, too, he believes that they serve to counter any suspicion that in publishing looted data, he condones the practice of looting.

Although many archaeologists find Donnan's position deeply disturbing, the various elements of his response to Alexander are consistent with the statements on ethics set out by the major North American archaeological societies, with the exception of the editorial policy adopted by the SAA in 1992 and related policies of the SHA and AIA. As Donnan notes, he does not violate the terms of the UNESCO convention and local law. In arguing that publication has a negligible effect on the antiquities market he seems mindful of the requirement, central to the ethics policies endorsed by the SAA, the AIA, the SHA, and SOPA, among others, that archaeologists not engage in practices

that stimulate or legitimize the commercial trade in antiquities; he maintains that he is not in violation of injunctions against involvement in the "commercialization" of archaeological resources. Moreover, he can point to ways in which he has actively worked to "direct the efforts" of those interested in Moche antiquities "into scientific channels" and to "discourage commercialism," as required by the SAA's "objectives" and the SOPA code of ethics.[16]

Finally, Donnan's appeal to a salvage principle fits comfortably into the tradition of debate over programmatic and ethical issues in which scientific goals are affirmed as the central methodological and ethical commitment of North American archaeology. He could well have invoked, as the rationale for his salvage principle, the consequentialist wording of the SAA's 1961 "Statements." There, involvement in the "buying and selling of artifacts" is censured because—"*inasmuch as*"—such practice "usually results in the loss of context and cultural associations" (Champe et al. 1961: 137; emphasis added). By focusing on the *consequences* of the trade in artifacts for scientific inquiry, this formulation leaves open the possibility that in circumstances in which the loss of context and associations is not total or does not completely compromise the scientific value of the data, archaeologists may be vindicated in dealing with commercially traded artifacts. Donnan's justification for analyzing and publishing looted data exploits precisely this logic: *given* that information of scientific value can (sometimes) be salvaged, he urges archaeologists to set aside scruples about working with looted and commercially traded data.

When the details of Donnan's defensive arguments are considered, however, it is clear that this general salvage principle is subject to a number of significant restrictions. Donnan's argument requires not only that the data in question have scientific value but also that the market for antiquities not be affected by archaeological publication. He thereby suggests that the *costs* of pursuing the goals of science must be taken into account, not just potential *benefits* to the research enterprise; if publishing looted data in the Moche case is acceptable, it is not only because there is much to gain but also because, in his estimation, there is little to lose. These considerations suggest a doubly conditional salvage principle in which benefits must be systematically weighed against

harms: archaeologists should do what they can to salvage information from looted data *insofar as* it promises to be of scientific value (despite the loss of context and associations), and *insofar as* these interventions do not exacerbate the threat to archaeological resources posed by commercial exploitation (directly or indirectly).

Thus, although a conditional salvage principle of the sort suggested by Donnan may sometimes justify the publication of looted data or other forms of involvement in commercializing processes, by no means does it establish a general warrant for such practices, however central scientific goals may be to the mission of (anthropological, professional) archaeology.[17] Even when the requirement of scientific value (the first condition) can be met, by Donnan's own argument the publication of looted data will not be justified if there is reason to believe that professional publication will enhance the commercial value of antiquities or increase the demand for looted material, compounding the costs of looting that he deplores. The salvage principle Donnan invokes supports his decision to publish Moche data only if his claims about the impact of publication on the antiquities trade hold for the Moche case. By extension, it provides justification for publishing looted data only on a case-by-case basis, when the presumptions of substantial benefit and limited harm can be met. In fact, contra Donnan's crucial assumptions about harm, there is an extensive literature documenting cases that demonstrate just how profoundly academic publishing can affect the commercial value of and trade in antiquities, much of which predates his exchange with Alexander in the pages of *Science* (for an early summary, see R. Ford 1973; also Cook 1991; Herscher 1984; Davis 1986; Elia 1991; Joukowsky 1991; Kaiser 1990, 1991; Vitelli 1981, 1984; E. Green 1984). Two especially poignant examples follow.

BAN CHIANG CERAMICS

In an article published in 1984, Vitelli documents the role played by archaeological publication in creating a highly lucrative market for Ban Chiang ceramics in the 1970s. These Thai materials came to the attention of archaeologists when, by a circuitous route, a sample of Ban Chiang shards was sent to the United States for dating and was found to be surprisingly old; thermoluminescence dates of fifth and fourth millennia B.C.E. (now disputed)

were released by the University Museum Labs at the University of Pennsylvania in 1970 (Vitelli 1984: 145). Vitelli argues that it was the publication of early dates for this site, not just the intrinsic beauty of the material, that drew the attention of collectors. Their attention, in turn, precipitated massive looting that has now virtually destroyed all the sites where these ceramics are known to occur.

Although this is an especially tragic case, it should be noted that the important role played here by archaeological analysis and publication is by no means unusual. The conditions of confidentiality respected by the auction houses that handle the most legitimate forms of trade in antiquities effectively reinforce a dependence on precisely the sorts of scientific authentication and publication that concern Vitelli.[18] It is the rule, rather than the exception, that even when auction houses have information about the circumstances under which antiquities were originally acquired and traded, they hold it in the strictest confidence; as a result, most material is traded without any detailed documentation of provenance or market history.[19] In such cases, comparison with published descriptions, scientific dating, and materials analysis of similar artifacts are often the only grounds on which the authenticity of antiquities and therefore their market value can be assessed. Directly or indirectly, professional publications and appraisals by professional archaeologists and materials analysts—among others on whom dealers and acquisitors rely for authentication—are crucial to the commercialization of archaeological material.[20] Several sharply critical discussions have drawn attention to ways in which the integrity of archaeological assemblages is compromised when, closing the circle, professionals also publish on looted material (e.g., Chippindale 1993; Elia 1993, 1994; Renfrew 1993b; Gill and Chippindale 1993; Chase, Chase, and Topsey 1988).[21]

"THE LOOTING OF ARKANSAS"

A special feature titled "The Looting of Arkansas," published in *Archaeology* within a year of Donnan's exchange with Alexander, includes a disquieting article by Spencer Harrington, who describes the work of Dan Morse, a county archaeologist responsible for protecting archaeological sites in Arkansas (1991). To illustrate how archaeological publication can stimulate the antiquities market even if it treats only material recovered through legitimate excavation, Harrington quotes Morse: "If an archaeologist publishes something about an important artifact—say, end scrapers—then all of a sudden end scrapers become items that are sold . . . and all of a sudden people want them in their collections and bang! end scrapers are selling for five bucks a piece. . . . Every time we publish we aid and abet the market that's costing us our data base" (interview with Morse, quoted in S. Harrington 1991: 28).[22]

In this case, as well as that of the Ban Chiang ceramics, a refusal to publish *looted* data might well be futile as a measure for protecting archaeological resources, but not because professional publication has no impact on the market for archaeological material, as Donnan suggests. It may be the case that the gold foil masks and strikingly beautiful ceramic art of the Moche would find a lucrative contemporary market no matter what archaeologists publish (or refrain from publishing) about its cultural significance, although many are less sanguine than Donnan is on that point (e.g., R. Adams 1991). At the same time, however, there is a wide range of material whose marketability and market value depend heavily and directly on archaeological assessments of its significance. And in these contexts the publication of data recovered from even the most careful scientific excavation may have the negative consequences Donnan deplores. If it is appropriate to weigh the benefits of pursuing immediate scientific goals against the costs of stimulating the market for archaeological material, then worries about consequences may extend well beyond illegally acquired, commercially traded, and destructively looted data. This is by no means a new or original suggestion; policies have long been in place that restrict access to information about site location (e.g., Halsey 1991), and informal discussion suggests that a good many archaeologists are judicious about publishing information not already in circulation that would be of use to looters, dealers, and collectors in locating and marketing antiquities.

A final case illustrates another way in which appeals to a salvage principle push the limits of archaeological wisdom about the boundaries separating professional research practice from commercial interests in the record. If it may be appropriate to publish looted data when the impact of publication on the market is likely to be negligible

and the gains for science substantial, is there a distinction to be made between publishing looted data held in public as opposed to private collections? If it is sometimes acceptable to document material held by dealers and in private collections in order to salvage information that will otherwise be lost to the scientific community, is it appropriate to collaborate in other ways with those responsible for bringing archaeological material to the market? This last is the question raised with particular force by the controversy over the involvement of professional archaeologists in commercial salvage operations like that of the *Whydah* shipwreck; such cases proliferate in the worlds of underwater and historical archaeology (see, e.g., B. Arnold 1978; Bass 1979, 1985; Cummings 1986, 1988; Geisecke 1985; and for a parallel case, G. Miller 1992).

THE *WHYDAH* CONTROVERSY

The *Whydah* galley was sunk off the coast of Cape Cod on April 26, 1717. It was a slave transport that had been captured by the pirate captain Samuel Bellamy in the Caribbean and is evidently the only verified pirate vessel ever discovered in the coastal waters of the United States. As such, it has attracted considerable professional attention as a very significant "early colonial site," worthy of nomination to the U.S. National Register of Historic Places. It lies in the jurisdiction of Massachusetts, a state that does issue permits for legal commercial salvage although it requires compliance with scientific standards of recovery and reporting of the material salvaged. Barry Clifford, of Marine Explorations Inc. (MEI), initiated the commercial salvage of this wreck in the early 1980s; he secured a permit to proceed (though legal challenges were not resolved until 1988; Elia 1992: 106); attracted a large pool of investment capital, initially through MEI and later through a private offering of shares in a newly created venture, the *Whydah* Partners (Elia 1992: 106); and set to work in his inimitable way, hiring Mel Fisher, a professional treasure hunter, as archaeological consultant on the project. To meet the conditions of a memorandum of agreement signed in 1985,[23] Clifford had to enlist professional archaeologists in the survey, testing, and recovery of material from the *Whydah* wreck. Indeed, he was able to attract a series of professional archaeologists to the project who, one after another, said they thought it was worth trying to work with Clifford because the wreck is so significant (Elia 1992: 106–108). In effect, they invoked a version of Donnan's "salvage principle," construed in this case literally as well as figuratively: they were prepared to collaborate in the recovery, not just the postrecovery documentation, of material that was destined for the market in the hope of salvaging scientifically valuable information about the wreck. But one after another they resigned from the project and made strong public statements against the naïveté of ever assuming that the investors and highliving principals involved in commercial salvage will honor a commitment to support the expense of properly scientific documentation and recovery of underwater cultural resources. Critics of the project insist that the financial interests that drive ventures of this sort are inimical to the demands of responsible archaeology.[24]

Hamilton, the one professional archaeologist who stayed with the *Whydah* project, has been at the center of an acrimonious controversy that began when he was barred from presenting a paper on the results of the *Whydah* salvage project at an annual meeting of the SHA in the late 1980s. The ground for this decision by the program committee was an SHA policy that prohibits the presentation of the results of commercial salvage at society meetings, consistent with a more general stance of opposition to commercial archaeological excavation articulated in 1985 (Hamilton 1991; Elia 1992: 108). Hamilton did subsequently present a paper on the *Whydah* controversy at the annual meeting of the SHA in 1991, in a session on ethical issues raised by collaboration with commercial salvors. In this context he too invoked the salvage principle, not only to justify his own involvement with commercial salvage but also to condemn those who had excluded him from the earlier SHA program. In his view archaeologists who close a professional forum to the presentation of valuable data, however acquired, breach their own commitment to the goals of science and their responsibility to ensure the free exchange of information within the scientific community and with the wider public (Hamilton 1991).[25] If, by collaborating with responsible commercial interests, it is possible to save archaeologically useful information about the wreck, why compound the loss that will result when the artifacts are sold at auction and the assemblage dispersed?

Hamilton frequently adds that there are no grounds for systematically distinguishing what he does from the work of any number of other archaeologists involved in legitimate contract archaeology. He insists that the salvage operation in which he was engaged is not fundamentally different from that undertaken by archaeologists employed on CRM projects to recover whatever material they can from sites threatened by, for example, road or pipeline construction. This comparison is made especially contentious when it is acknowledged that U.S. law gives private landowners whose property is transected by such projects the right to claim possession of artifacts recovered in the course of survey or excavation; and they can dispose of these artifacts in any way they please.

Finally, Hamilton urges his colleagues to consider the possibilities for educating commercial salvors—for example, by convincing them to create theme parks that might eventually meet the requirements for a return on investment but would keep the recovered collections together and make them available for more detailed study. This was his ambition for the *Whydah* project; but it was never clear that the commercial partners in the project were willing to commit to permanent curation (rather than sale) of the *Whydah* material, and there now seems little prospect that a proposed pirate theme park will be built around the *Whydah* wreck. Nevertheless, the question remains: Are there no partnerships with commercial interests that might serve the (scientific) purposes of archaeologists? Is it realistic to refuse to consider such partnerships, even if they involve some compromises, given that government bodies and public agencies cannot afford to protect the sites and collections already in their care, let alone fund much primary research in areas as expensive as underwater archaeology? Hamilton's view is that a commitment to scientific goals requires archaeologists to salvage whatever information they can in the face of a rapacious commercial demand for antiquities, and he holds that this requirement may justify not only documenting material recovered by others for commercial purposes but even some forms of direct involvement in the legal recovery of archaeological material destined for the antiquities market.

Critics of Hamilton's appeals to this expanded (and unconditional) version of the salvage principle generally begin by observing that the *Whydah* wreck was not endangered until Clifford got a permit to salvage it, so the claim that commercial exploitation of the wreck is analogous to the practices of CRM is spurious; indeed, it perniciously misrepresents the nature of the case (see, e.g., Ruppe et al. 1986; Elia 1992: 109). Moreover, they argue, in most of the jurisdictions that allow commercial salvage the relevant legal and governmental bodies will not grant a permit for salvage unless a professional archaeologist has agreed to work with the project. Here professional collaboration is a necessary condition for these sites becoming endangered, in the sense of being subject to destructive exploitation by commercial salvors. Why not collectively refuse to make it possible for such operations to get under way?

Finally, Hamilton's critics object that experience has demonstrated time and again that the likelihood of productive collaboration is so slim, given the economic realities of the investment climate in which commercial salvage projects operate, that even the seemingly promising exceptions are not worth the gamble. The indirect costs of participating in commercial exploitation of the record—the loss of credibility for archaeologists who otherwise oppose commercial salvage and the legitimation of commercial salvors and their operations—not to mention the direct costs of destroying an underwater site that was otherwise not in danger, seem just too great to be worth the limited (some would argue nonexistent) returns of collaboration under current conditions. Here it is Hamilton's critics who insist that the salvage principle be conditionalized; they object that if Hamilton is truly committed to the protection and scientific investigation of archaeological resources, he must take seriously the larger negative consequences of his collaboration with commercial salvors.[26]

Cases of this sort are proliferating as the antiquities markets expand and speculative investment continues to grow, and as public funding for scientific archaeology shrinks. They have the effect of throwing into sharp and agonizing relief the tensions between scientific goals and the increasingly urgent demands of an ethic of conservation. No longer can these commitments be assumed to be congruent. On the one hand, the Ban Chiang and Arkansas examples suggest that even the most purely scientific practice may put archaeological resources at risk. And on the other, Donnan and

Hamilton argue that if archaeologists are serious about their scientific commitments, they should be prepared to work with looted data or with those directly involved in profit-making enterprises. Commercial exploitation of the archaeological record is so pervasive and the forces driving it so powerful that it is counterproductive to refuse to salvage what information survives in private collections and material destined for the market.

The common feature of these otherwise quite different cases is that they arise under conditions that make it increasingly difficult to maintain a sharp separation of scientific from nonscientific practice. Whether deliberate or inadvertent, the entanglement of professional with commercial exploitation of the record is inexorably eroding the disciplinary boundaries set by the intentions that determine who will be considered an expert who has both a mandate to exploit the record and a commitment to protect it. Ambiguities abound: in one context, practices that are morally exemplary by conventional wisdom may have deplorable consequences; in another, practices that have been censured, often because of their consequences, may find (limited) justification under the very guidelines that are assumed to prohibit them. It is striking, in fact, that what divides archaeologists is often not so much a conflict over fundamental ethical principles as disagreement on essentially empirical questions about the relationship between archaeological practice and the antiquities market. As Donnan observes, archaeologists on all sides of the controversy over publishing looted data staunchly oppose the commercial trade in antiquities and the destructive looting that feeds it. For the most part they also concur that their central responsibility as archaeologists is to develop as rich an understanding of the cultural past as they can, whether or not they understand these goals in strictly scientific terms. But they differ fundamentally on the question of whether archaeological goals are served by the analysis and publication of looted data, disagreeing on whether or to what extent these practices affect the commercial market responsible for the looting that is so rapidly destroying the richest archaeological sites and resources.

By all accounts, the contexts in which archaeologists practice are now so complex, in all the ways indicated by the cases considered here, that the dilemmas posed by competing commitments will not be resolved by establishing a simple rule for or against certain kinds of controversial practice. Perhaps the way forward is to set aside the categorical imperatives that have generated so much acrimonious debate and focus on procedural directives. Regarding the professional use of looted data, a doubly conditional salvage principle offers a flexible, context-responsive way of sorting through options that inevitably involve compromises. It is, moreover, a principle that might fruitfully be generalized not just to various forms of direct and indirect involvement in the commercial exploitation of archaeological material but also to the insights that lie behind Lipe's much broader conservationist guidelines: any form of archaeological investigation that is potentially destructive is justified only if there is the real prospect that it will contribute significant understanding of the cultural past; if there are no other means of getting the relevant information; and if harm is minimal, or actively minimized, and warranted when weighed against possible gains.

While this kind of procedural principle allows some latitude for developing local solutions, by no means does it legitimate any action based on arbitrary personal preference (see Elia's critique of Hamilton on this point, 1992: 108). The onus is squarely on individual researchers or research teams to substantiate the empirical claims they make about the potential harms as well as the benefits of the course of action they choose, and to publicly justify their weighing of these considerations. Those who endorse the publication of looted data bear the burden of demonstrating, with reference to specific contexts of practice, not only that they are operating within the law and that the data they propose to publish offers insights that cannot be gained by any other means, but also that their use of these data does not in fact put archaeological resources at greater risk of destructive exploitation than they already face. At present, as Fagan (1993) has argued, relatively little systematic analysis has been undertaken of the diverse markets in which archaeological material circulates and of the ways in which archaeological research is entangled with the commercial valuation of and trade in antiquities.[27] If shared (conditional) principles are to be effectively applied, it will be crucial to invest in research that can provide the nuanced empirical understanding of conditions of practice needed for responsible decision making.

Moreover, it is crucial to make the process of deliberation on ethical issues an integral part of archaeological practice—indeed, part of the process of deciding which projects to initiate and how to design and conduct them—not a set of supplementary considerations that arise largely after the fact, and then mainly when things have gone badly.

NONARCHAEOLOGICAL INTEREST GROUPS

While North American archaeologists debate these internal issues, they have also faced powerful challenges from a quite different direction. They have been called to account by nonarchaeological constituencies—most successfully by First Nations and indigenous groups in the Americas and elsewhere, but also by a number of other cultural, ethnic, and religious communities—who consider the archaeological record part of their heritage and do not necessarily regard its scientific exploitation as serving their interests. What follows is a brief survey of stances adopted in this connection.[28] Together, they call into question the second presupposition identified above—that the commitment to scientific goals establishes special justification for archaeological uses of the record—putting considerable pressure on the uneasy alignment of scientific commitments with conservationist values.

Some of those who challenge archaeologists' rights of access to archaeological sites and materials take a strongly conservationist stance and object to destructive use of archaeological resources for any purpose, scientific or otherwise. For example, some First Nations communities argue for the preservation of sacred sites and invoke traditions that closely circumscribe who can visit such sites and what they can do there; this is a central issue in the public debate over appropriate uses of the Black Hills. In other cases, however, traditional practices may call for uses of sites that are not strictly conservationist. In Australia, some aboriginal groups strongly object to any suggestion that rock art sites should be protected as a static archaeological record of a vanished form of life; they regard them as living sites that must be regularly repainted (Bowdler 1988; Creamer 1991; Horton 1987; McBryde 1985; Mowaljarlai et al. 1988). Likewise, some southwestern pueblo groups insist that sacred images be left out in the elements to decay naturally, and some Canadian

tribal groups prefer that the threatened destruction of graves should take its course and does not justify archaeological intervention, even when this destruction is perpetrated by the building of roads and suburbs.[29]

Even when sites are not regarded as sacred in senses that prohibit nontraditional uses of them, descendant communities take a wide range of positions on the question of whether archaeological research is (ever) desirable or acceptable. Many members of the First Nations are willing to collaborate with archaeologists and see archaeological interests in investigating the record as complementary, even essential, to their own cultural and legal interests; in the United States, tribes are now a major employer of archaeologists (Ferguson 1999: 34–36), and they have entered into a wide variety of partnerships to manage resources (e.g., Klesert and Downer 1990; Welch 1997, Kluth 1993, Schwab 1993, and Beck, Nieves Zedeño, and Furlow 1997, all reprinted in Dongoske, Aldenderfer, and Doehner 2000), as part of consultation processes (Swidler et al. 1997: 149–177), to train researchers and educate local communities and the public (B. Mills 1996, Bruseth et al. 1994, and Nicholas 1997, reprinted in Dongoske, Aldenderfer, and Doehner 2000; Bielawski 1982), and to create community heritage centers and museums, archaeological reserves, and archaeotourism (Knecht 1994; A. Ford 1999; Alva 1995; Vargas Arenas 1995; Welch 1997, Stothert 1998, and Brumfiel 1994, reprinted in Dongoske, Aldenderfer, and Doehner 2000). But descendant communities often urge or even require that archaeologists address different questions than those to which they would ordinarily give priority (Deloria 1992); many make a compelling case for taking seriously the integrity of oral history as a resource for understanding the cultural past (e.g., Anyon, Ferguson, Jackson, and Lane 1996; Anyon, Ferguson, Jackson, Lane, and Vicenti 1997; Echo-Hawk 1993, 1997, 2000; Ferguson et al. 1995); and they routinely insist that archaeologists proceed in their research with very different sensibilities than have been typical in the past (see Echo-Hawk 1993, Kelly 1998, Spector 1994, and Bruseth et al. 1994, reprinted in Dongoske, Aldenderfer, and Doehner 2000, and other contributors to that collection; Swidler et al. 1997: 149–177). Spector offers a compelling discussion of the decision not to pursue archaeological excavation

of a suspected dance floor and describes in some detail how, at the same time, her research project was much enriched by ongoing collaboration with direct descendants of the Wahpeton Dakota community that had occupied the contact period site she was investigating (1993: 121–22; 1994).

But in addition, a significant contingent of Native, aboriginal, and other cultural, ethnic, and religious communities are overtly and implacably hostile to archaeological research of any kind (e.g., Deloria 1995; Sanchez 1992). In particular, Native Americans object that archaeology continues a long tradition of cultural and scientific imperialism; they see archaeologists as nothing more than glorified looters. Much archaeological research is undeniably destructive, and this destruction serves what Native Americans regard as the parochial concerns of a narrowly defined interest group, most of whose members have little connection to the cultural heritage they study and who do, in fact, derive financial and other economic and social benefits from their exploitation of the archaeological record.[30] In effect, archaeologists are foxes who have set themselves up to guard the chicken coop: they are primary users of the archaeological record who establish their priority of access to it by claiming that they are properly its guardians. From the point of view of Native American critics, the distinction between archaeological and commercial interests in the record is unsustainable precisely because they reject the second presupposition identified above: they do not regard archaeological investigation as the source of an understanding of the cultural past that has intrinsic value for all people. In legal and practical terms this critical stance has carried the day. Any assumption that archaeologists have priority of access to archaeological resources, or that museums have unconditional rights of ownership of cultural property because they serve society as a whole, has been decisively overturned in most jurisdictions in North America with the establishment of the Native American Graves Protection and Repatriation Act (NAGPRA) in the United States and related legislation in Canada (see Yellowhorn 1996).

While the most powerful of such challenges have come from outside the discipline, some have been articulated internally. In 1973 Elden Johnson argued that the SAA should make "responsibilities to American Indians" a central and defin-

ing commitment of professional archaeological practice (1973: 130), and he identified a number of pressing issues—notably disrespectful treatment of burials and a lack of communication or consultation with Native Americans whose heritage is the subject of archaeological investigation—that have since become pivotal in conflicts over repatriation and reburial. Another striking statement comes from William Adams, a self-avowed "rescue archaeologist" who worked on salvage projects in the Sudan and Egypt for many years. In the early 1980s he argued against the scientism associated with North American archaeology, objecting that while archaeologists have been clear about their responsibility to science and their own discipline, "they do not seem to be aware that they have any other responsibilities" (W. Adams 1984: 11). They betray "a moral myopia not much different from that of the 19th century treasure-seeker[;] . . . both engaged in excavation—which is to say, destruction—of archaeological sites for narrowly defined objectives of their own, disregarding any interests which other scholars, or the lay public, may have in the same sites" (11). He concludes that "in truth, [archaeologists have] many publics with many interests, and most of them are as legitimate as ours": "What price science, then?" (13, 14).[31] Here, Adams shares with the advocates of a conservation ethic a concern that scarce archaeological resources are rapidly being depleted, but he draws much stronger critical conclusions than they do. His demand for public accountability suggests that responsibility "for the whole resource base," to use Lipe's phrase (1974: 214), may require archaeologists to take seriously not just longterm as opposed to immediate scientific goals but a range of *non*scientific interests and goals as well. This willingness to call into question the scientific commitments central to North American archaeology echoes the sharply antipositivist critiques of the New Archaeology that were then beginning to appear in the programmatic literature, though Adams does not explicitly cite them or align himself with any broader critical movement.

Of the societies that drafted codes of conduct in the 1970s and 1980s, only SOPA includes statements that make archaeologists accountable to nonarchaeological interest groups and explicitly acknowledge the legitimacy of a diversity of interests in the record. For example, SOPA requires its members not only to accept responsibility for en-

suring the systematic recovery and public reporting of archaeological material but also to "be sensitive to, and respect the legitimate concerns of, groups whose culture histories are the subjects of archaeological investigations" (SOPA 1991:7). The ethics statements that include more detailed consideration of archaeologists' obligations to nonarchaeological interests were all formulated in the 1990s. Chief among them is the code adopted by the World Archaeology Congress (1991 [1990]), an international organization created in 1986 with a mandate to address the varied political concerns and interests of an enormously diverse membership.[32] The central focus of the WAC code is "members' obligations to indigenous peoples"; it requires archaeologists to seek formal consent from, and to actively consult and collaborate with, any indigenous groups whose heritage is the subject of archaeological investigation. Variants of the WAC code have been adopted by national archaeological societies in Australia and New Zealand (Bulmer 1991; Davidson 1991; Australian Archaeological Association 1994; New Zealand Archaeological Association 1993), and a parallel code has been developed independently by the Canadian Archaeological Association (Canadian Archaeological Association 1997; Zacharias 1994). While the reasons for foregrounding public accountability are very different in the case of SOPA than in WAC and the AAA, CAA, and NZAA, they are indicative of the range of factors that are forcing archaeologists to question the second pivotal assumption identified above: that scientific goals have special status, that they serve humanity as a whole and thus guarantee privileges of access to archaeological resources.[33]

THE MOVE TO AN ETHIC OF STEWARDSHIP

With the proliferation of these conflicts, ambiguities, and challenges, North American archaeologists are now at a critical juncture: they are under strong pressure to reassess the balance struck in the 1970s and 1980s between scientific goals, conservationist commitments, and various forms of accountability. The process of negotiating these issues has far-reaching epistemological implications for the discipline. Appearing at a time when the meaning of a commitment to scientific goals is being rethought more generally, the current eth-

ics debates are one site at which shifting contextual values can be seen to infuse and transform a program of scientific research. Therein lies a complex story of interplay between contextual and constitutive values that I hope to tell in more detail in subsequent analyses of this process of disciplinary transformation. To draw together the threads of this diagnosis of what is at issue in the current debate over ethics issues, I conclude with a brief account of how archaeologists in the SAA are now addressing the tensions I have described.

There is great diversity in the ways North American archaeologists have responded to these issues. Many abhor the restrictions imposed by NAGPRA and related legislation and have adopted a defiantly defensive stance in the face of charges that they are in any sense like looters or should in any way compromise their scientific ideals and goals by making their practice accountable to nonprofessional interest groups (Meighan 1994; G. Clark 1996). At the same time, as demonstrated by contributors to *Working Together: Native Americans and Archaeologists* (Dongoske, Aldenderfer, and Doehner 2000) and to *Native Americans and Archaeologists: Stepping Stones to Common Ground* (Swidler et al. 1997), many take seriously the sea change they are witnessing and recognize in it the potential for productive transformation. They have been active in exploring possibilities for fruitful collaboration with members of the First Nations and other descendant communities, and continue to build connections with avocational archaeologists working in a variety of contexts. Meanwhile, efforts to oppose looting are redoubled and questions about the ethics of collaboration with commercial interests are more contentious than ever. On both fronts, archaeologists are exploring ways to make their research more relevant to various publics and to communicate more effectively what kinds of understanding of the past archaeology can offer that are not accessible by other means and that are irrevocably lost when the record is destroyed by commercial exploitation.

The constructive tenor of these responses to pressures for change characterizes the work of a Committee for Ethics in Archaeology that was created by the SAA in 1991 (it became a standing committee of the SAA in 1996). Its mandate was to review the ethics commitments embodied in the SAA's bylaws and editorial policy. Through a series of workshop and panel discussions and sev-

eral rounds of consultation, this committee has drafted a set of "principles of archaeological ethics" that were adopted by the society in 1996 (see Lynott 1997; Lynott and Wylie 2000). These make stewardship the primary commitment of SAA members. The first of what began as six principles (subsequently expanded to eight) specifies that "it is the responsibility of all archaeologists to work for the long-term conservation and protection of the archaeological record by practicing and promoting stewardship of the archaeological record" (SAA 1996: 451). *Stewards* are defined in this context as "caretakers of and advocates for the archaeological record"; they are expected to "use and advocate use of the archaeological record for the benefit of all people," drawing on their specialized knowledge to "promote public understanding and support for [the] long-term preservation" of archaeological resources (451). Seven additional principles draw out the implications of a commitment to stewardship for specific areas of responsibility: accountability to nonarchaeological groups affected by archaeological research (including, but not limited to, descendant communities who regard the record as their cultural heritage); a commitment to discourage the commercial exploitation of archaeological resources; requirements of respect for intellectual property and for public education and timely publication, ensuring that the results of research are widely accessible; and a responsibility to get the training necessary for competent practice, and to secure the resources and support necessary to preserve archaeological collections and records (see commentaries in Lynott and Wylie 1995, 2000).[34]

There are several points to be made about these "principles" in light of the history of debate described here. One is that the professional status of the SAA and of archaeology as a discipline continues to be ambiguous. The demand for concrete guidelines by which to assess archaeological credentials and performance is an increasingly urgent concern among professional archaeologists, but at the same time, there has never been greater need for effective public outreach and collaboration with avocational archaeologists. Mindful of strong democratizing pressures that continue to counter any impulse to set professional sharply apart from nonprofessional practitioners, the SAA Committee for Ethics in Archaeology followed the precedent set by previous committees; the guidelines for archaeological conduct drafted by the committee in the early 1990s articulate quite general regulative ideals. Thus the principles are deliberately exhortatory; to use a standard phrase, they define ceilings rather than floors for archaeological conduct. At the same time, one outgrowth of the work of the committee was a reopening of negotiations to establish a Register of Professional Archaeologists (ROPA), now with an expanded range of partners: not just SOPA but also the SAA, SHA, and AIA (Lipe and Steponaitis 1998). In 1997 and 1998, the membership of these four societies voted to support the register and the associated code of conduct and grievance procedures that SOPA has maintained since the late 1970s (Niquette 1999; Lees 2000). The result is that each of these societies now endorses a general set of objectives and ethics guidelines, as well as a more rigorous (and enforceable) code of conduct for those of its members who apply for and meet the standards necessary to be registered as professional archaeologists.

As general guidelines, the principles developed by the SAA do not specify how exactly archaeologists should realize the ideals they articulate. For example, they require "adequate training and experience" but do not specify what that means for work in particular areas (e.g., as is set out in the accreditation guidelines for SOPA). SAA members are also expected to publish the results of their research in a timely fashion and to ensure that archaeological records are preserved and made available to others who might want to work with them, but the principles do not indicate what will count as publication or adequate archival conditions. More controversially, the principles impose a strong requirement to consult with those who will be affected by archaeological research, with the aim of establishing working relationships that will be "beneficial to all parties," and they require SAA members to do all they can to discourage and avoid activities that commercialize archaeological material. But again, the questions of whose interests must be considered, what will count as beneficial, and what activities are to be avoided because of their commercial implications remains open.

While their lack of specificity is unsatisfying for those who seek the security of clear-cut rules about what archaeologists can and cannot do, these principles do represent a quite decisive shift in emphasis, with concrete and wide-ranging im-

plications for practice. By making stewardship central, they broaden the scope of archaeological accountability on a number of fronts: they reflect a commitment to take seriously Lipe's insight that "a focus on resource conservation leads us to a position of responsibility for the whole resource base" (1974: 214), and add to it an appreciation that from the perspective of divergent interests in the record, there may be many different ways in which this resource base has value. Scientific goals remain central to the research agenda of most North American archaeologists, but they are not invoked in the principles and are not assumed to take precedence over all other interests in the record. My own view is that archaeologists do, in fact, have a special role to play in the protection, valuation, and use of archaeological resources, by virtue of their scientific interests and expertise. Effective conservation depends on an understanding of the significance of archaeological sites and material. But the significance of a *cultural* resource cannot be defined exclusively in terms of the interests of a particular research discipline; it must be negotiated among as many parties as have a claim on the archaeological record, and most likely must be negotiated locally. As co-stewards of a scarce and irreplaceable resource, archaeologists are accountable to publics who may not share their disciplinary goals. The onus is thus on archaeolo-

gists to explain what their research contributes and to whom, and to take seriously the ways their practice affects others and the archaeological record itself.

Finally, it is clear that the recognition of competing interests central to the principles does more to acknowledge than to resolve tensions between scientific, conservationist, professional, and public responsibilities. While this lack of resolution is, again, unsatisfying for many, it reflects the complexity of the circumstances under which archaeologists typically work. I suspect that there are no simple, generalizable answers to questions about how archaeologists should proceed. They must expand the dimensions on which they conditionalize the salvage principle central to archaeological practice, carefully weighing the benefits and costs of different courses of action under specific circumstances. The open-ended nature of the "principles of archaeological ethics" underscores the need for ongoing deliberation on these matters. And it foregrounds the need to establish the empirical bases necessary for making informed decisions and for integrating this decision making into all aspects of archaeological education and practice. Perhaps the most significant feature of the principles is in setting as the point of departure for deliberation a recognition that values are, indeed, constitutive of scientific understanding.

Notes

1. The account of the field project at Fort Walsh given in this section is adapted from the introduction to a keynote address presented at the 1993 annual meeting of the Society for Historical Archaeology, later published under the title "'Invented Lands/ Discovered Pasts': The Westward Expansion of Myth and History" (Wylie 1993a). Reprinted by permission from *Historical Archaeology* volume 27, number 4, pp. 1–19. © 1992, The Society for Historical Archaeology.

2. For summaries of this history as it informed archaeological investigations at Fort Walsh, see Sciscenti and Murray (1976); Sciscenti et al. (1976); McCullough (1977); Karklins (1987: 1); Klimko et al. (1993). A more detailed historical account is available in Sharp (1973), especially chaps. 4 and 5, "Massacre at Cypress Hills" (55–77) and "Law in Scarlet Tunics" (78–106), and chap. 12, "Sitting Bull and the Queen" (247–267); see also Chambers (1972 [1906]). For popular histories, see McLean (1992: 26–35); Stegner (1962).

3. In a pamphlet titled "Archaeology at Fort Walsh: The Mounties as Pioneers" (ECPS 1981), the presence of an extensive assemblage of alcoholic beverage bottles is described in some detail. They include a range of American and British beer, French cognac, whiskey, wine, and champagne bottles as well as patent medicine bottles (1981: 4), and an intriguing map illustrates the supply routes by which these types of bottle reached Fort Walsh.. The authors conclude that "from the sample of alcoholic beverage bottles found at Fort Walsh, the NWMP might be suspected of breaking the liquor laws they enforced and this applies equally to all ranks" (3).

INTRODUCTION

1. In an assessment of "changing aims and purposes in Americanist archaeology," Sterud notes that "during the last 10 years [1968–1978], when the American-born 'processual' orientation has made its impact, the foremost work, judged by citational occurrence, has been that of the late British scholar, David Clarke (*Analytical Archaeology*)" (1978: 300; see Clarke 1968, 1978).

2. By describing the philosophical influences on the New Archaeologists as analytic, I invoke a distinction between two broad traditions in contemporary philosophy: analytic and Continental philosophy. Although it has long antecedents, this split became entrenched after World War II (see Friedman 1996; Giere and Richardson 1996). Analytic philosophy is generally aligned with a commitment to clear argument and systematic conceptual analysis (sometimes formal), in the tradition of Russell and Moore, the Oxford ordinary language philosophers, and Wittgensteinian philosophical analysis. Continental philosophy is associated with German idealism, phenomenology and its heirs (including existential and hermeneutic

phenomenology), and some forms of critical theory (Habermas). See Radnitzky (1968a, 1968b) for an account of how central issues in philosophy of science have been treated by philosophers working in each of these two traditions. And compare M. Salmon's discussion (1993) of analytic philosophy of archaeology with Embree's account (1989a, 1992: 37, 165) of Continental approaches to philosophy of archaeology.

3. The aspects of logical positivism that influenced the New Archaeologists are outlined in the introductory discussion that follows. For a more detailed account of logical and classical positivism, and its relationship to empiricism, see "Philosophical Interlude" in chapter 1.

4. The influences of Continental philosophers are evident in the work of Hodder (e.g., 1991); Gosden (1994); Johnsen and Olsen (1992); Leone, Potter, and Shackel (1987); Shanks and Tilley (1987); and in contributions to Baker and Thomas (1990), Tilley (1990, 1993), and Preucel (1991b).

5. One such reference, which is especially striking given Hempel's later influence on the New Archaeology, is a passage at the end of "The Function of General Laws in History" where he considers the tacit dependence of archaeological inference on laws—for example, in dating archaeological materials (1942: 48); see n. 30. In the nineteenth century, Whewell discusses "comparative archaeology" as a component of the "palaetiological sciences," the sciences that deal with objects that are descended from "a more ancient condition, from which the present is derived by intelligible causes" (1967 [1847]: 637).

6. See M. Salmon (1993) for an account of the kind of practice that constitutes "analytic philosophy of archaeology," and for a distinction between this and "philosophical approaches to archaeology" (323–327).

7. See, for example, Binford's description of traditional archaeology as exemplified by Griffin (L. Binford 1972a: 3).

8. R. A. Watson is one philosopher who has consistently defended the New Archaeology against its critics. In two early essays (1972, 1976) he endorsed the positivism of the New Archaeology, providing an account of the location of archaeology among the sciences and an analysis of its dependence on laws and background knowledge drawn from a range of collateral disciplines. In so doing, Watson invoked a traditional positivist conception of the sciences as epistemically and methodologically unified. M. Salmon was also sympathetic to the objectives of the New Archaeology but drew on philosophical models of explanation and

confirmation that were being developed in response to critiques of positivist theories. In articles that appeared in *American Antiquity* in 1975 and 1976, she set out a number of distinctions relevant to the archaeological application of philosophical concepts and suggested some alternatives to the Hempelian models. A few years later she published the first monograph on philosophical issues in archaeology (1982), and here she went a good deal beyond clarification. In a more recent overview, Salmon (1993) situates her own work in the larger context of analytic philosophy of archaeology as it developed in the previous twenty-five years.

9. For example, Nickles (1977) made a case for taking seriously the possibility that singular causal explanations may be achieved *without* the benefit of covering laws, as required by Hempel. Rather than using Hempel's models as a standard against which to measure, or reform, archaeology, he urged that these models be assessed in light of what he took to be credible examples of archaeological practice. Levin (1976) likewise developed an account of ascriptions of function to archaeological material that was subsequently critiqued and revised by M. Salmon (1982: 57–82), using a rich store of archaeological examples.

10. One of the central and defining preoccupations of logical positivists/empiricists, from the 1920s on, was to precisely formulate a principle of verification that could serve as a criterion for distinguishing between meaningful statements (scientific knowledge claims) and nonsense (abstract metaphysics, idealism, superstitions, religious beliefs, etc.). The intuition underlying this principle —the cornerstone of logical positivism/empiricism—was that the meaning of a cognitively significant proposition is its means of empirical verification; meaningful propositions are those that can be observationally verified. The fortunes of this principle are outlined by one of its chief proponents, Ayer, in the preface to the second edition of *Language, Truth, and Logic* (1946). Here he observed that "in the ten years that have passed since *Language, Truth and Logic* was first published, I have come to see that the questions with which it deals [especially those having to do with the principle of verification] are not in all respects so simple as it makes them appear" (5). Although he still held that the positivism/empiricism he had espoused is "substantially correct," he reviewed a number of critiques of the "principle of verification" (5–16) that forced him to the conclusion that strict positivist formulations of this principle are untenable; it is not feasible to require that for a sentence to be meaningful, it must be capable of

conclusive verification (135). All that can be re-quired, on a "weakened form of the positivist ver-ification principle," is that some empirical obser-vations should be relevant to the truth or falsity of a "genuinely factual" proposition (136). Thus by the mid-1940s, the central challenges to logical positivism were well developed and widely recog-nized, even by its proponents.

For reasons that include those elaborated by Ayer, Popper describes his own struggle with the issues central to logical positivism as leading him, as early as 1919, to the conclusion that no form of verificationist principle would prove to be feasible and, indeed, that it is fruitless to search for a for-mal criterion of demarcation capable of clearly dis-tinguishing meaningful (cognitively significant) propositions from metaphysical speculation and other forms of nonsense (1989 [1963]: 33). As the self-declared "honorable opposition" to Vienna Circle positivism, Popper shared many of their empiricist presuppositions but insisted on a falsi-ficationist, rather than verificationist, account of the bearing that evidence can have on hypotheses.

By the 1950s and early 1960s, analyses were ap-pearing that extended these earlier challenges to the fundamental tenets of logical positivism/em-piricism: for example, Quine's critique of the ana-lytic:synthetic distinction and the requirements of experiential reduction in "Two Dogmas of Empir-icism" (1951), Feyerabend's critique of formal ac-counts of reduction and explanation(1962), and Putnam's various challenges to the distinctions drawn between theory and observation (1962, 1979 [1962]). The fortunes of logical positivism and empiricism are discussed in more detail in "Philosophical Interlude" in chapter 1. For detailed historical and conceptual overviews of these internal debates, see Scheffler (1963); Suppe (1977a, 1977b, 1977c).

11. This is, in fact, a *return* to engagement with science. Many of the original logical positivists, particularly members of the Vienna Circle, were practicing scientists—social as well as natural scientists and mathematicians—whose analyses were grounded in just such knowledge of scientific practice and its results (see, e.g., Cartright and Cat 1996; Uebel 1991; Giere and Richardson 1996).

12. As Carnap puts the question: "philosophers have ever declared that their problems lie at a different level from the problems of the empirical sciences[;] . . . the question is, however, where one should seek this level" (1934: 5).

13. In particular, Carnap was at pains to show that a great many apparent conflicts of interpretation or metaphysical commitment are artifacts of confu-sion about the proper object of philosophical anal-ysis; they simply disappear if translated into a suit-ably formal mode of expression (1934: 15).

14. Suppes considered analysis of the conceptual foundations of science a distinctively philosophi-cal task because, in his view, "physicists are not well suited to the task of serious research in foun-dations"; they are not sensitive to purely formal or mathematical questions (1954: 243).

15. As Bunge puts this point, philosophers must pro-duce theories of science that "account for scien-tific research . . . [and are] true of it regardless of their philosophical loyalties" (1973: 18).

16. The "grue" to which Feyerabend refers is a fic-tional property that figures in a widely discussed philosophical thought experiment designed (by Goodman) to throw into relief the insecurity in-herent in standard practices of projecting predi-cates. As this thought experiment is typically for-mulated, an emerald is "grue" if and only if it is green and observed before a specified date, or is blue and is not observed until after that date. The problem is to determine what evidence could al-low us to distinguish "grue" from green objects and therefore avoid errors in the projection of ob-served properties at any time before the crucial date (see Goodman 1965). Feyerabend's frustra-tion with these types of philosophical puzzles is palpable. He is scathing in his condemnation of "beautiful but useless formal castles in the air" (1970: 183), and traces the course by which philos-ophy of science turned away from an earlier tra-dition of critical engagement with science (exem-plified by Mach) to what he describes as an arid "conformism" (180), dedicated to the goal of "cor-rectly present[ing] rather than . . . chang[ing] sci-ence" (180–181). He objects that the resulting en-terprise "has nothing to do with what goes on in the sciences. There is not a single discovery in this field (assuming there have been discoveries) that would enable us to attack important scientific problems in a new way or to better understand the manner in which progress was made in the past" (181). In dissociating themselves from the sci-ences, philosophers have lost the opportunity to contribute to the transformation of "scientific pro-cess" (183). Moreover, the sciences themselves have little to gain by "participating" in philosophi-cal analysis; indeed, Feyerabend concludes, "it is much more likely that they will be retarded" by en-gagement with philosophy (181).

17. McMullin objects that all too often, the resulting models "turn out to be nothing more than exer-cises in logic, ingenious and interesting in their own right, occasioned to be sure by the formal

properties of empirical science, but too remote from the thought sequences that constitute 'science' as the practitioners know it to warrant their being called 'philosophy of science' in anything other than an honorific sense" (1970: 14).

18. See also Dupré (1996a) for a discussion of the form and limitation of empirical arguments against unity theses as they arise from analysis of the particularities of diverse sciences.

19. A legacy of logical positivism that social and historical naturalism undermines is the commitment to an implicit asymmetry principle according to which there is a fundamental difference between good, successful science, the course and outcomes of which are to be explained in epistemic terms— by appeal to the dictates of empirical evidence and sound reasoning—and bad science, in which scientists are swayed not by rational, evidential considerations but by intrusive interests, sociopolitical factors that distort the enterprise. On this principle the job of philosophers is to reconstruct scientific rationality when it is working properly and to formalize the conceptual foundations of science, leaving to empirical science studies the task of explaining cases in which this rationality has been subverted. See, for example, Barnes and Bloor's critique of this assumption (1982) and Latour's argument for radically extending their challenge to the asymmetry principle (1993).

20. See, for example, Barnes (1974); Mulkay (1979); contributions to Knorr-Cetina and Mulkay (1983); Latour and Woolgar (1986); discussions and assessments in McMullin (1992).

21. Logical positivists had always acknowledged that the initial processes of discovery and subsequent applications of scientific knowledge are infused by contextual values and interests; it is the systematic evaluation of hypotheses—the context of justification (or verification)—that they insist must be value free, insulated from the influence of idiosyncratic interests and external contextual factors. SSK practitioners challenge the presupposition that the contexts of discovery and justification can be as sharply segregated as this model of practice suggests. They argue that many of the forms of inference, values, considerations, and conventions germane to the process of discovery and to the application of scientific results also play a role in the evaluation of scientific knowledge claims.

22. Hacking holds the view that the constituents of any given stabilization of practice "stand in no necessary or unitary relation to one another" (Pickering 1992a: 8).

23. Tuggle, Townsend, and Riley, all archaeologists, developed one of the earliest philosophical critiques of archaeologists' use of covering law mod-

els of explanation (1972). Sophisticated overviews of the philosophical debates surrounding Hempelian models were published by Kelley and Hanen (1988), an archaeologist and philosopher who have collaborated extensively, and by Gibbon (1989), an archaeologist with substantial philosophical training. M. Salmon, whose first two articles on archaeology provided a philosophical framework for assessing deductivist models (1975, 1976), approached this debate as a philosopher of science and mathematics but drew on a longstanding collegial involvement with archaeologists centrally involved in the New Archaeology. In later work she has addressed foundational questions about theoretical assumptions (e.g., to do with efficiency; M. Salmon 1989) and ethical questions (e.g., M. Salmon 1997, 1999a, 1999b), as well as the epistemic questions on which I focus here.

24. See also the analyses of explanation, mentioned earlier, in which the philosophers Nickles (1977) and Levin (1976) draw heavily on archaeological examples to make the case for models of explanation that, respectively, do not require Hempelian laws and that capture the distinctive logic of attributions of function. Like Salmon, Levin and Nickles approach the archaeological literature as philosophers with a primary interest in contributing to philosophical theories of science, but their analyses are grounded in a consideration of archaeological practice and reflect the postpositivist approach typical of philosophers of science who had taken the first of the two scientizing turns I have described. They thus make use of archaeological examples as a basis for assessing and reframing philosophical models.

25. See chapter 4 for further discussion of the debate about the merits of model-based and law-governed accounts of explanation.

26. B. Smith takes a similar approach, arguing the case for an explicitly inductive approach to hypothesis testing; see chapter 4 for further discussion of his hypothetico-analog model (1977: 609).

27. As Kelley and Hanen put it, "what is at stake, ultimately, is the objectivity of the discipline" (1988: 162). They distinguish between an "old view of objectivity," which imposed unrealistic requirements for value and interest neutrality, and more realistic conceptions that acknowledge degrees of objectivity and require an appraisal of the extent to which knowledge claims offer an understanding of specific phenomena that is not strictly an artifact of "the realities of science as a social enterprise." They conclude that "once we come to understand the socio-political and ideological factors affecting the discipline, we are in a position to take

the next step of evaluating the factors involved with a view to selection, on carefully justified intellectual and moral grounds, of directions to be pursued"; far from undermining ideals of objectivity altogether, systematic sociological analysis "opens the way to a greater objectivity" (162).

28. This conciliatory stance has been advocated by Hodder in connection with a program of "interpretive archaeology" (1991); more recently he characterizes it as a matter of "bridging humanity and science" (1999: 20).

29. Actualistic research includes any empirical study (ethnographic or experimental) designed to provide an understanding of how particular elements of material culture may have been produced or used in a living context (i.e., in actual use).

30. Although Hempel is never directly cited in this connection, he provides direct support for such construal of the role of auxiliaries in archaeological "arguments of relevance." He concludes "The Function of General Laws" (1942) with several examples of circumstances under which historians and archaeologists tacitly rely on laws drawn from collateral (natural science) fields, several of which are archaeological: "The use of tree rings in dating events in history rests on the application of certain biological regularities. Various methods of testing the authenticity of documents, paintings, coins, etc., make use of physical and chemical theories" (47–48). Hempel's argument here is that "even if a historian should propose to restrict his research to 'pure description' of the past, without any attempt at offering explanations or statements about relevance and determination, he would continually have to make use of general laws. . . . [H]e would have to establish his knowledge by indirect methods: by the use of universal hypotheses which connect his present data with those past events" (48).

31. The analysis undertaken by E. Adams and W. Adams, a philosopher and an archaeologist, is particularly interesting because the resulting model arises from close consideration of extended case studies. It is this engagement with practice that forces attention to the complexity of typological practice.

32. For an archaeological counterpart to Kosso's argument that processualists and postprocessualists differ little in their practice, see VanPool and VanPool (1999).

33. In his checklist Bell gives particular emphasis to eliminative strategies of the kind also discussed by Kelley and Hanen (1988). See also Gibbon (1989) and M. Salmon (1982) for assessments of the value and limitations of Popperian refutationism as a model for archaeological practice.

34. Collingwood makes the case that the meaning and credibility of propositions can be grasped only when they are treated as answers to specific questions; they must be understood in terms of what he calls a "logic of question and answer" (1978 [1939]).

35. The term *induction* is often used to describe all forms of argument in which the truth of the premises provides support for but does not establish the necessary truth of the conclusions drawn. It is also used more narrowly to refer to a particular form of inductive argument: that by which a generalization is inferred from observations of particulars. I follow the convention of using the term *ampliative inference* (or *argument*) to refer to inductive inference in the general sense; it extends to all forms of inference in which more is claimed in the conclusion than has been established by the premises that are cited in its support (see chapter 3, n. 13). For an influential indictment of usage that presupposes a "very loose notion of induction," see Peirce (1943: 103).

CHAPTER 1. HOW NEW IS THE NEW ARCHAEOLOGY?

1. See Trigger's discussion of divergent historiographic views about the continuity of archaeological traditions generally, and in connection with recent developments in particular (1989b: 4–12).

2. For example, in his introduction to *Contemporary Archaeology,* Leone uses a Kuhnian framework to characterize the changes undergone by North American archaeology in the previous ten years (represented by contributions to this collection) and to assess the claim that they constitute a decisive break with past practice (1972b: 14).

3. While this contested revolution was in process, several reviews appeared that were designed to assess changes in the topics and perspectives represented in North American publications that might be attributed to the impact of the New Archaeology. Zubrow (1972) undertook a citations analysis of publications that appeared between 1902 and 1970, using a set of categories designed to capture shifts in research interest that reflect the influence of a processual paradigm after the early 1960s: for example, a shift in emphasis to questions about subsistence and environment and society (183–185). He determined that the evidence for any broad reorientation was still indeterminate by 1970 (205), though he identified a noteworthy pattern in the emergence of an interest in subsistence that dates to the early 1950s (201). Sterud undertook a parallel study, published six years later in a special issue of *American Antiquity* on the "chang-

ing aims and purposes [of] Americanist archaeology" (1978; see also Schiffer 1978b). He argued that the fact of "changes in theoretical focus" was "beyond serious question" by the mid-1970s; his aim was, in part, to update Zubrow's study and to determine the degree to which these changes in theoretical orientation had been operationalized (Sterud 1978: 294). He concludes, from the citation patterns he documents, that "serious implementation of the 'new' ideas [which appeared in a few highly influential articles the early years of the 1960s] really began to occur during the last years of the 1960s and early 1970s"; in particular, he notes a substantial shift in the mid-1970s "from a predominantly theoretical focus to a greater reliance on more analytical papers" in which the results of implementation were reported (1978: 299).

4. Meltzer lists together, as advocates of a common cultural paradigm, Krieger (1944) and Willey and Phillips (1958), who explicitly defend a normative conception of culture; Deetz (1967, 1968), who is (uneasily) associated with the New Archaeology but developed a distinctive humanistic approach to historical archaeology; and J. Watson, LeBlanc, and Redman (1971), who are staunch New Archaeology advocates of the materialist ecosystem theory and vehemently oppose anything normative (Meltzer 1979: 653).

5. It seems misguided, however, to treat "revolution" and "[unbroken] linear continuum" as exclusive and exhaustive options for describing intradisciplinary development; see Trigger (1989b: 1–26) on the complexity of the question of what counts as continuity and what counts as (revolutionary) change as it arises for historians of archaeology. Certainly Kuhn's critics object that even the classic instances of scientific revolution with which he deals show continuity that goes unrecognized on his model (see Hacking 1992a: 37–44, and contributions to Lakatos and Musgrave 1970; L. Laudan et al. 1986). At the same time, he has been criticized for postulating a normal state of scientific activity characterized by a degree of internal coherence and continuity that seems rarely realized in actual scientific practice and is considered undesirable, even antithetical to the ideals of critical engagement that many believe should inform scientific practice (see especially Popper 1970; J. W. N. Watkins 1970). The historical inadequacies of Kuhn's account were quickly recognized and have been the focus of much sustained debate; L. Laudan subsequently initiated an empirical research program designed to assess the historical claims made by Kuhn, among others (L. Laudan et al. 1986; see also L. Laudan 1977). Meltzer reviews this critical literature, the bulk of which

had appeared by 1979, and acknowledges that the Kuhnian model is fundamentally flawed if construed, in literal terms, as postulating radically discontinuous revolution as the key mechanism of change and development in the history of science. It is therefore unclear why he would take a variant of it—indeed, a particularly stringent, idealized variant of it—as a useful benchmark for assessing the impact of the New Archaeology. He does make a strong case against claims of radical discontinuity as invoked by the advocates of the New Archaeology. But failure to fit a historically improbable idealization lends no support to Meltzer's final conclusion that there has been an absence of any significant change.

6. I here paraphrase Kluckhohn, who describes one option open to archaeologists as that of approaching research as a series of "sequent phases" (1940: 49); see the discussion later in this chapter in "Divergent Models for Development."

7. A number of internal histories of Americanist archaeology published in the late 1960s and 1970s share Caldwell's and Meggars's assessment, identifying the postwar years as a period in which there was substantial change in archaeological practice: "a technological revolution," as Fitting describes it (1973: 287), or, more ambitiously, the beginning of research distinguished by a "scientific orientation" (Schwartz 1967: 311, 313–314). By contrast, on Willey and Sabloff's influential account, the postwar period was an extension of the prewar "classificatory-historical period," marked by some new "experimental trends" but still dominated by a preoccupation with space-time systematics and a deeply pessimistic conviction that archaeology was unlikely to move beyond its marginal status as "the lesser part of anthropology" (1974: 131). Willey and Sabloff argue that a decisive break with this limited and limiting tradition did not come until the 1960s, though they acknowledge a growing dissatisfaction with current forms of practice and recognize innovative new work on settlement patterns and on culture:environment interactions (133–138, 132), all anticipations of major developments that were to come.

8. In contrast to Meltzer, Caldwell opens his *Science* article by observing that although the refinement of "technical aids" (he cites radiocarbon dating) has had important implications for archaeology, the shift of conceptual framework and thus research interests is, in his estimation, "a more important but less celebrated advance" (1959: 303).

9. I will discuss only the later of these two earlier episodes of debate, which falls at the end of the period in which North American archaeology was professionalized (roughly between 1860 and

1920), but intriguing antecedents arose earlier in this process. See, for example, Patterson's account of the conditions under which professionalization took place (1995b: 46–60), especially his account of how it was reflected in a transformation of archaeological discourse and the elaboration of a body of technical expertise and methodological standards by which the distinction was increasingly sharply drawn between the "academically certified and employed scholar" and amateurs who were being displaced from the field (51–53). As Trigger describes early arguments that gave rise to the "more professional era that was to dawn after 1860" (1989b: 108), they turned on questions that continued to be pivotal long after professional archaeology was established: questions about whether priority should be given to data collection over theorizing, and about the limits of archaeologically based knowledge of the past. On Bieder's account (1986: 108–116), these issues were prefigured, in the 1840s, by E. G. Squier and E. H. Davis's resolution to make a decisive break with the haphazard and speculative practice of their contemporaries and immediate forebears; they described those they criticized as producing "mere collections of odds and ends," fragmentary facts lacking any organization or precision. When Squier and Davis called for a more systematic approach to archaeological research, however, they emphasized the need to avoid speculation of all kinds and, like conservative reformers in the twentieth century, insisted that the first responsibility of archaeologists must be to systematically describe the archaeological record. Meltzer would seem to agree with this assessment, as he is skeptical about the suggestion, attributed to Willey and Sabloff, that Squier and Davis anticipated later practices of hypothesis testing (Meltzer 1998: 51).

As Meltzer describes the dynamic of debate in the nineteenth century, several successive generations defined their archaeological agendas in reaction to their immediate antecedents; and he characterizes Gerard Fowke and Cyrus Thomas, in particular, as "busy plotting [in the 1890s] their own version of the 'New Archaeology' (as Thomas put it), for which [Squier and Davis's] *Ancient Monuments* was a convenient rhetorical foil against which their own work would and must be measured" (1998: 69). Meltzer's detailed analysis of Squier and Davis's *Ancient Monuments* (30, 36, 42–44) suggests that the question of what counted as speculation was a central issue throughout this period.

10. A discussion that parallels that of Dixon in interesting respects but is not referred to in later literature was published by Hewett in 1908: "The Groundwork of American Archaeology" (brought to my attention by Linda Gibbs). Hewett concludes his article with the assertion that "archaeological research is more than the recovery and study of material"; like historians, archaeologists must go beyond the mere "recital of events" and undertake to investigate "their genesis" (1908: 595). To this end Hewett urged archaeologists to expand the scope of their investigations to the "physiographic conditions," which, he held, would prove "essentially correlative with facts of culture" (595). Thus, where Dixon recommends enriching the basis for understanding the archaeological record ethnographically, Hewett recognizes a close interdependence between anthropological understanding and knowledge of the physical, ecological conditions of human cultural life.

11. The language of "multiple working hypotheses" was widely popularized by Chamberlin, in an article that was originally published in *Science* (1890) and reprinted seventy-five years later, after its central tenets were discussed by Platt (1964).

12. These comments were evidently made when Dixon's paper was discussed after he presented it to a meeting of the American Anthropological Association in New York (1913: 566).

13. Wissler notes, "There is no mystery about such work [the work of the 'real, or new archaeology']. It is largely toil, but toil under the direction of a scientific mind" (1917: 100).

14. By the late 1930s there was considerable pressure to move beyond a preoccupation with fact gathering, given the enormous store of unanalyzed archaeological data that had already been accumulated and that was reaching crisis proportions with the advent of federally supported (WPA) relief programs during the Depression (see Patterson 1995b: 73–78). For many the most pressing need, and most obvious next step, was to make these data manageable in descriptive terms. While Steward and Setlzer, and Bennett, among others, share these concerns, they object that if descriptive systematization becomes an end in itself, the residual antiquarian tendencies of the field will simply persist under a new guise.

15. Tallgren registered similar concerns about British and European archaeology in this period: "forms and types, that is, products, have been regarded as more real and alive than the society which created them and whose needs determined these manifestations" (1937: 155).

16. For the nineteenth-century antecedents to this stance, see n. 9.

17. Strong resists what he describes as "Radin's conception of 'history'—a tight little body of written personal records" (1936: 361–362). Historical hy-

potheses based on archaeological evidence "may have only transitory value, yet they rest on the same general principles as more complete hypotheses derived from distributional and sequential facts in other historical sciences" (361).

18. Patterson makes the case that this cautious professionalism embodies not just a generic empiricism but, more specifically, the influence of logical positivists (many of whom had fled the Nazis for the United States and United Kingdom in the 1930s), which was clearly evident in archaeology by the early 1940s: "Logical positivism became the unacknowledged theoretical and ideological perspective of this emerging group of professional archaeologists [government-based and academic]. It accompanied their continuing attempts to create standards that distinguished professional experts from amateurs and to develop uniform terminologies, procedures, and standards for measuring performance. It allowed scholars to focus their attention on methodology rather than content" (1995b: 77).

19. Issues of public accountability and professional responsibility were clearly in the air at the time; see Patterson's account of the steps taken to address these issues by the newly formed Society for American Archaeology (1995b: 74–76), and the retrospective accounts given by McGimsey (1995) and Jelks (1995). But Kluckhohn's arguments in this connection took rather a different turn from those voiced by practicing archaeologists. He quotes Ralph Linton's account of the aims of anthropology—"to discover the limits within which men can be conditioned, and what patterns of social life seem to impose fewest strains on the individual" (Kluckhohn 1940: 43)—and on this basis identifies public interest with academic interests and the latter, in turn, with an interest in social technology. This line of argument is elaborated in particularly candid terms by the editors of *Nature;* in an editorial published in 1940 they described the mandate of a committee, struck by the National Research Council in 1939, "to study the needs of American archaeology" ("Editorial" 1940: 437). It emphasizes throughout the larger political and technical significance of research concerning the "indigenous civilization of the Americas," and urges that standards be established for archaeological research which will ensure that it produces results of practical value: "An academic problem in archaeology [that of reconstructing past lifeways], may have a practical bearing on the affairs of even such a progressive modern community as is found in contemporary American civilization [in the sense that it may provide an understanding

of] such significant subjects as long-continued land utilization, cycles of climactic change and the history of important agricultural crops" (438). The question of whose interests are to be served lies just below the surface of this discussion and brings into play the whole range of the political and economic forces that Trigger (1980, 1989b) and Patterson (1986a, 1986b, 1995b) find at work in archaeology, specifically in this postwar period of transition from historical to processual concerns.

20. This argument about selectivity is prominent in Kluckhohn (1939: 330) and in Steward and Setzler (1938). Kluckhohn observes that "at most, only the first task of scientific research (that of pure description of concrete phenomena) can be performed independently of theory," and even this claim is questionable: "simple description necessarily involves selection out of the vast amorphous body of sense data which impinges upon the consciousness of the observer" (1940: 330). Steward and Setzler ask, sardonically: "When taxonomy and history are thus complete ['when every possible element of culture will have been placed in time and space' and 'the invention, diffusion, mutation, and association of elements will have been determined'], shall we cease our labors and hope that the future Darwin of Anthropology will interpret the great historical scheme that will have been erected?" (1938: 5).

21. In the essay Kluckhohn published in *Philosophy of Science* in 1939, he develops this argument with reference to anthropology generally. He notes a tendency, exemplified by a number of prominent anthropologists of the time, not just to presume a sharp and untenable dichotomy between fact and theory, and to caution against overextended theorizing, but to regard the theoretical as "slightly indecent"; indeed, he observes that " 'theory' . . . tends to be roughly equated with 'speculation' " (1939: 333).

22. Kluckhohn adds that crucial experiments depend on the existence of a theoretical framework within which particular factual results can be understood to have specific significance as evidence for or against a test hypothesis: "no science has prospered until it has defined its fundamental entities," thereby establishing a "small number of categories and elementary relations between them" capable of guiding the observation and systematization of facts (1940: 47).

23. In the standard example of an analytic statement —"all bachelors are unmarried men"—the meaning of the concept *bachelor* is said to be entirely contained in the definitional phrase, "unmarried men." Such a statement cannot be false; to deny

it is to embrace a contradiction. By contrast, the truth of synthetic statements is contingent on factors not established by or contained within the statement itself; e.g., the truth or falsity (or degree of credibility) of the statement "All bachelors live in mansions" depends on questions about where bachelors tend to live that cannot be settled by appeal to the definition of who counts as a bachelor. This distinction has been challenged, famously by Quine, as one of "two dogmas" that have compromised empiricism (1951); see introduction, n. 10.

24. Famously, Hume went so far as to argue for wholesale excision of any body of knowledge that does not meet this stringent criterion of epistemic adequacy: "When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or of school metaphysics, for instance; let us ask, 'Does it contain any abstract reasoning concerning quantity or number' [i.e., any analytic truths]? No. 'Does it contain any experimental reasoning concerning matters of fact and existence?' No. Commit it then to the flames: for it can contain nothing but sophistry and illusion" (1966 [1748]: 184).

25. A focal concern of classical positivists in the late nineteenth century was the question of whether the principles of scientific practice could be extended to human, social subjects. Both Mill and Comte argued that they could; indeed, these philosophers' analyses of scientific practice in other fields was very largely motivated by a concern to extract methodological and epistemological guidelines for establishing various sciences of human nature and society. In order to make a case for extending *positivist* principles of practice to social, human subjects, however, Mill and Comte (among others) had to establish grounds for presuming that this subject domain is indeed law-governed, such that systematic empirical analysis might reasonably be expected to reveal "constant conjunctions." As their correspondence suggests, and as Mill's subsequent commentary on Comte's position makes clear, they differed fundamentally in their assessment of where—at what level of analysis—lawlike regularities might be discovered in the messy affairs of human, social life (Mill 1866; 1969 [1873]). Mill maintained that the laws of human action and social life were to be found at the level of individual psychology, while Comte insisted that they could be discerned only in the large-scale structural features and historical dynamics of social entities considered as integrated wholes. This theoretical disagreement prefigured the epistemological and methodological differences that emerged with increasing clarity as they developed what came to be sharply divergent views about the role of the "method of hypothesis" in establishing psychological, social, and historical laws and about the implications, for the social sciences, of a positivist commitment to eschew speculation beyond observables.

26. "Naturalistic" social sciences are those that model themselves on the natural sciences; naturalists embrace the ambition of realizing, in the social sciences, the goals and standards or forms of practice thought to exemplify the natural sciences. Naturalism in this sense refers to a family of positions that have been articulated within the social sciences (see the history of the formation of the social sciences outlined in Gulbenkian Commission 1996: 1–69) and that have long structured debate in philosophy of social science (see, e.g., the organizational structure of Martin and McIntyre 1994). It is quite distinct from naturalism in the philosophy of science, which is increasingly conceived in inclusive (even nonnaturalist) terms. The turn to ground philosophical analysis in empirical science studies now extends to a wide range of fields, including some (e.g., the history, sociology, and anthropology of science) that naturalizers in the social sciences would not consider naturalistic. For further discussion of naturalism in (and about) the social sciences, see chapter 4.

27. The naturalizing turn in philosophy of science, described in the introduction, reverses the trend of emphasizing language and logic, and in the first instance its subject was distinctly Humean. It was initially associated with a commitment to make more systematic use of the results of research in cognitive science and psychology to better understand the capacities of individual epistemic agents (see, e.g., Goldman 1986, 1999).

28. See introduction, n. 10, and the discussion in chapter 15 of attempts to establish demarcation criteria that capture what distinguishes properly scientific practice.

29. Feminist empiricists have been among the most articulate defenders of liberalized empiricism: see Anderson's discussion of the "modest empiricism" advocated by Longino (1990b), L. Nelson (1990), and other feminist philosophers of science (Anderson 1995a, 1995b). See also the "constructive empiricism" advocated by van Fraassen (1980) in opposition to scientific realism (discussed in the introduction).

30. Here is Kluckhohn's statement in full: "When one reasons by enthymemes [i.e., by means of arguments that depend on suppressed premises] one is proceeding blindly—not by conscious choice between points of departure which (while it may

not prove in the light of facts later available to have been the wisest alternative), is at last [*sic*] patent to the investigator and to others as a choice and hence the more open to detection as a possible fallacy in the argument" (1940: 48). It is this practice that Kluckhohn describes as "a dangerous form of intellectual slovenliness" (1939: 335).

31. A recurrent theme in this literature is that a good deal of accepted practice is in fact compromised by its dependence on commonsense conventions about cultural phenomena—"unanalyzed, far-reaching assumptions" (Kluckhohn 1940: 48)—which have never been clearly articulated, much less systematically assessed, but which seem highly questionable when made explicit. Kluckhohn cites, in this connection, assumptions "as to cultural stability; the mechanics of diffusion; relations of race, language, and culture; poly- and monogenesis; and the like" (48). Bennett makes reference to the tendency, among even the most vehement critics of speculative theorizing, to rely on assumptions about the nature of cultural phenomena and the cultural significance of archaeological data that they endorse simply on the grounds that they are widely accepted (1946: 201). What the critics of theorizing should object to is not, Bennett insists, theory and interpretation per se, but the dependence of archaeological research on hypotheses that are not recognized and systematically tested as such.

    Similar themes are prominent in Tallgren's critique of British and European archaeology, which, he insists, was then in a state of crisis: "archaeology, in spite of its remarkable achievements has got into a cul-de-sac" (1937: 154). Writing immediately before Kluckhohn, Steward and Setzler, and Bennett published their analyses, Tallgren argued that the root of the difficulties he outlined was the unquestioning acceptance by archaeologists of a "stereotyped attitude toward historical and cultural phenomena" (155). For example, he notes that his colleagues widely and mistakenly assume that cultures are uniform, and that "a uniform population or ethnic group [lies] behind cultural phenomena, that is, behind the forms of material culture [with which archaeologists deal]" (156). He regards this assumption as self-evidently untenable, despite having functioned as the foundation for a great deal of archaeological description, classification, and interpretation.

32. Brew extends this endorsement of the method of multiple hypotheses to classification schemes, which he treats as hypotheses (1971 [1946]: 77); see chapter 2. These arguments for considering multiple hypotheses are also clearly prefigured by Wissler (1917) and others who, as indicated above, seem to have been influenced by Chamberlin (1890).

33. The context in which Meggars makes this point is particularly interesting, given the critiques of traditional archaeology that were later central to the programmatic arguments of the New Archaeology: "With the sharp criticisms directed against the cultural theorists of the past still ringing in our ears, we have generally concerned ourselves with sticking close to the facts and proposing conclusions only when they seem to be proved beyond the possibility of contradiction. We tend to feel that when the data are complete . . ." (1955: 126).

34. See Trigger's account (1989b: 194–195) of the racist presuppositions that informed the tradition of culture-historical archaeology to which Wedel and Strong were responding.

35. Indeed, Kluckhohn adds, by way of an "an experiential generalization," that unless evidence is collected for scientific purposes it cannot be expected to be relevant to them: "a focus of interest upon events in their uniqueness . . . is most unlikely to provide that quantitative basis for generalization which is scientifically essential" (1940: 49).

36. Steward subsequently drew back from this strong position. Although he coauthored with Setzler an argument for treating problems about cultural process as the central, unifying objective of anthropology (Steward and Setzler 1938), he later defended the direct historic approach against what he saw as the encroachment of a preoccupation with taxonomy (Steward 1942, 1944). He insists that McKern-type schemes cannot contribute to the historical reconstruction and ethnic identification of archaeological cultures; any classification scheme that is resolutely formal and, therefore, nonhistorical directs attention "away from historical problems which are surely the most important consideration of archaeology" (1942: 339).

CHAPTER 2. THE TYPOLOGY DEBATE

1. See Swartz's discussion (1996) of McKern's "Taxonomic System and Culture Classification" for the historical details of its formation and for comparisons with the other major classificatory systems that were developed in the 1940s and 1950s.

2. McKern thus concludes, in response to Steward's objections, that "taxonomy in archaeology is no more an objective [in itself] than are ethno-historic demonstrations" (1942: 171).

3. Swartz claims that McKern's system is unique in being the only "archaeological culture classification" that ignores spatial and temporal variation (1996: 4).

4. Indeed, Cole and Deuel insist that archaeologists cannot even establish chronological sequences without first developing a formal taxonomy. Once begun, the process of defining typological units that capture inherent formal variability "begins to furnish us materials on which a chronology may perhaps be based" (1937: 200). These units provide the necessary foundation for reconstructing the cultural affinities, contacts, interactions, or developments that might link a prehistoric manifestation with ethnohistorically documented cultural groups.

5. For a closely reasoned assessment of subsequent debate in which questions about the purpose specificity of classification systems, see Cowgill (1990). Cowgill argues that although by the late 1980s it was widely accepted that "different classifications would be better for different purposes," in most publications the focus was still on developing single-purpose classifications (62). He considers three different purposes for which classifications are standardly developed and argues that they reflect not just different selections of focal attributes but different strategies of classification. W. Adams and E. Adams (1991) grapple with these issues, but despite their emphasis on "purpose and practicability" and on the constructed and essentially experimental nature of typologies (1991: 5, 61), I have argued that in their account considerable tension is evident between this constructivism and an assumption that some typological purposes and constructs are foundational relative to others (Wylie 1992b: 488–489).

6. I use the term *constructivist* to refer to those who argued that typological categories and systems are constructs. Those who hold such a position are typically motivated by some form of contextualist argument to the effect that the empirical features of the archaeological record do not determine any unique or fundamental typological systematization. Extending the biological metaphor invoked by McKern and other advocates of formal taxonomic systems, constructivists can be understood to argue that archaeologists cannot assume that the archaeological record has a natural set of joints that will determine how it should be (typologically) carved up, so long as the right analytic tools are developed. Given the empirical underdetermination of typologies by the material they are meant to systematize, other considerations must inform the choices archaeologists make in selecting the traits they will use to define typological units. A philosophical contextualism suggests that these considerations are features of the conceptual context of practice: theoretical presuppositions and a specific problem orientation. Some constructivists move in the direction of a more radical social contextualism; they argue that these choices depend, ultimately, on social conventions or on subjective intuitions. I refer to these forms of constructivism as *conventionist* or *subjectivist*.

7. The references to Phillips and Willey include a two-part series of articles titled "Method and Theory in American Archaeology" published in *American Anthropologist;* the first is authored by Phillips and Willey (1953), the second by Willey and Phillips (1955; see also Phillips 1955). These articles were the basis for a book by Willey and Phillips, *Method and Theory in American Archaeology,* that appeared in 1958. See reviews by Spaulding (1958) and McKern (1956).

8. Here Brew echoes Bennett's insistence that archaeologists should generate more rather than few hypotheses (Bennett 1946: 200), as well as Dixon's argument (1913) for considering multiple hypotheses.

9. For much earlier antecedents of Ford's argument, see Kroeber's classic account, "On the Principle of Order in Civilization as Exemplified by Changes of Fashion" (1919).

10. Ford suggests that even when variability on one or another of these dimensions reveals significant shifts in rate of change, it rarely exhibits sharp discontinuities.

11. As radical as this analysis sounds, Thompson clearly did not see his position as isolated. He cites half a dozen researchers, including Brew and Ford, whose discussions of classification and typology serve to "remind the reader of the widespread acknowledgment of the role which the subjective element plays in this [probative] phase of archaeological reconstruction" (1958: 8).

12. Note that Spaulding is exploiting two distinct lines of argument here. One concerns the presuppositions necessary to get a research enterprise like science off the ground, presuppositions about the reality of the world investigated that, he insists, cannot be questioned without abandoning the enterprise itself. The other concerns more specific features of subject domain that are presupposed as contingent, not necessary, conditions for inquiry. It is a happy (but not inevitable) fact, Spaulding argues, that archaeological data have proven to be more highly structured than Ford suggests.

13. Spaulding later observes that his main objective in this 1953 paper was to "explore techniques for discovering consistent and well defined behavior patterns, and if the techniques actually do what they are supposed to do they cannot fail to yield historically useful units" (1954b: 392).

14. P. Watson, LeBlanc, and Redman endorse just this kind of down-to-earth realism as the metaphysical complement to their positivism:

> we are scientists, not philosophers. We assume that there is a real world that has existed in the past, exists now, and will exist in the future. This world is knowable, and we are capable of understanding it. The world is knowable because the elements of which its objects and events consist, and the objects and events themselves, are related to one another in orderly patterns. We can know the world because we are capable of abstracting and comprehending the patterns and regularities exhibited by the objects and events in the world. And, most importantly, this knowledge is public in the sense that any human being can perceive the world, understand it, and improve knowledge of it through critical discussion and critical comparison with the knowledge accumulated by other human beings. Our knowledge of the world is thus empirical, and the world we know is objective. As scientists, we begin with these assumptions. (1984: 62)

15. Lowther characterizes the position he opposes as the view that there exists a "corpus . . . of basic, existential phenomena usually known as 'fact,'" an empirical foundation "available to the observer but separate from him; in other words . . . [a] given" (1962: 502).

16. Krieger is quite clear that subjective elements must play a role in the initial process of formulating typological categories. He argues that if one used Spaulding's statistical methods of analysis as a method of discovery, one would risk generating different types for every site or assemblage analyzed: "it appears that something else is needed, namely the element of personal experience [or 'prior knowledge'] with the manner in which attributes cluster in time and space perspective" (1960 [1956]: 146).

17. Krieger observes that "without being able to observe first-hand what patterns of manufacture were considered desirable in the culture being studied, it must be admitted that 'types' are arbitrary" (1960 [1956]: 145).

18. Their assertion was amended in 1958 to read "we maintain that all types are likely to possess some degree of correspondence to this kind of reality" (Willey and Phillips 1958: 13).

19. Willey and Phillips note that this passage was revised in response to "a long and exceedingly astringent letter" from Spaulding (Willey and Phillips 1958: 16; see also Spaulding 1958).

20. It is perhaps telling that Krieger notes, in connection with these claims of cultural significance, that

"if analytical methods fail to interpret archaeological material in terms of 'concrete human behaviors,' the historical reconstructions based upon them must be in greater or less degree fictitious" (1944: 371).

21. Elsewhere Taylor objects that his contemporaries had consistently failed to recognize that "results depend at least as much upon the work of their minds as upon that of their spades" (1967 [1948]: 6).

22. Taylor notes that parallel difficulties are evident in the work of anthropologists who, taking up Franz Boas's directive to establish a firm foundation for anthropological understanding of "culture itself" (cultural process and development), had so concentrated their energies "upon collecting data, interrelating them, and synthesizing them into accounts of particular cultural entities" that there had developed among them "a certain disregard for the [central, discipline defining] problems of culture and of cultural process" (1967 [1948]: 37).

23. In fact, when Taylor describes how his "conjunctive approach" works, he reverses the order in which he presents the stages following problem formation; after beginning with problem formation, he moves to the final, anthropological stage, the "study of culture, its nature and workings" (1967 [1948]: 151), and then back through increasingly less ambitious and general levels of inquiry to end with data collection. Presumably his intent is to make clear the sorts of inquiry that archaeological results should ultimately support.

24. By sharp contrast with this highly circumscribed and reductive characterization of archaeology, Bennett has recently described Taylor's account of the autonomous role and status of archaeology as a matter of "defining archaeology as a kind of *metadiscipline*—a field of study within but also between other disciplines, supplementing and sometimes transcending their goals and accomplishments" (1998: 301). He goes on to note that on this account, "archaeology is not any one of [the various things Taylor says it may be: anthropology, history, technical field practice] all the time, but can be any of them depending on context" (301).

25. Taylor holds that insofar as "the archaeologist is a technician concerned with the production of data," archaeologists "should be aware of the concepts and goals of many disciplines" but must not be "restricted in [their] exploration of the site by the dictates of any of them" (1967 [1948]: 153). The archaeologist's main concern, as archaeologist, must be to "transpos[e] the record from the ground to some form, both permanent and available" (154).

26. Taylor equates culture with the ideas, norms, be-

liefs, and other contents of mind that constitute the cumulative tradition that the individuals who bear or participate in a particular culture internalize through a prolonged period of infant dependency (1967 [1948]: 98). See Bennett's treatment of the question of why Taylor embraced this "ideational" concept of culture (1998: 302–303).

CHAPTER 3. THE CONCEPTUAL CORE OF THE NEW ARCHAEOLOGY

1. By contrast, in the mid-1940s Bennett (1946) had been forced to conclude that the transition to a more scientific archaeology had stalled, despite the range of promising initiatives he had described three years earlier (1943a).

2. In Wissler's case, of course, the objective was to make a decisive break with antiquarianism.

3. That is, if archaeological data do not determine a unique conclusion about the antecedent cultural conditions that produced them, then the worry arises that these data could be used to support any number of different (incompatible) generalizations, interpretations, or explanations; there are no clear-cut empirical grounds for choosing among them.

4. The presence of deep-seated skepticism among traditional archaeologists, and its role as a catalyst for the New Archaeology of the 1960s, is remarked on by a number of commentators. See, for example, Klejn's discussion of the skeptical tradition that arose in reaction against the speculative excesses of anthropologists of the prewar period (1977: 3–4) and of the "enthusiasm for caution" that characterized subsequent inquiry (5). Klejn describes this skeptical tradition quite explicitly as "condemning archaeology . . . to a choice between collecting (the 'new antiquarianism') and 'subjective guesses'" (5), clearly recognizing that the traditional archaeology rejected by New Archaeologists embodied not one but two modes of practice, related to one another as dilemmic options in the manner described here. Others have offered similar analyses (e.g., Renfrew 1973b, Glassie 1975, DeBoer and Lathrap 1979), which are discussed in more detail in chapter 4, as well as in part III.

5. See Hill's appraisal of "the methodological debate in archaeology," also published in 1972, for further discussion of those issues. In it he provides a more detailed account of the philosophical presuppositions that frame his analysis, with Evans, of archaeological classification—specifically, the opposition between inductivist and deductivist methodologies and the reasons why "the hypo-

thetico-deductive method is crucial to the advancement of archaeology as a science" (1972: 89).

6. Hill and Evans often conflate these two lines of criticism, attributing to empiricists the view that the empirical foundations of knowledge have inherent (unitary) meaning, but they also recognize that the normative conception of culture to which they object—the theoretical commitments that underwrite specific (traditional) attributions of cultural meaning—is distinct from the epistemic foundationalism they call into question when they argue against the view that archaeological data constitute a stable foundation of empirical givens. They observe, for example, that "there is . . . more to understanding these issues than is implied in our discussion of the empiricist and positivist philosophies of typology" (1972: 260), and then consider the theoretical background that also divides the archaeologists in these camps: their commitments to normative as opposed to systemic theories of culture.

7. Hill and Evans observe that so long as archaeological data are conceived as "basic data" that have "inherent or primary meanings to be discovered" (1972: 231, 252), it will be assumed that archaeologists not only can but must assemble these data before any analysis and interpretation can be attempted. Indeed, on the view they oppose, the ordering of this material, once recovered, is a process of establishing (inferentially) its evidential significance (234).

8. Although there are clear parallels between Hill and Evans's critique of empiricism and the contextualist arguments developed by Kluckhohn (1939, 1940), by Steward and Setzler (1938), and by Bennett (1943a, 1946), these are not cited by Hill and Evans or, indeed, by other advocates of the New Archaeology. Lewis Binford is an exception (1968a); he does cite many of these antecedents in developing his arguments against traditional archaeology and its commitment to a normative theory of culture.

9. Examples of such dissertations include the doctoral research of Hill himself (described in 1966, 1968, 1970), and of Longacre (1964, 1966, 1968; see also contributions to Longacre 1970). Evidently many of the field projects that came to be associated with the New Archaeology were influenced by Deetz's early work on the Arikara (1960, and later described in 1967, 1968; see also Deetz and Dethlefsen 1967) and by Cronin's analyses of southwestern ceramics (1962).

10. By the late 1970s Gumerman and Phillips offer a more optimistic retrospective assessment: "The verbal battles of the late 1960s and early 1970s ap-

pear to have abated, and for the most part, archaeologists feel that the war that existed between the culture historians and the new archaeologists is over and has been won by the latter (Leone 1972[b]; Flannery 1973; Klejn 1977). To be sure, there are still isolated skirmishes, but the concern now seems to be with the quality of archaeology, rather than breast beating over new or traditional" (1978: 184).

11. In the early 1970s, when Hill and Evans's discussion of typology appeared, advocates of the New Archaeology published a number of important programmatic overviews (see especially P. Watson, LeBlanc, and Redman 1971) and several collections representing the breadth of both theoretical and substantive work by New Archaeologists (e.g., Deetz 1971; Leone 1972a; Redman 1973), consolidating the presence of the New Archaeology in North American archaeology. Even the Cambridge-based editor of *Antiquity* felt compelled to "deal with the so-called 'new archaeology'" (editor's introduction to R. Watson 1972: 210) and published a series of articles titled "The 'New Archeology' of the 1960s," which included R. Watson (1972). The editor notes that "there is great talk in America and Britain of the 'new archaeology.' No one talks of this in continental Europe and indeed it is sometimes regarded as an eccentricity of modern American archaeology" (1972: 210). For more systematic assessments of the impact that the New Archaeology was having on Americanist practice, see the citations analyses by Sterud (1978) and Zubrow (1972) described in chapter 1, n. 3.

12. In his review of theoretical developments at the end of the 1970s, Klejn characterizes Binford's role: "Binford led the campaign for the 'new archaeology.' He formulated, in the most clear-cut and operational way, the new ideas in their fullest combination. In addition, he published frequently, rapidly surrounded himself with students and sent them out to battle in droves" (1977: 6).

13. Inductive inference, broadly construed, is at work in any argument in which the conclusion contains more information than is presented in the premises as reasons for accepting the conclusion. Such inference is also often referred to as *ampliative* (see introduction, n. 35): it includes any form of inference in which the conclusions *amplify on* the information given or assumed in its premises. The key feature of ampliative inference is that the premises cannot guarantee the truth of the conclusions; even if the premises are unquestionably true, the conclusions may still be false because they make claims that extend beyond what the premises establish. Sometimes the term *inductive*

is used to designate more narrowly a particular form of ampliative inference, viz., enumerative induction whereby established patterns of events or states of affairs are directly projected onto the future or other unknown contexts. There are many other forms of ampliative inference that involve more complicated extrapolation from the known to the unknown, including analogical inference. When Binford uses the term *inductive inference,* he seems to be referring to the range of forms of inference captured by the less ambiguous term *ampliative inference* inasmuch as he identifies analogical inference, as well as simple inferences of generalization, as inductive.

14. "While agreeing with Binford's goals and recognizing the stimulus he has provided in the 1960s by emphasizing the need for a new outlook, we do not feel that the path he has outlined is the only way to reach the goals he has set. . . . [A]n understanding of historical events can lead to the placement of processual factors in proper perspective rather than the reverse" (Sabloff and Willey 1967: 313).

15. Binford observes, in this context, that unless such explanatory links are established, "we will have achieved only knowledge of the archaeological record itself, which is, of course, a contemporary phenomenon" (1968d: 270–272).

16. Binford earlier defines process comprehensively, as "the dynamic relationships (causes and effects) operative among the components of a system or between systematic components and the environment" (1968d: 269).

17. Contra Binford's most ambitious hopes for a testing methodology, claims about the cultural past are inevitably inductive in the sense that they amplify on any empirical premises that might be produced to support them, whether these be the results of testing or not. The most plausible construal of Binford's claim here seems to be that interpretive or explanatory hypotheses will not be *as strongly* supported by the data when built post hoc to fit these data as they could or would be if the data cited had been recovered in a concerted effort to test them, and if that data proved consistent with empirical expectations derived from these hypotheses. This issue is discussed in part III.

18. See also Cordell and Plog (1979) for an account of "normative thought" as it influenced regional syntheses.

19. Binford adds, in this context, that the reductive approach of traditional archaeology is no more plausible than the presumption that "the functioning of a motor is explainable in terms of a single component, such as gasoline, a battery, or lubricating oil" (1965: 205).

20. There is considerable tension inherent in Binford's argument that normative theories of culture should be rejected in favor of an ecosystem "paradigm" because they are too reductive to capture adequately the complexity of cultural phenomena. Despite acknowledging the role of multiple subsystems—including the "ideotechnic" and "sociotechnic" dimensions of cultural systems (1962)—the ecosystem approach he advocates is highly reductive in ways I describe in later sections of this chapter (see n. 25) and chapter 7.

21. This argument about the bases of Binford's theory is developed in more detail in chapter 7.

22. Elsewhere Binford describes normative theorists (idealists) as assuming that their "field of study [is] the ideational basis for varying ways of human life" (1965: 204), excluding from consideration any of the nonideational factors that may shape these animating ideas, beliefs, conventions, or customs.

23. A decade later, when responding to postprocessual critiques, Binford takes up this line of argument again and develops a much more starkly reductive ecosystem theory than he proposed in his early programmatic articles (1983: 217–221); these later arguments are discussed in more detail in chapter 7.

24. Binford develops these rebuttals (discussed in chapters 7 and 12, above) in several introductions to essays in *Working at Archaeology* (1983), as well as in the articles reprinted in that collection and in *Debating Archaeology* (1989); see, for example, "Objectivity—Explanation—Archaeology—1981" (1982b), "Meaning, Inference, and the Material Record" (1982a), and "Data, Relativism, and Archaeological Science" (1987).

25. These issues are discussed in more detail in chapter 7. The relevant point here is that Binford presupposes a complex set of claims about the causal efficacy of the material dimensions and conditions of human life when he argues that an ecosystem model should be adopted because (only) on this conception of the cultural subject is it tractable for archaeological investigation. He grants ecological factors causal priority in reconstructing and explaining the form and dynamics of cultural systems, and treats mentalistic factors as epiphenomena—as dependent variables that take whatever form is necessary for (or compatible with) effective adaptive response at a systemwide level. In defending an ecosystem model on these grounds Binford adopts a strategy of a priori theorizing dangerously similar to the interpretive arguments from normative theory that he repudiates in traditional archaeology. He delimits, in advance, the range of factors that can be considered causally, explanatorily relevant, in the process compromising an important aspect of his early argument against normative theories: his insistence on the interactive, multivariate complexity of cultural systems. When he argues that the material dimensions and ecological contexts of cultural systems are uniquely reconstructable, he reinstates at the core of his program the principles central to the "ladder of inference" he had so decisively rejected in his early programmatic statements. Finally, despite his distaste for the implication of normative theory that cultural agents are nothing more than the passive bearers of cultural tradition, in later defenses of ecosystem approaches he firmly rejects any form of romantic humanism that presupposes a more robust conception of agency; he asserts the primacy of ecological pushes and pulls in determining human behavior and beliefs in particular, and cultural traditions more generally, whatever our self-conception.

26. In the introduction to the first section of *An Archaeological Perspective,* which includes many of his most influential early articles, Binford notes that he emerged from graduate school with an appreciation that "once one adopted a strategy of 'model building' . . . the epistemological problems of verification loom large" (1972a: 18). It was in this connection that he turned to the philosophical literature on science: "from a practical-science point of view, the arguments of Karl [*sic*] Hempel . . . were the most helpful. Many of the ideas of [Leslie A.] White were presented in explicit analytical form by Hempel" (1972a: 18).

27. The argument sketched here is developed in more detail in chapter 4.

28. See the detailed review of these critiques published by B. Smith just a few years after they appeared (1977: 599–600).

29. The irreducibly inductive component of hypothetico-deductive confirmation is the reason Popper (1959), among others, insists that scientific inquiry can never confirm a hypothesis—at least never an interesting (universal) one. When a test hypothesis is universal in scope and its test implications concern a limited sample of its domain, the most conclusive result that testing can establish is disconfirmation, when evidence subverts the expectations of the hypothesis and produces a counterinstance: a swan that is not white, metal that does not break at the expected stress point, an invasion that does not result in cultural collapse. Finite evidence can never conclusively confirm a universal hypothesis, but it can demonstrate that the claims of the hypothesis do not, in fact, hold for all members of the population that it covers.

The best a strict falsificationist can ever say of a hypothesis is that it has survived rigorous testing; it is not yet falsified (Popper 1959).

This proscription against any claim that nonfalsifying evidence lends support to a hypothesis is generally considered rather extreme. In practice, researchers in all fields of systematic empirical inquiry make closely reasoned and nuanced judgments about the relative degree to which "bold conjectures" (to use Popper's term, 1959) are confirmed by evidence that conforms to the expectations of a hypothesis. A considerable body of philosophical analysis is devoted to explicating the grounds for these judgments (see, e.g., Earman 1983, for a summary of discussions that were unfolding at the same time as the debates about confirmation in archaeology). It presupposes (contra Popper) that evidence that conforms to the expectations of a hypothesis can confer at least some degree of credibility on a test hypothesis, though the simple outlines of the hypothetico-deductive model elaborated by Hempel are generally considered inadequate to capture the complexity of these judgments. In practice, assessments of the credibility of a test hypothesis are comparative (the credibility of rival test hypotheses must be weighed) and depend on considerations of prior plausibility (in light of established bodies of knowledge) as well as of the likelihood that the test in question would turn out as it did whether the test hypotheses were true or not.

30. Similar skeptical worries were made explicit by, among others, M. A. Smith in a British context (1955) and Slotkin in the United States (1952).

31. That is to say, Binford relies heavily on background assumptions and judgments of prior plausibility in just the ways outlined by M. Salmon (1976, 1982) and by B. Smith (1977)

32. To expand on Binford's proposal: if the evidence potentially available to Allchin includes, for example, beads, bone implements, and projectile points of various forms, then the determination of whether discontinuities in their distribution reflect discontinuity in the antecedent cultural traditions will depend on linking principles concerning such matters as the technologies by which artifacts are produced in the media that exemplify the artistic tradition in question and the likelihood that artifacts produced in these media would survive in the archaeological record in the intervening regions where the cultural traditions are not in evidence. The necessary linking principles will likely derive from physical science and chemistry as well as geology of various kinds, and from background knowledge about various ethnohistoric practices associated with these technologies and with the use and deposition of the resulting artifacts. Background knowledge of this kind will be the basis for determining whether similarities in the resulting artifact forms are a function of technical or material constraints on production rather than the influence of shared (or similar) cultural conventions, and whether breaks in the distribution of certain classes of artifact are a function of poor preservation conditions rather than discontinuities in the cultural traditions from which they derive. It cannot be expected that all, or even many, auxiliary hypotheses of these kinds can be established with deductive certainty, much less applied to archaeological contexts with deductive certainty.

33. As my reconstruction of the Allchin case suggests, the strength of the testing procedures proposed by Binford derives from the variety of lines of evidence that may be brought to bear on any given interpretive hypothesis when it concerns cultural events and processes that, on Binford's systemic model, can be assumed to have had diverse material consequences. This is a point made persuasively by M. Salmon (1982) and by B. Smith (1977), and it is central to the analysis of archaeological testing that I develop in part IV.

34. Analysis of these philosophical incongruities has already been developed in some detail (see, e.g., Gibbon 1989; Kelley and Hanen 1988; M. Salmon 1982).

CHAPTER 4. EMERGENT TENSIONS IN THE NEW ARCHAEOLOGY

1. In this context *naturalism* refers the view that the social sciences should be modeled on the natural sciences; for a more detailed definition, see introduction, n. 27.

2. See, for example, Radnitzky's uncompromising critique of attempts to construct the social sciences in the image of the natural sciences as (mis)represented by positivist theories of science. In the end, he argues, they became less like the most successful of the physical sciences than they had been before importing philosophical models: "by becoming—in [Pitirim] Sorokin's wording—testomaniacs and quantoprenetics, they have imitated a pop image of physics which mirrored only the outer shell of physics" (1968a: 145).

3. In making this case, Gibbon (1989) relies on influential analyses of sociology, political science, and geography developed by Hawthorn (1976), Kolakowski (1968), and Bernstein (1976) and cites Harvey (1969). Several more studies have since appeared that detail the development of the "Amer-

ican historical profession" (Novick 1988) and "American social science" (M. C. Smith 1994), and a provocative, forward-looking overview of global scope has been published by the Gulbenkian Commission (1996).

4. See, in particular, Novick's account of the influence of Ranke in the formation of the North American historical profession in the first section of *That Noble Dream* (1988: 26–31).

5. Indeed, historical inquiry has long been a source of provocative puzzles for (philosophical) empiricists and positivists; a strict empiricist/positivist conception of the proper source and content of empirical knowledge seems to entail wholesale skepticism about the possibility of establishing any (genuine) knowledge of the past (e.g., Danto 1965; see also Meiland 1965). Consistently maintained, empiricist commitments call into question even "our right to regard anything as a record of the past" (Ayer 1956: 23, 129; see also Lewis 1946: 334–354, 1956 [1929]); the characterization of a body of data as evidence "of the past" involves substantial inferential extension beyond any observational claims that may be made about (or checked directly against) those data (see Meiland's discussion of historical skepticism, 1965: 4–6).

6. Keat and Urrey describe American social science in similar terms, as having been "largely positivist . . . from the 1930s to the 1960s" (1975: 90). See also Gulbenkian Commission (1996: 33–69).

7. Horowitz, who edited *The New Sociology* (1964), the collection in which Rousseas and Farganis's essay appears, later describes mainstream sociology as dominated by a positivism that arose in the 1940s when sociology "had to choose between humanist and scientist affiliations" (1968: 201). He characterizes the latter as a "new strategy . . . within empirical sociology," distinguished by an "emphasis on observational independence of action, nomological laws apart from real laws, logical stipulation apart from ontological status, and above all, criteria of verification apart from standards of valuation" (198). In striking contrast with the New Archaeologists, Horowitz explicitly identifies positivism as a subspecies of empiricism.

8. In fact, the realist options I refer to were explicitly pro-science; Harré and Secord argued the need to reconceptualize, not abandon, scientific modes of inquiry. They were just as critical of anti-scientific, postmodern, and deconstructive responses to positivism as of positivism itself (Harré 1983: 151–174).

9. Compare, for example, Gumerman and Phillips's cautious but positive internal assessment: "archaeology, at least superficially, seems to have entered

a new age of optimism" (1978: 184); for the continuation of this quotation, see chapter 3, n. 10. Klejn's assessment is described in more detail in chapter 3, n. 12.

10. See, for example, B. Smith's diagnosis of the situation (1977: 599–600). He argues that while critiques of the Hempelian deductivism endorsed by Lewis Binford and other New Archaeologists are well-founded, ultimately Binford (at least) was not so much interested in defending the philosophical models he invokes as in "analyzing the structure of archaeological reasoning" and developing "a rigorous logical method of confirmation that archaeologists could employ in their reasoning" (599). Smith notes that even such outspoken internal critics as Sabloff, Beale, and Kurland (1973) acknowledge this disjunction between Binford's rhetorical allegiance to Hempelian deductivism and what I refer to as his substantive objectives.

11. See, for example, B. Smith (1977), Gibbon (1989), and Kelley and Hanen (1988) for initiatives that draw on alternative philosophical traditions.

12. In advising archaeologists to keep theoretically up-to-date, Aberle warns against "the usual spectacle of the use of ideas outworn in one field as the basic assumptions in another" (1968: 354). The specific objections he raised against the early projects were as follows. In some cases the New Archaeologists contributing to *New Perspectives* had not, in fact, made effective use of the theories they invoke; this was Aberle's critique of Hill (1968) and of Flannery and Coe (1968). In others they had attempted to apply ethnographic concepts to archaeological data that had not yet been effectively operationalized in ethnographic terms; Aberle thus takes to task Whallon's attempt (1968) to measure degrees of "corporateness." And in still others, they had failed to consider alternative or complicating cases which make it clear that the forms of social organization inferred from a particular type of material culture might, in fact, be much more complex than supposed; Aberle cites Deetz (1968) in this connection.

13. See also the parallel critique published several years later by L. Binford: "Although one may offer strong support for the meaningful identification of some observed phenomena, this must remain an exercise or, at worst, a trivial endeavor. . . . Hill's work does not provide us with a scientific context of relevance beyond some functional 'understanding' of pattern variability in the archaeological record. Here Hill's work becomes unclear, since understanding is sought in the absence of theoretical relevance" (1978: 3).

14. Note that P. Watson, LeBlanc, and Redman use the

term *postdiction* for this form of backward-looking inference (1971: 6).

15. Note that although P. Watson, LeBlanc, and Redman substantially broaden their account of scientific explanation in *Archeological Explanation* (1984: 15–36), they are still committed to a "generalized covering law model" (61) and retain the earlier emphasis of *Explanation in Archeology* (1971) on the role of Hempelian laws (both nomological and statistical). In this connection, they affirm the view described here that "archaeologists rarely test the suspected or confirmed general laws upon which their explanations depend" (1984: 11).

16. This objection to the lack of deductive certainty is raised in virtually all the critiques of the deductivist/positivist ideals of the New Archaeology that appeared in the 1970s: M. Salmon (1975, 1976); Sabloff, Beale, and Kurland (1973); B. Smith (1977). And it is acknowledged both explicitly by Read and LeBlanc (1978) and implicitly by J. Fritz and Plog (1970) when they equivocate in their characterization of the covering laws required to support ascriptions of function to archaeological material, as I describe later in this chapter.

17. Earlier in this discussion, Hole argues that whether the point of departure is a body of archaeological data that requires explanation or a hypothesis that needs testing, "we must deal in the first instance with the relations between artifacts and the behavior we are seeking to explain. . . . [T]his *is precisely the point at which archaeology is weakest*" (1973: 25; emphasis in the original).

18. See, for example, contributions to the collections edited by Gould (1978b), Kramer (1979), and Gould and Schiffer (1981).

19. At the same time, Gumerman and Phillips, among others, emphasized the need to expand the range of disciplines on which archaeologists rely as a source of bridging principles and to develop more systematic strategies for making use of these resources (1978: 186, 189).

20. This analysis of arguments of relevance is developed in more detail in chapter 7.

21. With this claim, J. Fritz and Plog not only introduce requirements of content that go substantially beyond what positivist principles would allow, given restrictions on the cognitive content of theoretical claims, but also recognize that postdictive inference will not be secure if covered by a law that establishes only what consequences can be expected to follow from a particular set of initial conditions. As critics (see n. 16) of the deductivist commitments of New Archaeology make clear, the inference from effect to cause is fallacious—it is a matter of affirming the consequent—if the law

covering the inference establishes nothing stronger than a conditional relationship between cause and effect (i.e., that the specified cause is a necessary condition for the effect to occur). Only if the law is biconditional and specifies that the antecedent is also a sufficient condition—that the effect will occur *if and only if* that specified cause obtains—is the retrodictive inference deductively valid. Fritz and Plog recognize this when they qualify their initial treatment of first-level covering laws, arguing that they must show that "one set of phenomena (past behavior) was *sufficient* to produce the second set (the characteristics of an artifact or archaeological feature). . . . [They must] further imply that if the latter did not occur, then the former also did not occur" (1970: 407; emphasis added).

22. Such causalist intuitions are evident even in the most straightforward of replication studies, which, as Coles describes them, have the aim of understanding how and why behavioral, functional, and material attributes are associated: "Copies of simple or complex objects have been made in attempts to emulate the technological processes employed in ancient times, and other copies have been made more rapidly using modern equipment, the aim being to test the functional capabilities of the objects themselves. Some experiments have tried to do both" (1973: 110).

23. See chapter 7 for further discussion of the tensions inherent in the way New Archaeologists conceptualized the goals of actualistic research; there I focus on guidelines for making effective use of archaeological data as evidence rather than the explanatory goals of the enterprise as a whole.

24. See L. Binford's critique of the way Schiffer conceptualizes and proposes to counter this "Pompeii premise" (1981a: 200).

25. Nickles (1977) develops a different line of argument, which does not depend on a systems analysis, for recognizing the possibility of explaining events that are unique and do not fit any discernible regularity: "singular causal explanation."

26. See also, for example, Hole's argument, in response to arguments for a systems approach, that any adequate explanatory theory in archaeology is bound to require "many cover-laws of different magnitude and of different layers in a hierarchy" (1973: 22).

27. Watson, LeBlanc, and Redman's account is, in fact, consistent with Hempel's treatment of the explanation sketch he gives as an example in "The Function of General Laws in History" (1942: 40): that of the migration of dust bowl farmers to California during the Depression. Here any number

28. of smaller-scale covering laws are implicitly at work, having to do with patterns of communication and typical human responses to devastating drought and shortage of resources.

28. At the beginning of *The Principles of Scientific Thinking*, Harré reproduces a line drawing of a "Black and White Swan, habitat South America," prefaced: "An awful warning to those who suppose that 'All swans are white' and its confirmation or falsification by instances exhausts the logic of the laws of nature" (1970: vi).

29. As Hesse puts it, on a formalist account of the prediction of novel phenomena, "it can never be more than a lucky accident that a satisfactory isomorphism is found [between subject domains]" (1966: 46).

30. See chapter 16, and also W. Salmon (1984) and Kitcher and Salmon (1989), for a more detailed account of the models of explanation at issue in the philosophical debates engaged by M. Salmon and Salmon (1979). I am describing here the case Salmon and Salmon make for a causalist model of explanation, as an alternative to covering law models and W. Salmon's own earlier "statistical relevance" model (1971), and in opposition to unificationist and erotetic accounts.

31. See, for example, the range of different theoretical perspectives and problem orientations evident in the contributions to the early collections edited by Clarke, *Models in Archaeology* (1972b); by Renfrew, *The Explanation of Cultural Change: Models in Prehistory* (1973b; see also Renfrew, Rowlands, and Segraves 1982); and later by Hodder, *Simulations in Archaeology* (1978). Although these editors are all British, North American archaeologists (many associated with the New Archaeology) are well represented in their collections; and Clarke, for example, is identified in appraisals of the late 1970s as a highly influential exponent of the kind of systematic, scientific approach to inquiry advocated by the New Archaeology (see Sterud's assessment, quoted in the introduction, n. 1).

32. A further problem noted by Doran and Hodson and central to most subsequent discussions is the "fundamental noisiness" of archaeological data (Aldenderfer 1991: 230), which makes it difficult to assess the empirical adequacy of descriptive and explanatory claims about the cultural past generated by highly precise formal models.

33. Possible exceptions are the most closely connected of direct historic analogs (see chapter 9) and models of an archaeological subject based on nonarchaeological evidence, such as archival sources or oral history, that are rich enough to ensure that the model is effectively homeomorphic.

34. Philosophical interest in scientific models has re-emerged in recent years, in reaction to a new generation of empiricist analyses (see chapter 5). The early realist analyses I describe here rarely figure in these discussions, but the arguments for taking models seriously as a central (nonderivative) feature of scientific inquiry cover much of the same ground. For example, Morrison and Morgan (1999) take exception to a long-standing philosophical tradition in which models are presumed either to be derived, top-down, from theory (as "models of theory" or as applications of theory to real-world systems) or to be built, bottom-up, from the analysis of a specific body of data (as simplified phenomenological descriptions): they are tools for operationalizing theory or for systematizing data. Morrison and Morgan make a case for recognizing that scientific models have much more autonomy, both in both structure and in function, than these standard accounts suggest. In practice, models are rarely constructed in either of these literally derivative ways, and often they put us in a position to learn things about a subject domain that we could not have learned either by direct empirical investigation or by manipulating (testing, refining, extending) an existing theory. I argue that apart from the most narrowly phenomenological models, the models developed by archaeologists are autonomous in both of the senses described by Morrison and Morgan (construction and function), if only by default. There is very little in the way of fully developed theory about cultural systems that archaeologists could deploy in a top-down modeling exercise, and they have only limited empirical access to the cultural subjects of their inquiry on which phenomenological models could be based.

35. In developing this hypothetico-analog account, B. Smith (1977) takes internal critiques of Hempelian deductivism as his point of departure (e.g., Sabloff, Beale, and Kurland 1973), and he draws heavily on M. Salmon's (1975, 1976) and W. Salmon's (1967, 1970) account of the role played in confirmation by assessments of the prior probabilities of alternative hypotheses (assessed in light of background knowledge about relevant reference classes) and probabilistic assessments of evidential significance.

36. Gibbon provides a useful overview of realist theories of science (1989: 143–158) and, for his own account, draws especially on Bhaskar (1978) and Harré (Harré 1970; Harré and Secord 1972), and on summaries provided by Keat and Urrey (1975). At the same time, he observes that he is "neither promoting realism nor supporting the Harré-

Bhaskar realist perspective but merely illustrating one alternative to logical empiricism" (Gibbon 1989: 142).

CHAPTER 5. ARGUMENTS FOR SCIENTIFIC REALISM

1. In a comprehensive review and assessment of these realist debates that considers both their subsequent and previous history, Psillos favors a similar strategy of argument: "going for realism is going for a *philosophical package* which includes a naturalised approach to human knowledge and a belief that the world has an objective natural-kind structure" (1999: xix; emphasis in the original).

2. On standard accounts, three types of realist claim are distinguished: semantic realists hold that theoretical terms should be construed literally as really referring to existing (if unobservable) entities, events, properties, and processes; epistemic realists argue that "to have good reason for holding a theory is to have good reason for holding that the entities postulated by the theory really exists" (Merrill 1980: 229); and metaphysical realists defend a commitment to the existence of a mind-independent reality. Sometimes metaphysical realists emphasize the reality of causal powers (Harré and Madden 1975) or of entities (e.g., the entity realism defended by Hacking 1983), and sometimes they argue the case for the reality of structures generally (Maxwell 1970) or natural-kind structures more specifically (Psillos 1999: xix). Some argue for more open-ended realist commitments; for example, see Dupré's "promiscuous realism" (1996a, 1996b).

3. In a somewhat cynical moment, Putnam describes default arguments as the stock-in-trade of all philosophy: "All philosophers attempt to shift the burden of proof to their opponents. And if one's opponent has the burden of proof, to dispose of his arguments seems a sufficient defense of one's own position" (1978: 18). I argue here that philosophers on both sides of the debate about scientific realism depend on this strategy of argument despite periodically condemning others for resorting to it.

4. Questions about the viability of van Fraassen's observability criterion were among the first and most challenging raised by critics of *The Scientific Image* (1980). See, for example, Foss's critical discussion (1984) and reviews by P. Hanson and Levi (1982), Peacocke (1981), and Friedman (1982: 278–279). A recurrent theme is that the vagueness inherent in van Fraassen's criterion is a more significant problem than he acknowledges, generating a range of counterexamples to undermine his argument that claims about unobservables are candidates for belief as true or false but claims about unobservables can only ever be assessed on instrumental grounds (i.e., they are not true or false, just more or less useful). The counterexamples make it clear that epistemic judgments about the credibility or soundness of knowledge claims do not always track observability in ways that support the categorical difference of epistemic attitude set out by van Fraassen.

   Van Fraassen anticipates the challenge to his observability criterion, proposing an object-based distinction between claims that concern those things that are observable by an unaided act of (human) perception and those that are "only detectable in some more roundabout [mediated] way" (1980: 16). Observability so conceived is, van Fraassen acknowledges, a vague predicate (18); degrees of observability fall along a continuum and may shift as our understanding of human perceptual capabilities change. Ultimately, what counts as observable is a matter to be decided by scientific means: "if there are limits to observation, these are a subject for empirical science, and not for philosophical analysis" (57).

   For a more recent discussion of the difficulties that antirealists face in connection with observables, see R. Miller (1987). He argues that antirealists must either accept the unpalatable conclusion that they must "put chairs on a par with electrons and count nothing as observable but sense data, sensations, qualia, raw feels, or the like," or they must pursue something like the option chosen by van Fraassen, in which case "vacuity is avoided at some cost in arbitrariness in the distinctions [his antirealism] emphasizes" (R. Miller 1987: 361). And for a nuanced account of observability and its implications for scientific realism, see Kosso (1988).

5. As Putnam puts this point, researchers who concern themselves with "radio stars or genes or mesons" typically "do not want theories to obtain predictions[;] . . . [indeed, these] are then not the slightest interest in themselves, but are only of interest because they tend to establish the truth or falsity of some theory" (1971: 72).

6. Van Fraassen develops this line of criticism in a review (1975) of Putnam's *Philosophy of Logic* (1971). *The Scientific Image* also contains discussion of indispensability arguments, both in response to Putnam (van Fraassen 1980: 34–46) and, more generally, in discussion of the "phenomenology of scientific activity" (80–83). In his review of Putnam's *Philosophy of Logic,* van Fraassen invokes an analogy between the leap of faith that realists must make when they resort to indispensability arguments and that undertaken by theists who infer the existence of God from the fact that for them, it is unquestionable "that life is impossible without faith, that the existence of God alone can ground morality and give meaning to life" (1975: 734). He elaborates this analogy in the closing chapter of *The Scientific Image,* "Gentle Polemics" (1980: 204–215; see also 1974).

7. See Brown (1985: 49–66) for an assessment of realist arguments against methodological Darwinism.

8. Rescher argues that this methodological amendment is sufficient to account for the nature and rate of scientific success because methods are inherently general, operating on whole classes of hypotheses. Thus once researchers hit on a method that is success-producing, their capacity to formulate and evaluate scientific knowledge claims increases exponentially in the manner manifested historically (Rescher 1977: 146–166, 1978: 72–74).

9. See, for example, Lugg (1978, 1980), MacKinnon (1972), Shapere (1982), Boyd (1973), and C. Hardin and Rosenberg (1982).

Rescher does accord metaphysical commitments an important role in the development and justification of cognitive methodologies, but these depend on very abstract claims about the active, responsive nature of humans and the uniformity of the natural world that serve primarily to establish a philosophical rationale for treating pragmatic success as a criterion of truthfulness (1977: 81–98). The studies cited above suggest that research methodology depends on much richer, more detailed factual and theoretical presumptions. Lugg demonstrates that the formulation of new theories, as well as debates about their plausibility, is closely constrained by requirements of consistency with the evidence they are meant to cover and also with what we presume to know about related, contributing, or analogous phenomena. Likewise, MacKinnon and Shapere show how background theory underwrites fine-grained and highly discriminating judgments about which postulated entities warrant existential commitment and further investigation as real or, in an attenuated sense, observable (Shapere's concern), and which will be admitted only as heuristically valuable. Within the context of debates over realism, Hardin and Rosenberg argue on the basis of historical analysis that researchers do typically select for theories that expand on or save (are consistent with, incorporate, or correct and explain the limitations of) past theoretical successes. Boyd claims that in fact it is a general methodological principle in science that "new theories should, prima facie, resemble current theories with respect to their accounts of causal relations among theoretical entities" (1973: 8).

10. Scientific realists generally focus on experimental practice, but a parallel argument could be made for field observation, which must be selective and which inevitably depends on theoretical assumptions or hunches about the nature or underlying causal structure of the phenomena observed.

11. Presumably van Fraassen could extend his analysis to all intertheoretic assessments of plausibility. He might even agree that consistency with past theory guides the formulation and selection of new theories, as Boyd claims, but he would then add the caveat that this presumes no more than that theories are perhaps statistically more likely to be fruitful in saving the phenomena if they preserve and expand on the structural resources of existing instrumentally successful theories.

12. In subsequent arguments for scientific realism, a pivotal question has been whether the history of science supports the antirealist thesis that scientific theories show such substantial instability that they cannot plausibly be understood to refer to really existing (but perhaps poorly understood) entities. This historical thesis received canonical formulation in Duhem's early-twentieth-century assessment that while there is steady expansion of the empirical core of scientific knowledge (consisting of observations and the low-level generalizations that underpin classification schemes), what is "sterile and perishable" are attempts to formulate theories that explain this core (1954: 17–19). Among realist rebuttals to this view, P. Smith's (1981) progress-based argument for scientific realism depends largely on the claim that there is sufficient continuity of reference (to key theoreti-

cal entities and structures), despite fundamental changes in theoretical understanding, to warrant a carefully qualified realism. An enriched version of this argument from referential stability is also central to Psillos's arguments for "epistemic optimism" about the existential claims of our best theories (1999: xxiii, 70–182). And it is the basis, in part, for Hacking's endorsement of entity realism (1983, 1992a).

13. Boyd (1983, 1985) cites cases in which researchers rely on untested theoretical hunches or, more important, on conjunctions of independently established hypotheses without testing these conjunctions themselves. This common practice could not be justified unless the individual conjuncts were presumed approximately true of different aspects of a unitary subject reality. There are important discussions of this conjunction objection in two reviews of van Fraassen's *Scientific Images:* Friedman (1982: 280) and Demopoulos (1982: 605).

14. See Friedman (1982: 279–281) for a similar assessment of antirealist treatments of science specifically as embodied in van Fraassen's "constructive empiricism." More recently Kukla, extending a deflationary argument of Fine's (1984, 1986: 112–150), makes a persuasive case for recognizing the inconclusiveness, for both realists and antirealists, of appeals to scientific practice: "all scientific practices are compatible with both realism and instrumentalism. . . . [T]here are no scientific practice arguments on the table that support either side of the debate [to the exclusion of the other]" (Kukla 1994: 955).

15. *Abduction* is the name given by Peirce to ampliative forms of inference by which one reasons from effects to their probable or possible causes: "abduction consists in studying facts and devising a theory to explain them," and it is the originating source of our scientific ideas (Peirce 1934: 90; see also 105–107, 121–127).

16. The appropriate epistemic stance to take with regard to all theoretical claims is agnostic.

17. As a general guideline for translation or interpretation, the principle of charity requires that so far as is possible, you should attribute to others beliefs that are true and rational for them to hold given their epistemic resources. See Quine (1960: 59) for the canonical statement of this principle and Henderson (1993: 13–42) for a recent review of its extensions, revisions, and critiques.

18. This conception of the philosophical enterprise is supported by the science-based argument that the success of scientific practice is a contingent matter; it depends on whether or not science has reached what Boyd calls a "take-off point" (1981:

627) at which the theoretical tradition backing practice incorporates enough approximately true claims about the subject domain to be an effective guide for further investigation. Boyd argues that a parallel metaphilosophical principle holds: philosophy of science should be conducted as an a posteriori study — itself "realistic," causal, and naturalistic — of how theoretical traditions have actually developed and of the dialectical relationship between the beliefs and the mechanisms of belief regulation that constitute these traditions. Beyond this "there will be nothing more to say" (615, 622–625).

19. Boyd argues, in this connection, that realist theories of science enjoy special plausibility because they "rest upon the commonsense, pre-philosophical, realistic understanding of the principles involved [in research practice], and of the reasons why they are justified" (1983: 82).

20. There are some affinities between this argument for taking science as we find it and the "natural ontological attitude" advocated by Fine (1984, 1986). The main difference is that Fine recommends that philosophers abandon both realist and antirealist commitments. As Kukla (1994: 971–973) has argued, however, I believe Fine is unsuccessful in disentangling himself from these commitments and urge a pragmatic commitment to a modest (defeasible) scientific realism.

21. See, for example, R. Miller's characterization of a defensible "piecemeal" scientific realism: "Realism is the view that we are often in a position to make certain existence claims, not that we always are. So it does not exclude isolated indeterminacies" (1987: 364). Miller goes on to argue the case for a realist stance that recognizes a "certain pluralism" in the ontological inventories foundational to many scientific fields (365–367).

22. I have in mind here science-specific analyses that build on the general account of "inference to the best explanation" proposed by Harman (1965) and by Thagard (1978). In addition to the studies of scientific methodology cited earlier, some more general theories of confirmation also display this approach; see especially Glymour's bootstrapping model (1980; see chapter 13 in the present volume) and the kinds of practice-specific considerations that inform many of the more substantive responses to it (e.g., contributions to Earman 1983, and Glymour's replies in 1983a, 1983b).

23. See n. 9 above. C. Hardin and Rosenberg (1982) and P. Smith (1981) have undertaken historical analyses that exemplify this approach.

24. Consider, for example, R. Miller's argument for a realist stance that allows for taking seriously

the claims of some, if not all, competing theories (1987: 371–372).

CHAPTER 6. BETWEEN PHILOSOPHY AND ARCHAEOLOGY

This chapter is based on an article of the same title (Wylie 1985a) that appeared in *American Antiquity* 50 (1985): 478–490. © 1985, Society for American Archaeology. Reprinted by permission of the Society for American Archaeology. It has been revised and includes sections from "The Interpretive Dilemma" (Wylie 1985b), which appeared in *Critical Traditions in Contemporary Archaeology*, edited by Valerie Pinsky and Alison Wylie (Cambridge: Cambridge University Press, 1989), 18–27.

1. "Between Philosophy and Archaeology" was originally written for the fiftieth anniversary issue of *American Antiquity*, edited by P. J. Watson.

2. This proposal was also advanced by Bhaskar in a distinctively realist argument for reframing philosophy of science as a "midwife to the sciences" (1978). For an earlier iteration of this debate in which the primary referent was the relationship between philosophy and social research, see Winch's critique of the deflationary "underlabourer" conception of philosophy: the view, which he attributed to Locke, that "philosophy cannot contribute any positive understanding of the world on its own account: it has the purely negative role of removing impediments to the advance of our understanding" in the form of conceptual confusions (1990: 4). Gibbon provides an excellent overview of dominant conceptions "of the philosopher's task"; these include working as underlaborer, midwife, and system builder dedicated to "preparing the conceptual ground for the edifice of science," as well as more ambitious views of the philosopher as ground-clearer for all knowledge and as "master scientist" (1989: 174–175).

3. Renfrew's sardonic commentary, "Isms of Our Time" (1982b: 8–13), appears in a collection to which F. Plog also contributed an assessment of the role of philosophy, "Is a Little Philosophy (Science?) a Dangerous Thing?" (1982). See also Dunnell's early repudiation of philosophical models of science (1971) and later discussion of why philosophy is generally irrelevant to the practice of archaeology (1989b). Dunnell was one of the first internal critics to assess the New Archaeologists' "search for models"; he urged that archaeologists should "look to the practice and structure of science," not to philosophy of science, for methodological guidelines. The latter, he objects, is "not itself a product of science"; as often as not, philo-

sophical models fail to provide "an accurate reflection of what science does or how it does it" (1971: 3). But despite this repudiation of abstract, second-order philosophical accounts, the model of the "systematics" of science that Dunnell ultimately provides—which he claims is based directly on the actual practice of scientists, uncompromised by "the way or ways in which non-scientists care to rationalize the procedures" (13)—bears all the marks of abstraction and derivative inspiration that he abhors. It is entirely general; Dunnell makes no reference to, and gives no analysis of, any specific examples of scientific theory or practice. And it reproduces, point for point and without attribution, the main outlines of standard logical positivist models of science.

4. P. Watson, LeBlanc, and Redman conclude their first response to Morgan, "as a philosopher of science, he [Morgan] should surely be concerned with aiding the advancement of knowledge by clarifying crucial issues in fields with which he has familiarized himself. However, his discussion betrays such a lack of understanding of the accomplishments and current problems in archaeology . . . that it verges on the irresponsible, and in no small measure constitutes a disservice both to his own discipline and to ours" (1974: 130–131).

5. The locus classicus for this argument is Kuhn's rebuttal to the "incremental progress" view of the development of science (1970: 2); this critique stands, though the details of his alternative account have been sharply contested (especially his claims about revolutionary discontinuities).

6. See, for example, P. Watson, LeBlanc, and Redman's defense of their appeal to philosophical authorities: "we follow the method of practising scientists in a field where knowledge is cumulative: the results of other practitioners are examined and accepted or rejected. . . . If accepted[,] . . . wholesale repetition and restatement of their entire context is not considered necessary" (1974: 129). It is striking that not only do they assimilate philosophy to science but they also accept the model of scientific progress—the "development-by-accumulation" model—associated with the positivist theory of science that they endorse, even though it had been undermined by Kuhnian challenges by the early 1970s.

7. For a detailed rebuttal to these claims, see Wylie (1992e), and R. Watson's reply (1992).

8. As Gibbon puts this point, "philosophy has a role to play in archaeology if for no other reason than that 'substantive' disciplines are by their very nature philosophical pursuits" (1989: 178).

9. The pragmatic or erotetic account of explanation

outlined here is one of three dominant philosophical models of explanation discussed in chapter 16. It is not the model Salmon himself endorses; he holds a causalist view of explanation. I refer to Salmon's treatment of the erotetic model because he draws from it the metaphilosophical conclusions relevant to my discussion of the goals of philosophical analysis.

10. In this spirit Gibbon identifies six ways in which philosophy (or philosophers) can be useful to archaeology, which include the critical functions described here of questioning methodological commitments and raising "awkward questions" about fundamental assumptions, in addition to constructive contributions—refining commonsense concepts and concepts already in use, as well as articulating regulative ideals (1989: 177–178). He then turns to the argument (described in the introduction) that archaeology needs more than philosophical analysis: the questions that arise in practice require the insights and investigative tools afforded by sociological, historical, and anthropological studies of archaeology.

11. As I argued in the introduction, this point has not been lost on those working at the intersection between philosophy and archaeology. Since Schiffer's and Flannery's critiques appeared in the early 1980s a rich body of work has taken shape that is grounded in close analyses of archaeological practice.

12. In fact, the research programs identified as "traditional" by New Archaeologists were by no means as innocent about the presuppositions of their practice as sometimes suggested (see chapters 1 and 2).

CHAPTER 7. THE INTERPRETIVE DILEMMA

This chapter is based on two earlier pieces: a conference paper, "Binford's Second Loss of Innocence," presented at an annual meeting of the Theoretical Archaeology Group (Cardiff, Wales, December 1983), and an essay, "The Interpretive Dilemma" (Wylie 1989b), that appeared in *Critical Traditions in Contemporary Archaeology: Essays in the Philosophy, History, and Socio-politics of Archaeology,* edited by Valerie Pinsky and Alison Wylie (Cambridge: Cambridge University Press, 1989), 18–27.

1. See, for example, the critiques of the early attempts to implement a hypothetico-deductive testing program that appeared in the late 1960s and early 1970s in response to the field studies reported in *New Perspectives in Archaeology* (S. Binford and Binford 1968), discussed in chapter 4.

2. It was this realization that I characterized as Bin-

ford's "second loss of innocence" in the original conference version of this paper.

3. One possible exception is Dunnell (1971, 1982).

4. This is, in essence, a specific instance of the argument described in chapter 4 in connection with the debate about explanatory goals more generally.

5. I make out two possible interpretations of this analogy. One is that it has always been simply self-evident—a matter of common sense recognized by virtually all competent agents in everyday contexts of action—that normativist theory, like flat-earth theory, is false (or would prove to be false if anyone thought it necessary to subject it to empirical test). But in this case, Binford ignores the fact that for many purposes and in most historical, cultural contexts, flat-earth theory and normativism have been anything but self-evidently false. Perhaps Binford intends instead that archaeology, like the earth sciences, is now at a point where it is *no longer* tenable, given the course of archaeological research (or social scientific research more generally), to maintain commitment to any form of normativism. Like the flat-earth theory, normativism is an article of commonsense faith that was formerly unquestioned but has now been decisively proven wrong by more systematic forms of inquiry than everyday life generally requires. Those who persist in endorsing it are stubborn traditionalists, bent on obstructing the progress of science. But this is by no means true; in fact, programs of research predicated on various tenets of normativism are still thriving in sociocultural anthropology, social psychology, and qualitative sociology, for example. More to the point, when Binford invokes self-evident truth to settle a dispute between competing theories within a field of research, he ignores the implications of his own Kuhnian arguments. In the context of an intradisciplinary dispute about the nature of the subject of inquiry, self-evidence is a paradigm-specific accomplishment. Binford's strategy of argument at these junctures reinforces the suspicion that the conflict between eco-materialists and normativists reflects precisely the kind of "locked in" paradigm dependence he means to repudiate.

6. See chapters 11 and 14 for a more detailed account of the role played by an assessment of the security of linking principles in stabilizing evidential claims.

The problem with strictly biophysical linking principles is that on their own, they are extremely limiting; they may allow secure reconstruction of the material conditions under which the archaeological record was produced, but provide little understanding of how these conditions were realized

or manipulated by human agents operating in a cultural context. To reiterate an earlier line of argument: unless it can be assumed that all archaeologically interesting aspects of cultural life are determined by biophysical conditions, these principles do not ground any very rich ascription of cultural significance to archaeological data. In particular, they do not solve the problems that originally motivated Binford, among other New Archaeologists, to rethink their early confidence in archaeological testing: the problems of interpretation that arose in connection with the New Archaeologists' ascriptions of function and reconstruction of social, organizational structures (see the discussion in chapter 4 of the debate about early attempts to implement Binfordian principles published in *New Perspectives in Archaeology*, S. Binford and Binford 1968).

7. See chapters 12 and 13 for further discussion of this property of "vertical independence."

There is an irony in Binford's special endorsement of biophysical linking principles in contexts in which he advocates an eco-materialist paradigm as the only viable ground for scientific practice in archaeology. If archaeological thinking about the cultural past had been informed exclusively and pervasively by biophysical theories (e.g., about adaptive strategies, niche exploitation, population dynamics), the independence Binford prizes would be especially vulnerable to compromise because the linking principles used to interpret the archaeological data as evidence would also have been drawn from the biophysical sciences. On the principle of independence Binford endorses, the evidence for testing hypotheses derived from an eco-materialist paradigm will be most compelling if it is interpreted using linking principles that derive from very different (e.g., sociocultural) sources.

8. This point is elaborated in terms of "bootstrapping" theories of scientific confirmation in chapter 13, and in connection with postprocessual critiques in chapters 12 and 15.

9. See chapter 3 for a more detailed account of Binford's debate with Bordes.

10. To recapitulate the discussion of chapter 3, on Binford's account Mousterian assemblages had been understood to have been produced by distinct cultural groups characterized by stable constellations of material attributes, like those familiar from contemporary European contexts. The anomalous variability that attracted his attention was, he argued, more likely the result of functionally different uses of sites than their occupation by culturally different groups of people.

11. This is a second locus of realist commitment in Binford's early programmatic statements; see chapter 4 for discussion of the realist intuitions implicit in his proposal of a modeling approach to archaeological explanation.

CHAPTER 8. EPISTEMOLOGICAL ISSUES RAISED BY SYMBOLIC AND STRUCTURALIST ARCHAEOLOGY

This body of this chapter was originally published as "Epistemological Issues Raised by a Structuralist Archaeology" (Wylie 1982b) in *Symbolic and Structuralist Archaeology*, edited by Ian Hodder (Cambridge University Press, Cambridge, 1982, pp. 39–46). Reprinted with permission of the publisher. I have revised the original and incorporated into it unpublished material from "Positivism and the New Archaeology" (Wylie 1982c: 299–333).

CHAPTER 9. THE REACTION AGAINST ANALOGY

The body of this chapter originally appeared as "The Reaction against Analogy" (Wylie 1985c), an expansion of "Analogical Inference in Archaeology" (Wylie 1980); it has been revised and incorporates sections that appeared in Wylie (1998b), "'Simple' Analogy and the Role of Relevance Assumptions: Implications of Archaeological Practice," *International Studies in the Philosophy of Science* 2.2 (1988): 134–150.

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1. Sollas's lectures, originally delivered in 1906, were published as *Ancient Hunters* in 1911; several editions followed. The 1924 edition cited here was the third.

2. Clark (1951) uses the term *genetic* in this connection to refer to historical connections as well as descent relations. When Sollas posits such connections between prehistoric subjects and ethnographic sources, he invokes a relationship of homology rather than of analogy.

3. Sollas makes the ideological commitments that underpin this projection of present onto past fully explicit when he responds to the question "what part [of this history] is to be assigned to justice in the government of human affairs?":

So far as the facts are clear they teach in no equivocal terms that there is no right which is not founded on might. Justice belongs to the strong, and has been meted out to each race according to its strength; each has received as much justice as it deserved. What is perhaps most impressive in each of the cases we have discussed is this, that the dispossession by a new-comer of a race already in occupation of the soil has marked an upward step in the intellectual progress of mankind. It is not priority of occupation, but the power to utilise, which establishes a claim to the land. Hence it is a duty which every race owes to itself, and to the human family as well, to cultivate by every possible means its own strength: directly it falls behind in the regard it pays to this duty, whether in art or in science, in breeding or organisation for self-defence, it incurs a penalty which Natural Selection, the stern but beneficent tyrant of the organic world, will assuredly extract, and that speedily, to the full. (1924: 599–600)

4. Ascher's three suggestions, "sketched to aid in placing analogy on a firmer foundation" (1961: 322), are as follows:

   1. Systematically select an analog that represents the "best solution" to the interpretive problem by a process of elimination.
   2. Make better use of existing ethnographic sources, and develop a body of ethnographic specifically relevant to archaeological interpretation.
   3. Give up the entrenched assumption that "a fast distinction [can be drawn] between the ongoing and the extinct, the living and the dead." (323, 324)

   Ascher argues, in connection with this last suggestion, that every living community is in the process of "becoming . . . archaeological data," and archaeological sites are themselves undergoing a continuing process of decomposition. Consequently, archaeologists who study the cultural past and sociocultural anthropologists have much more in common than is typically acknowledged: "the observational fields of ethnology and archaeology overlap on that proportion of a living community which is in the process of transformation" (324).

5. See Lightfoot (1995) for a sophisticated appraisal of the advantages and limitations of analogical inference based jointly on historical and ethnographic sources. He is concerned, specifically, with the problems of understanding complex multicultural colonial sites in North America, where various components of these historic period communities differ greatly in the degree to which (and in the manner in which) they are represented in archival sources. Part of his analysis, especially relevant to

the present section, is a critique of the artificiality of the distinction between prehistoric and historic archaeology (202–204).

6. These reconstructions of antecedent cultural forms incorporate, on the one hand, the critical and comparative use of ethnographic resources that Clark recommends in connection with the use of Clark's folk culture analogies and, on the other, Hawkes's "tele-historic" methods of extrapolating from historical sources.

7. See the discussion in chapter 1 of the research by which Wedel and Strong established archaeologically that the "environmental limitations of the [North American central plains] are not so drastic as have often been believed" and had, in fact, supported sedentary horticulturist adaptations in prehistory (Strong 1935: 300, see also Wedel 1938).

8. See chapter 2 for an overview of Thompson's account of these aspects of archaeological practice.

9. R. Thompson argues, on this basis, that "the archaeologist injects a subjective element into his inferential reconstruction at least twice" (1956: 331): once in formulating the original interpretive hypothesis in the indicative phase of research and then again in the probative phase, when ethnographic analogs are sought that will establish that this hypothesis is anthropologically plausible (see chapter 2).

10. Stahl describes this recurrent pattern of debate about the risks of relying on ethnographic sources to construct "visions of past lifeways" in similar terms: "its long and controversial history in archaeology . . . has led us to recognize that analogy is an indispensable tool in our attempts to approximate the past, yet at the same time analogical inference is always subject to a degree of uncertainty" (1993: 235). In response to this uncertainty, Stahl emphasizes the need for more critical handling of analogical sources and argues for a judicious use of multiple lines of evidence, historical and ethnographic, in constructing and testing models of the cultural past.

11. The contentiousness of Gould's claims about limiting conditions is made clear by the protracted debate between processual and post- or antiprocessual archaeologists about the functionalist and eco-reductive assumptions of the New Archaeology, and it is reinforced by the continuing debate about evolutionary archaeology (see chapters 4 and 7).

12. See, for example, L. Binford (1982b), in response to postprocessual critics, and Dunnell (1989a, 1992) in defense of evolutionary archaeology.

13. Gould (1980: 49) cites Carson's *Silent Spring* (1962) in this connection.

14. See, for example, Gould's discussion of the "dis-

proportionate expenditure of time in hunting game by desert Aborigines" (1980: 10) and of the veneration of "righteous rocks" (141) as anomalous behaviors that indirectly serve ecological ends. Gould argues, for example, that the (apparently) functionally inexplicable behavior related to righteous rocks actually reinforces social networks that provide long-term insurance against the risk of rare but life-threatening local shortages of resources.

15. I argue this point in more detail in response to a dialogue between Gould and Watson about analogical inference (Gould and Watson 1982; Wylie 1982b).

16. There has been a long-running debate among philosophers about whether formal comparison, on its own, can ever establish grounds for drawing analogical conclusions. Shaw and Ashley report a range of positions bounded, at one extreme, by Mill's enthusiastic view that "every resemblance which can be shown to exist affords ground for expecting an indefinite number of other resemblances" (Shaw and Ashley 1983: 419) and, at the other, by critics who object that formal comparisons for similarity serve at best as a heuristic device. Weitzenfeld insists, in this spirit, that "nothing, not even an increase in likelihood, follows from mere similarity"; he argues that background knowledge plays a crucial role in any sound analogical argument, establishing supplementary reasons for assuming that the properties cited in the premises are linked by one or another form of "determining structure" (1984: 138). The thesis I develop later in this chapter is, in part, a response to this debate (Wylie 1988b). I argue that formal comparison can establish grounds for inferring further similarities insofar as it (sometimes) provides good indirect evidence that a nonaccidental relationship may hold among the properties that are compared and inferred.

17. Shaw and Ashley argue, in this connection, that "analogical arguments do not rely simply on resemblances . . . but on background beliefs, theories, generalizations, rules or principles which make the analogical move plausible and which are themselves open to epistemic appraisal" (1983: 423).

CHAPTER 10. PUTTING SHAKERTOWN
BACK TOGETHER: CRITICAL THEORY
IN ARCHAEOLOGY

Originally published under the same title as Wylie (1985b); reprinted here with minor revisions.

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1. This contrast is described in more detail in the introductory sections of chapter 4.

2. A comprehensive critique of positivism was also developed in the early 1980s by archaeological structuralists and post- or antiprocessualists of other stripes; see, for example, D. Miller and Tilley (1984b) and Hodder (1982a, 1982b, 1985), and discussions by D. Miller (1982) and Leone (1982a). I concentrate here on Leone's and Handsman's analysis because they ground their programmatic argument for a critical archaeology in empirical analyses of the play of interests in particular interpretations, and they offer an especially candid appraisal of the epistemological difficulties associated with a thoroughgoing critical self-consciousness.

3. Leone and Handsman typically identify their position as "critical" in the sense that it is influenced by Frankfurt School critical theorists (e.g., as characterized by Arato and Gebhard 1982; Geuss 1981), but they also cite structuralist Marxists. Leone draws on Althusser's theory of ideology when he develops the insight that much museum presentation of the past is interest-constituted and interest-serving. On this account museums, like other educational institutions, are understood to be one arm of an "ideological state apparatus" that supports the repressive structures by which the state controls its citizens and reproduces the social conditions necessary to sustain established modes of production; in particular, museums serve to ensure the reproduction and maintenance of a labor force that functions smoothly within the established social and economic system (Althusser 1971: 156). Educational institutions, Althusser's main interest, are a primary locus of socialization, which is reinforced by public institutions like the outdoor museums studied by Leone and Handsman. Leone thus finds in structural Marxism a theoretical corroboration of the Habermassian insight that the reproduction of ideological forms serves practical interests, which, in turn, sustain the social relations of production required by dominant economic-technological interests. For a later discussion of these influences and their implications for archaeology, see Leone and Preucel (1992); and for examples of critical analysis in practice, see contributions to Leone and Potter (1999).

4. In a later analysis Handsman, in collaboration with Richmond (1995), makes a powerful parallel argument about the political implications of re-

search informed by Eurocentric views about pre-historic settlement patterns. Handsman and Richmond argue that so long as the occupation of territory was equated with the existence of nuclear town or village sites, evidence of dispersed Native American presence in a kin-based homeland could not be recognized historically or legally. They consider, specifically, the role that this conceptual colonization played in legitimating the appropriation of the homelands of Mahican and Schaghticoke tribal groups in the repeated rejection of their land claims, and indeed in the denial of their very existence as a people (Handsman and Richmond 1995: 113–116). See also related analyses by Handsman of archaeological and historical treatments of the Weantinock homelands (1990) and of the archaeology of living traditions more generally (1989).

5. Leone and Potter (1986) and Handsman and Leone (1989) subsequently developed an analysis of how museum presentations can be designed to bring visitors to an awareness of the constructed nature of the past. In the case of Leone and Potter's collaboration, these proposals gave rise to a series of guidebooks to historic architecture and archaeological research at Annapolis that deal directly with how perceptions of the past have changed over time and how they reflect shifting contemporary interests in the past (Leone and Potter 1984).

CHAPTER 11. ARCHAEOLOGICAL CABLES AND TACKING: *BEYOND OBJECTIVISM AND RELATIVISM*

Originally published as Wylie (1989a): "Archaeological Cables and Tacking: The Implications of Practice for Bernstein's 'Options beyond Objectivism and Relativism,'" *Philosophy of the Social Sciences* 19 (1989): 1–18. © 1989 Sage Publications Inc. Reprinted, with minor revisions, with permission of the publisher.

1. Bernstein adds, in this context, that relativists believe there is "a nonreducible plurality" of conceptual schemes; they reject the claim of objectivists that concepts such as "truth, rationality, reality, right, the good, or norms" have any "determinate and univocal significance" (1983: 8).

2. Code suggests the term "mitigated relativism" for such positions (1991: 251). This reading of Bernstein's position is supported by an early passage in *Beyond Objectivism and Relativism* in which Bernstein observes that "while neither absolutism nor subjectivism is a live option for us now, the choice between a sophisticated form of fallibilistic objec-

tivism and a nonsubjective conception of relativism does seem to be a live—and indeed a momentous—one" (1983: 12–13). I argue the case for a "fallibilistic," or mitigated, objectivism in which judicious use of multiple lines of evidence can provide grounds for conclusions that extend well beyond their original contexts, even if those contexts are not transcended in any absolute sense.

3. In response to critics who have interpreted these authors as endorsing relativism in the second, extreme sense, Bernstein argues that it is more useful and illuminating to read them not as raising questions about the rationality of science as such but as arguing the need to rethink entrenched conceptions of rationality: they show why it is necessary to "set aside [the Cartesian] Either/Or" and "find a way of understanding the varieties of rational disagreement that cannot be eliminated in the frontiers of scientific inquiry" (1983: 60). The thesis common to Winch and to Kuhn "has been rightly taken as an attack on objectivism" (92); they argue that theories or forms of life may well be fundamentally incommensurable in the sense that they are not reducible to any universal grid, whether this be conceived in terms of an empirical basis (a set of facts), a common language, or a set of evaluative standards or criteria of rationality. Nonetheless, Bernstein insists that this critique has "*nothing to do* with relativism, or at least that form of relativism which wants to claim that there can be no rational comparison among the plurality of theories, paradigms, and language games—that we are prisoners locked in our own framework and cannot get out of it" (92; emphasis in the original). The equivocation on "relativism" that concerns me is particularly clear in this passage.

4. Bernstein notes, in several contexts, that the accounts given by Kuhn and more particularly by Winch of the options beyond objectivism and relativism are incomplete: "There is a gap or void at the center of Winch's analysis. . . . He has not given us the slightest clue about what *critical* standards we are to employ in [learning about and from unfamiliar cultures]" (1983: 106; emphasis in the original). Nor has he explained how researchers proceed when they "*compare* what may be *incommensurable*" (103; emphasis in the original).

It is here that Bernstein turns to the account of ethnographic practice developed by Geertz (1973, 1979 [1976]). This is as close as he gets to any direct consideration of practice, however. To give an account of the "options beyond" that are embodied, on his account, in the (hermeneutic) dimensions of science emphasized by Kuhn, Feyerabend, and Winch (and later obscured by their critics),

Bernstein turns instead to philosophical treatments of hermeneutics, specifically Gadamer's philosophical hermeneutics. Although this is a productive line of inquiry, revealing "themes [that] . . . contribute to the movement beyond objectivism and relativism" (1983: 165), Bernstein identifies a number of limitations in Gadamerian hermeneutics that parallel those he found inherent in Winch's and Kuhn's analyses. He concludes that Gadamer fails to answer the question "what is the basis for our critical judgments?"; he provides no justification for accepting the standards and norms to which he appeals when he invokes the notion of "appropriate forms of argumentation" that are warranted by particular traditions: "It is not *sufficient* to give a justification that directs us to tradition. What is required is a form of argumentation that seeks to warrant what is valid in this tradition" (154, 155; emphasis in the original).

What is required, Bernstein argues, is a detailed analysis of "how power as domination . . . operates in the modern world" (157), where this operation deforms *praxis* and *phronēsis*. Bernstein concludes that the options overlooked in the polarized debate between objectivists and relativists will be realized only insofar as forms of community life emerge that counter such deformation and foster dialogue and solidarity; they can flourish only in "dialogic communities in which *phronēsis*, judgment, and practical discourse become concretely embodied in our everyday practices" (223). He suggests that such forms of life are immanent in the existing social, communicative practices of researchers working "at the frontiers of inquiry" in pluralistic contexts. Thus it would seem that if we are to understand and, more important, bring into practice the "options beyond," we will need to pay close attention to the ways in which practitioners actually proceed when they must mediate deep cultural and theoretical differences. This is the direction forward suggested by Bernstein's account that I propose to follow here.

5. It is worth noting, however, that the claim Peirce himself made (accurately characterized by Bernstein) was that the collective force of the argument is *stronger* than its constituent strands, not that it is conclusive.

6. See, for example, the account of such epistemic contestation considered by MacIntyre (1985, 1984).

7. To illustrate the role of experience-distant concepts, Geertz considers examples drawn from his own work in Java, Bali, and Morocco in which he finds the focal concept of a person articulated in very different ways (1979 [1976]: 228). When Winch urges that social inquiry be organized around the "limiting notions" of birth, death, and sexuality—he believes these are central human preoccupations around which all cultures have elaborated symbolic orders and explanatory schemes—he recommends something like a Geertzian use of experience-distant concepts (1970: 107; see also 1990: 88–90).

8. See, for example, Bernstein's subsequent use of this tacking metaphor to explicate the hermeneutic features of Kuhn's and Feyerabend's analyses (1983: 133).

9. Consider, for example, Winch's argument that Evans-Pritchard (1937) would have done better to use our concepts of luck and misfortune as the framework for understanding Azande witchcraft rather than identifying these practices as an embryonic (or failed) form of scientific practice or assimilating them to our own concepts of witchcraft (1970: 90–95). Here he proposes not just that Evans-Pritchard's account of the Azande beliefs be revised to better explain the evidence he presents (i.e., to avoid contradictions between the experience-distant concepts he deploys and the experience-near practices and beliefs he describes), but also that we realign our own conceptual scheme in recognition that none of our categories makes sense of Azande practice. The disjunction between Azande beliefs and our own (both near and distant) throws into relief the context- and interest-specific nature of scientific rationality; the fact that the Azande appeal to witches, as well as to more familiar causal explanations, to explain puzzling everyday events suggests the possibility of forms of life whose point is quite different from ours.

CHAPTER 12. "HEAVILY DECOMPOSING RED HERRINGS": MIDDLE GROUND IN THE ANTI–POSTPROCESSUALISM WARS

it to Dr. James F. Pendergast, who received an SAA Distinguished Service Award at that meeting.

1. The debates I refer to erupted in the early 1980s between processualists and self-identified post-processualists, and were sharply polarized by the late 1980s and early 1990s. See chapters 4 and 6 for an account of the early stages of these debates.

2. L. Binford laments the fact that in these debates, "antagonists rarely perform at very admirable levels" (1989: 486); they routinely rely on ad hominem and ignoratio elenchi (straw man) fallacies (4, 78), attacking the moral and intellectual character of those who endorse the positions they oppose rather than engaging the specifics of the positions themselves. At the same time, however, Binford makes enthusiastic use of just these forms of argument; see, for example, his "field guide" to what he calls the Yippies, Yuppies, Guppies, Puppies, and Lollies of the postprocessual theoretical world (5–9).

Likewise, Shanks and Tilley insist that they have "a duty to engage in constructive dialogue and to take our critics seriously," declaring that the "adoption of rhetorical strategies . . . does not free us from the responsibility of dealing directly with the issues vital to the development of our archaeology" (1989: 42, 48). Nevertheless, they reaffirm the value of deliberate rhetorical provocation when it serves the purpose of unsettling the orthodoxy of archaeological (and other) conventions (see their discussion of chap. 1 of *Re-constructing Archaeology* [Shanks and Tilley 1987], 1989: 8). As noted by virtually every commentator who contributed to the *Norwegian Archaeological Review* forum on Shanks and Tilley's work, this inconsistency has resulted in a programmatic stance riddled with "serious contradictions" (e.g., Bender 1989; Hodder 1989; Renfrew 1989a; Trigger 1989a), or at least "incompatibil[ities]" (Olsen 1989). Shanks and Tilley retain, at the heart of their own position, substantial elements of most of the orthodoxies they reject. In some contexts, they continue to privilege evidence and related (empiricist and realist) presuppositions of foundationalism (Trigger 1989a: 29; Olsen 1989: 19); they embrace various structuralist assumptions (Bender 1989: 13) alongside a critique of the subject that is compromised by a failure to incorporate the insights of poststructuralism (Hodder 1989: 16); and their political stance exploits, or leaves unchallenged, many aspects of their own privilege and location within institutions of the establishment (Bender 1989: 12; Olsen 1989: 20). For an independent analysis that puts these internal tensions in a larger context, see Patterson (1990; also 1989).

3. "Red herring" fallacies are a form of diversionary argument in which one party deflects attention from the specifics of the case or conclusion in question by focusing on a side issue or on generalities. The name for this fallacy comes from the practice of drawing a herring across the path of a hunt to throw hounds off the scent of their quarry.

4. Renfrew (1989a) has objected that the positions identified by Hodder and others as postprocessual (Hodder 1985) do not, in fact, displace or transcend processual archaeology. He argues that they are more accurately labeled "antiprocessual." Because my aim is to identify common ground between the divergent views represented by this terminology, in what follows I will refer to these positions using both terms, as anti-/postprocessual.

5. Certainly, Renfrew's appeal to Popper is unhelpful, given the open-endedness of Popper's criterion of demarcation—the requirement that science be practiced with a critical attitude—and the controversial nature of his more specific, falsificationist account of scientific practice; see chapter 15 for the details of these arguments against unity theses.

Although Binford makes influential use of Hempelian models in his early essays, he does not invoke any specific philosophical models or conceptions of science in the discussions cited here; rather, he presupposes what might be described as a vernacular positivism (I thank Margie Purser for this characterization of his later position). In this connection, Binford frequently objects that anti-/postprocessualists measure scientific practice against inappropriately high expectations; he likens these to a hypothetical demand that science should provide an understanding of "life after death" by a group that thinks such insight crucial to the completeness of our knowledge and well-being and that rejects scientific method when it proves not to serve those goals (1989: 27–28). The point of this allegory seems to be that the failure of science to provide access to the intentions and beliefs of past agents—to internal and ethnographic dimensions of the cultural past (Hodder 1991)—cannot be taken seriously as grounds for concluding "that science is useless" and should be abandoned as "a learning strategy so far as the world of experience is concerned" (L. Binford 1989: 27). Binford recommends that the goals of inquiry be revised so that they are amenable to investigation by scientific method: the questions asked must be answerable in terms that refer to experience. In an earlier passage, when differentiating processual archaeology from both its antecedents and proposed successors, Binford characterizes its defining commitment as a concern to systemati-

cally "evaluat[e] the utility and accuracy of ideas" coupled with a conviction that scientific methods are the only way to accomplish this goal (17).

6. A central point of contention in the *Norwegian Archaeological Review* discussions of Shanks and Tilley's postprocessualism has to do precisely with this question of whether, or how, genuinely scientific practice is to be defined and identified. A recurrent worry raised in these commentaries is that Shanks and Tilley's antiobjectivism seems to entail the abandonment, or erosion, of any distinction between archaeological discourse (as scientific) and fiction. Renfrew (1989a) challenges Shanks and Tilley to demonstrate that this is not a consequence of their position, clearly assuming that such an outcome is manifestly untenable; in effect, he charges that anti-/postprocessualism is threatened by a reductio ad absurdum. But the failure of attempts to establish a coherent and plausible demarcation theory suggests that we must systematically rethink what it would mean to distinguish scientific from fictional discourse. As Longino observes, "the novelists among us might remind us that if there is a fiction in the discourses of truth, so there is a truth in the discourses of fiction" (1990a: 174). Ironically, Shanks and Tilley seem to share Renfrew's conviction that such boundaries must hold; "there is no simple choice to be made between a subjective or an objective account of reality unless one is to abandon science altogether and write novels instead" (1987: 110).

7. Here I paraphrase Shanks and Tilley's declaration that archaeologists should recognize a distinction between "being empirical and being empiricist" (1987: 115, 1989: 50).

8. Indeed, much of Hodder's subsequent work seems to have been framed as a response to critiques of the corrosive relativism imminent in his earlier arguments against processualism. For an overview of his later position, see Hodder (1999).

9. Hodder appeals to Gadamer and post-Gadamerian theorists (especially Ricoeur); their analyses of hermeneutic practice have the virtue of dealing explicitly with the problem of a disabling relativism that he finds implicit in the advocacy of pluralism. What he hopes to secure, with the help of hermeneutic theory, is a "boundary between an open multivocality where any interpretation is as good as another and legitimate dialogue between science and American Indian, black, feminist, etc. interests" (1991: 9). To this end, he advocates an interpretive position that "give[s] science a context in archaeology as methodology," thereby avoiding an "ungrounded undermining of knowledge claims by interested groups and . . . a subsuming of the past within a homogenized theoretical present" (10).

10. I refer to the broadly analogical practices described in detail in Wylie (1985c) and in chapter 9.

11. Despite the postpositivist tenor of this remark, Renfrew hastens to add that "when the chips are down, however, it is the data which have the last word" (1989a: 39). He then cites R. Braithwaite (1953)—not a notably postpositivist discussion!—to substantiate this claim.

12. Pickering defended one such constructivist position in *Constructing Quarks:* "In principle the decisions which produce the world [evidentially as well as theoretically] are free and unconstrained. They could be made at random, each scientist choosing by the toss of a coin at each decision point what stance to adopt" (1984: 406). Pickering has since moved to a position that lies between an uncompromising rejection of "constraint talk," in which he reaffirms a strongly relativist constructivism, and what seems an amendment of his earlier stance in which, for example, he distances himself from the views of Collins (1985). For the former position, see Pickering (1989, 1990); and for the latter, see Pickering (1987) and the introduction (1992a) to an extremely useful collection of essays on recent developments in interdisciplinary science studies (1992b) in which he outlines the history of development of a range of schools of sociology of science, most of which have moved decisively away from the earlier oppositional stance that characterized the Edinburgh Strong Programme.

13. Well-entrenched background knowledge suggesting that the linkages in question are radically unstable or idiosyncratic will obviously undermine, rather than secure, any inference based on them, so security in the first sense is not sufficient to establish grounds for archaeological inference without security in this second sense.

14. Although Kosso (1989) is mainly concerned with arguments that exploit the independence between the background knowledge used to constitute observational evidence and the claims such evidence is used to support or refute—he develops a formal measure of independence of this sort—he also considers the role played by the use of multiple lines of evidence in stabilizing evidential claims and thus securing their objectivity. When he considers independence in this sense, he refers to the way in which evidence is used to establish claims about "ancient history."

15. Compare Binford's discussion of the methodological significance of "areas of ambiguity" with

Gould's proposal of a "method of anomaly" (1980), discussed at the end of chapter 9.

CHAPTER 13. BOOTSTRAPPING IN THE UN-NATURAL SCIENCES— ARCHAEOLOGY, FOR EXAMPLE

Originally published as "Bootstrapping in Unnatural Sciences: Archaeological Theory Testing" (Wylie 1986b), in *PSA 1986*, volume 1, edited by Arthur Fine and Peter Machamer (Philosophy of Science Association, East Lansing, Mich., 1986, pp. 314–322). Reprinted, with minor revisions, with permission of the Philosophy of Science Association.

1. Psychoanalysis is one of the fields considered by Glymour in *Theory and Evidence* (1980: 263–277) in which bootstrapping arguments play an important role. Meehl subsequently elaborates several central points of Glymour's analysis, citing archaeology as one of the intellectually respectable "documentary disciplines" whose bootstrapping confirmation procedures are comparable to those that support psychoanalytic constructs (1983: 360).

2. Glymour's bootstrapping model of confirmation generated immediate and sustained debate as a rival both to the hypothetico-deductive models advocated by logical positivists and neo-empiricists and to the Bayesian models of belief revision that were then becoming influential. The outlines of this debate are well represented in Earman (1983), and reviewed in Wylie (1988a). Critiques especially relevant to this discussion include van Fraassen (1983), Christensen (1983), and Zytkow (1986); see also Glymour's responses to these critiques (1983a, 1983b). For recent proposals that move beyond this debate, see Mayo (1996); she reviews the now standard approaches to understanding scientific uses of evidence described above and argues that an "error statistical" analysis of testing practice makes better sense of how scientists learn from evidence, especially in experimental practice.

3. The third factor, the intensification of regional trade networks, subsequently became a dominant focus of research in southwestern archaeology (e.g., S. Plog 1978, 1980).

4. Meehl (1983) discusses this dynamic of mutual constraint when he considers "consistency tests" in connection with psychoanalytic theory. Elsewhere (in Wylie 1994a, 1996a, 2000c, and above in chapters 12, 14, and 15) I describe this as a form of evidential support that arises from *horizontal* independence between lines of evidence. In developing his bootstrapping model of confirmation, Glymour's primary concern is with evidential arguments that exploit the *vertical* independence that may exist between test hypotheses and linking principles even when both are derived from the same encompassing theory.

CHAPTER 14. THE CONSTITUTION OF ARCHAEOLOGICAL EVIDENCE: GENDER POLITICS AND SCIENCE

The first section of this chapter appeared as the introduction to "Evidential Constraints: Pragmatic Objectivism in Archaeology" (Wylie 1994a), in *Readings in the Philosophy of Social Science*, edited by Michael Martin and Lee C. McIntyre (MIT Press, Boston, 1994, pp. 747–766). Reprinted here with permission of the publisher.

The body of this chapter was previously published as "The Constitution of Archaeological Evidence: Gender Politics and Science" (Wylie 1996a), in *The Disunity of Science: Boundaries, Contexts, and Power*, edited by Peter Galison and David J. Stump (Stanford University Press, Stanford, Calif., pp. 311–343). © 1996 by the Board of Trustees of the Leland Stanford Junior University. Reprinted with permission of the publisher.

1. Although never a dominant concern, feminist interests and influences are evident in postprocessual research—for example, in contributions by Hodder (1984b) and M. Braithwaite (1984) to *Ideology, Power, and Prehistory* (D. Miller and Tilley 1984a). At the same time, however, feminist commentators object that postprocessual archaeologists have largely avoided reflexive critique of the gender biases that persist in their own practice, some of which are not just androcentric but quite explicitly sexist. See, for example, Engelstad (1991) and Gilchrist (1992); Gilchrist draws attention (190) to a notorious passage in which Shanks likens archaeology, specifically excavation, to striptease—each "discovery is a little release of gratification"—leaving little doubt that the subject position of the archaeologist is normatively gendered male.

2. See, more generally, McGuire and Paynter (1991) on the implications of focusing attention on "the archaeology of inequality."

3. See chapter 10 for a more detailed discussion of Handsman's earlier critiques of the assumption that occupation must be marked by nucleated, permanent settlements, a Eurocentric perspective that systematically obscured the presence of Native Americans in their traditional homelands historically and archaeologically.

4. See, for example, Spector and Whelan (1989) and Conkey and Spector (1984) for discussion of the

androcentric and sexist bias of "man the hunter" models of human evolution, and Gero's related critique (1991) of standard patterns of functional ascription to stone tools.

5. See also Blakey (1983) on sociopolitical bias that structures the research of one subfield of archaeology, historical archaeology.

6. This equity literature typically appears in society or institution newsletters, in publications produced by in-house report series, or in informal reports and internal documents. Some of the more accessible and widely known of these equity studies include Kramer and Stark (1988), Gero (1983, 1985), Levine (1991) and other essays in Walde and Willows (1991), and the final three sections of du Cros and Smith (1993). Some of these reports and articles, as well as many that are less accessible, are reprinted in M. Nelson, Nelson, and Wylie (1994), along with a number of new studies of equity issues that arise for women who work in a wide range of national contexts and subfields of archaeology. Claassen (1994) assembles some important historical analyses of the status and contributions of women to archaeology; and these issues also are addressed, though less centrally, by contributors to Claassen and Joyce (1997) and to Wright (1996).

7. Gero's analysis is particularly striking when read in light of critical reassessments that had begun to appear in the late 1980s of the dominant idea that there was a distinctive, continentwide "Clovis adaptation." In 1986 Meltzer and Smith argued that a generalized foraging model is more plausible for both Palaeoindian and Early Archaic subsistence systems (i.e., there is much continuity between the subsistence practices that dominated in these periods) than highly specialized large game hunting; the latter model "is unsupported either by archaeological evidence or theoretical expectations derived from models of foraging theory and data on late Pleistocene environments" (1986: 3). Meltzer (1993) later offers a trenchant critique of the defining assumptions of the tradition of research that concerns Gero, focusing on incongruities in the distribution and associations of Clovis points on North American Pleistocene sites. He argues that there is more internal variability in these assemblages than had been recognized: a number of sites provide clear evidence that Palaeoindians exploited a wide range of resources; collateral evidence (paleoenvironmental and ethnohistoric) calls into question the plausibility of the specialized hunting hypothesis as a feasible adaptation in much of Pleistocene North America; and fracture patterns suggest that Clovis points are not necessarily di-

agnostic of hunting, but may have been used in connection with a range of other subsistence practices. Meltzer does not consider the gender dimensions of this research program; but in a recent popular review of Palaeoindian research, James Adavasio is quoted as arguing that "the official mammoth-centric picture of early Americans completely neglects the role of women, children and grandparents" (in Nemecek 2000: 84).

8. Although they had not coalesced into a distinctive research program, feminist themes had been explored by North American archaeologists in a number of other connections before 1984. For example, Rapp (1977) discussed the potential of archaeology to contribute to the understanding of how states are formed; Barstow assessed Melaart's work at Catal Hüyük (1978) from a feminist perspective; an archaeologist (Voorhies) coauthored an early textbook in feminist anthropology that included consideration of archaeological questions about women and gender (M. K. Martin and Voorhies 1975); and Kehoe (1983) and Spector (1983), among others, published archaeological discussions of questions about gender relations in problem-specific collections (e.g., in Nelson and Kehoe 1990) or in region-specific literatures where anthropological and historical accounts of the status and roles of women had been developed already (e.g., Albers and Medicine 1983).

A number of annotated bibliographies are now available that provide summaries of conference presentations and publications on the subject of archaeology and gender: Claassen (1992a); Bacus et al. (1993); Hays-Gilpin and Roberts (2000). In the preface to the first of these (a guide to conference presentations between 1964 and 1992), Claassen notes that only 24 of the 284 entries she lists predate January 1988, and only 2 had appeared in print by the time she compiled her bibliography (1992a: 1).

Outside North America there were a number of feminist conferences and publications before 1984, and they have proliferated since the mid-1980s. These include a conference held in Norway in 1979, the proceedings of which appeared eight years later (Bertelsen, Lillehammer, and Naess 1987); subsequently, Norwegian women archaeologists have met regularly and have published a journal since 1985, *Norwegian Women in Archaeology* (the Norwegian acronym is KAN). In the United Kingdom several sessions on women and gender were organized for the annual meetings of the Theoretical Archaeology Group in 1982, 1985, and 1987; this history of discussion is summarized in a special issue of *Archaeological Review*

from Cambridge on women and archaeology (K. Arnold et al. 1988: 2–8).

For an earlier discussion of the developments examined in this section, see Wylie (1992d), as well as comments by B. Little (1994) and Fotiadis (1994).

9. Interest in feminist and gender research in archaeology has grown dramatically since the late 1980s. The first open conference devoted to these issues in North America was the 1989 Chacmool conference, "The Archaeology of Gender," and it drew a remarkable response. Chacmool conferences are organized annually around different themes by the Archaeological Association of the University of Calgary and typically attract forty to fifty contributions. Although there was little indication in print that there was a constituency of archaeologists interested in "gender research," the call for papers for the 1989 Chacmool conference drew more than 100 submissions from archaeologists all over the United States, New Zealand, Norway and Sweden, the United Kingdom, and Western Europe (for a more detailed descriptions of this watershed conference, see Wylie 1997; Hanen and Kelley 1992). The proceedings appeared two years later (Walde and Willows 1991) and in the same year *Historical Archaeology* published a special issue on gender in historical archaeology (Seifert 1991). During this period Claassen organized a series of annual meetings on women, gender, and archaeology at Appalachian State University in North Carolina; the proceedings of the first and selected papers from the second and third are now in print (Claassen 1992b; Claassen 1994; Claassen and Joyce 1997). And Australian archaeologists have organized annual and biennial conferences on gender issues in archaeology since 1990, several of which have produced published proceedings (du Cros and Smith 1993; Balme and Beck 1995).

As this overview suggests, the archaeological literature on women, gender, and feminism has grown exponentially since 1992. A dozen conferences have been organized on gender in North America, in the United Kingdom, and elsewhere in Europe, and more than 450 papers written by 300 individuals have been presented since 1987 (Claassen and Joyce 1997: 1; see also Conkey and Gero 1997: 414). This literature also now includes, in addition to a growing number of proceedings and edited volumes such as those cited above, several monographs (Spector 1993; Gilchrist 1993; Wall 1994), at least two readers (Hays-Gilpin and Whitley 1998; Colomer et al. 1999, a collection of English-language essays in Spanish translation), one early prospective overview of "women in pre-

history" (Ehrenberg 1989), and two ambitious book-length assessments of the research on gender that has since taken shape (S. Nelson 1997; Gilchrist 1999). There seems, then, to have been widespread latent interest in the topic of gender that was tapped by the 1989 Chacmool conference and has since given rise to an enormously diverse and expansive body of work. I give a more detailed account of why this interest in questions about women and gender arose so quickly and so late in archaeology in Wylie (1990, 1991, 1997).

10. The question of how feminist commitments have influenced archaeological practice is complicated. In Wylie (1997) I report the results of a survey and series of interviews indicating that although the organizers of early conferences and edited volumes on gender in archaeology typically do identify as feminists and sought ways to integrate their archaeological interests with feminist commitments, barely half of those who participated in the first public (North American) conference on gender and archaeology (the Chacmool conference of 1989) identify as feminist or indicate any involvement in women's organizations or activism. In Wylie (2001) I consider the implications of these results and address directly the question of whether, or in what sense, recent gender research in archaeology is feminist or feminist-influenced.

11. The most uncompromising critics of the relativizing tendencies inherent in a postmodern stance insist that "the feminist postmodernists' plea for tolerance of multiple perspectives is altogether at odds with feminists' desire to develop a successor science that can refute once and for all the distortions of androcentrism" (Hawkesworth 1989: 538).

12. G. Fritz (1999) has since argued for a reassessment of this analysis in light of evidence that gourds may have been used as net floats and domesticated earlier than, and independently of, the transition to horticulture. The role of women in this domestication process is more ambiguous than in that responsible for the complex of indigenous food plants that became central to horticulture in eastern North America: "as the cast of characters and sequence of events have shifted, so too have gender-based scenarios concerning the earliest food producers" (417).

13. The 1991 coauthored article cited in the text shares several main points with Conkey's conference paper (published as Conkey 1991).

14. "Determining structure" is Weitzenfeld's terminology (1984; see chapter 9 above); I use it to refer to a wide range of dependencies that may hold between observed and inferred traits, few of which are strictly deterministic.

15. This first of these two types of constraint can be

illustrated by an argument developed by Gero (1991) in her contribution to *Engendering Archaeology:* archaeologists should be prepared to question a range of assumptions that associate specific types of production and use of stone tools primarily with male activities and labor. Ethnohistoric and experimental evidence concerning the physical requirements of such production and the typical, even predominant, patterns of use documented among foragers undermine many standard assumptions about the gender associations of these tools and activities. The second, archaeological constraint is illustrated by Brumfiel's and by Hastorf's analyses.

16. In two subsequent articles Brumfiel extends her analysis. In her 1991 American Anthropological Association Distinguished Lecture in Archaeology (Brumfiel 1992), she situates her analysis of gender in the larger context of archaeological studies of social change and stratification/factionalization that have emerged in response to the shortcomings of ecosystem approaches. And in an analysis of evidence for women's resistance to the Aztec extraction of cloth tribute (1996), she develops a compelling argument for the potential of archaeological data to put complex hypotheses about social dynamics to the test.

17. I cite Tringham's work on Neolithic architecture because in it she uses close microstratigraphic and materials analysis to establish details of construction sequence and technology. But her central argument for gender-focused research has to do with the importance of restoring agency to archaeological interpretation: she seeks to rectify a problem Cowgill describes (referring to Tringham's analysis) as "underconceptualization" (1993: 555), in which those who populated the sites and used the artifacts of interest to archaeologists remain "faceless blobs" (Tringham 1991: 94). The challenge Tringham takes up is that of how to proceed "if one does not assume households to be faceless units of cooperation, and if one does not assume that housework is a given universal pattern of devalued at-home social action, and if one does not assume that the roles and relations of men and women in domestic space is more or less uniform, and if one does not assume that the built environment looks the same to prehistoric eyes as it does to ours" (1991: 103). In this connection she urges archaeologists to move beyond the constraints of safely operationalizable, natural science–warranted interpretation (Tringham 1994, 1998).

18. In fact, this recognition that the data cannot be (coherently or meaningfully) constituted as evidence seems to be the conclusion that Longino and Doell draw in analyzing the inferential distance involved in any ascription of social, functional significance to tools associated with early hominid sites. They argue that "any speculation regarding the behavior and social organization of early humans remains just that": "woman the gatherer" and "man the hunter" are equally unsubstantiable interpretations (1983: 217). It is perhaps significant that they cite interpretations offered by three prominent early New Archaeologists, including Lewis Binford, whose deductivist commitments made them among the most outspoken critics of any use of analogical inference. I have argued that the position taken by the New Archaeologists (especially Binford) on such reasoning represents a largely rhetorical reaction against the practices they associate with traditional archaeology. It is inaccurate as an account of analogical inference and of archaeological practice (chapter 9). Thus, while I wholeheartedly endorse Longino and Doell's insistence that the limits of reconstructive inference be clearly recognized, I question their generalized pessimism about the cases they consider. L. Nelson has developed a trenchant critique of their conclusions that articulates a more general principle: "Longino and Doell's conclusions are warranted only if we assume that *no* other scientific theories can be brought to bear on the reconstruction of our evolution, theories that might lend more credence to one interpretation of the chipped stones over another, and/or we assume that 'culturally determined' beliefs, or commonsense beliefs including those about sex/gender, are not subject to empirical control and cannot be considered as evidence" (1990: 243, emphasis in the original; see also 1993: 143–144).

CHAPTER 15. RETHINKING UNITY AS A "WORKING HYPOTHESIS" FOR PHILOSOPHY OF SCIENCE: HOW ARCHAEOLOGISTS EXPLOIT THE DISUNITIES OF SCIENCE

tion, Eastern Division, Philadelphia). I very much appreciate the comments and questions raised on that occasion by members of the audience and by Philip Kitcher in his commentary on the session. I subsequently learned a great deal from lively discussions of later drafts of the APA paper when I presented them to the Departments of Philosophy and Anthropology at the University of Pennsylvania, the Department of History of Science at Harvard, and the Minnesota Center for Philosophy of Science at the University of Minnesota. I hope those who commented see ways in which their engagement of the issues has sharpened my argument, even if they (still) disagree with me.

1. In a paper written originally for "Method and Theory 2000," a symposium at the 1999 Annual Meeting of the Society for American Archaeology (Wylie 2000b), I argue that these ideals of scientific unity underpin the protracted public debate now known as the Science Wars (e.g., as transacted by Gross and Levitt 1994 and by A. Ross 1996): "What science critics contest and science advocates defend is the very idea that there is such a thing as 'science': a unified enterprise defined by a set of shared attributes that uniquely determine what it is for a discipline to be scientific, that set real science apart from other (lesser) epistemic enterprises, and that trump any non-scientific interests or knowledge claims that might challenge the epistemic authority of science" (Wylie 2000a: 229). In the context of archaeology, I argued, these commitments are pivotal to a recurrent pattern of debate in which the credibility of a research program or theory or form of practice is defended (or rejected) not on the basis of an assessment of its accomplishments but by appeal to its promise of affiliation with "science." Debate turns on claims and counterclaims about which practices or theories or approaches are properly scientific, which display the essential (unifying) characteristics of science. My thesis was that just as global unity arguments fail (for reasons I develop in more detail here), so too does this local strategy of legitimation. It is an attempt to settle in advance questions about research practice that can be resolved only by evidence and experience. My recommendation for avoiding the pitfalls of Science Wars strategies of argument was thus "a variant on the advice offered by environmental activists: think globally, about the resources that a wide range of research fields have to offer archaeology, but make the case for the credibility and authority of archaeological practice locally, in terms of particular interfield relations and their efficacy in solving specific archaeological problems" (229).

2. The unity claims that Oppenheim and Putnam describe as doubtful include claims about the unity of scientific method and about logical unity. By "logical unity" they mean theses according to which all terms of science are reducible to "sensationalistic predicates" or "observable qualities of physical things" (1958: 5).

3. Hacking makes this point about diversity in science with reference to Comte and Whewell (1996: 38).

4. Note that such an argument does not establish the kind of unity thesis about scientific methodology that Dupré objects to—a monistic thesis that posits the uniqueness and "singleness" of scientific method—and it does not entail or support "scientific imperialism" (Dupré 1995). The core epistemic virtues cited by Dupré are "sensitivity to empirical fact," cohesion with things we know, reliance on plausible background assumptions, and exposure to as wide a variety of criticisms as possible. In addition, Ereshefsky observes that Dupré considers a number of other "free-floating aesthetic virtues," such as unity, generality, and simplicity, that may be differentially relevant to some fields, or subfields, of science (1995: 157). On Ereshefsky's account, the central challenge for philosophers of science is to understand, in local and contingent terms, how far Dupré's virtues extend across scientific disciplines and how they are realized in diverse contexts of practice. Ironically, similar arguments for studying, in fine-grained (naturalistic) detail, the flexibility, adaptability, and mutability of scientific method have been made both by critics and by advocates of unity theses. Compare R. Laudan and Laudan, "Dominance and the Disunity of Method" (1989), with Giere, "Toward a Unified Theory of Science" (1984).

5. What Oppenheim and Putnam refer to as "epistemic theses" are claims about methodological and logical unity, the senses of unity they thought untenable (1958: 5).

6. Wayne (1996) challenges Morrison's account of the unification accomplished by electroweak theory. He argues that the structural unification she describes depends on the selection of a particular "argument pattern"—a particular component of the complex array of formal models developed to make sense of different kinds of subatomic systems (1996: 399)—as the element of formal structure that will be held invariant through all transformations required to apply the theory to diverse systems. This choice, he insists, depends on prior ontological commitments that inform "an interpretation of the standard model that includes a small ontology of elementary quantum fields"

(404), and it is these ontological commitments, not the formalism, that makes unification possible. This claim undermines Morrison's argument that unification in the electroweak case is strictly structural, and may have important implications for her arguments against realist construals of theoretical unification (1990, 1994), but it does not necessarily establish (nor does it seem intended to establish) that the case fits the theory reduction model associated with Oppenheim and Putnam's "working hypothesis." If anything, it suggests that electroweak theory is an example of the kind of "interfield theory" (discussed later in this chapter) that Darden and Maull (1977) described as a standard form of field-bridging development in science that does not fit and, indeed, was obscured by a preoccupation with derivational reduction.

7. Fodor elaborates this point later in his discussion:

> This brings us to why there are special sciences at all. Reductivism . . . flies in the face of the facts about the scientific institution: the existence of a vast and interleaved conglomerate of special science disciplines which often appear to proceed with only the most token acknowledgment of the constraint that their theories must turn out to be physics "in the long run."
>
> I am suggesting, roughly, that there are special sciences not because of the nature of our epistemic relation to the world, but because of the way the world is put together: not all natural kinds (not all classes of things and events about which there are important, counterfactual supporting generalizations to be made) are, or correspond to, physical natural kinds. (1974: 112–113)

8. Dupré is careful to say that he does not claim to have "refute[d] *a priori* the possibility that future scientific developments might make a monistic, even essentialist view of species increasingly attractive" (1996b: 441). See also Fodor: "The question whether reductivism is too strong is finally an empirical question. (The world could turn out to be such that every natural kind corresponds to a physical natural kind, just as it could turn out to be such that the property *is transported to a distance of less than three miles from the Eiffel Tower* determines a natural kind in, say, hydrodynamics. It's just that, as things stand, it seems very unlikely that the world *will* turn out to be either of these ways.)" (1974: 103; emphasis in the original).

9. In the pattern described by Darden and Maull, interfield explanatory problems arise by virtue of causal interaction or part:whole interdependence between the entities that constitute the distinct domains of two fields, or because two fields study the same phenomena from different points of view; for example, one may focus on structure and another on function (Darden and Maull 1977: 45) or on process as opposed to product, in the same domain (Abrahamsen 1987: 370). Darden and Maull describe situations in which existing background knowledge in one or both fields establishes, in advance, that they are dealing with phenomena studied by another field. An interfield theory arises when, in order to account for these cross-field connections, it becomes necessary to introduce substantially new ideas not derived from either contributing field or the background knowledge that establishes the link between them. Bechtel describes the cases Darden and Maull consider as arising in situations in which interfield theories emerge to "fill . . . in missing information about a phenomena that was already partially understood in other fields" (1986: 45). He illustrates these points with an example in which the development of an interfield theory—one that links research on vitamins to research on metabolism—was triggered by the accidental discovery of domain-transgressive phenomena (45–46). Darden (1991) has since expanded this catalogue of interfield theories in connection with a general account of interfield relations that are instrumental to the formation of theories.

10. Galison argues that the formation of a distinct technical language, an interfield pidgin that became a creole (1996: 153), was instrumental in setting simulation researchers apart from colleagues in their home fields and in creating the heterogeneous domain that he calls a trading zone.

11. It is a contingent matter whether, in fact, these interfield theories or technical trading zones will crystallize into a distinct new field. That may be the ultimate outcome, but, Bechtel argues, the degree of autonomy that is realized by new research initiatives depends on such factors as the integrity of the interfield phenomena under study and the ability of interfield researchers to maintain ties with their home disciplines (1986b: 37).

12. As indicated earlier, Darden (1991) has subsequently broadened her account of the relations that bind (and divide) research fields.

13. This characterization of a spectrum of interfield connections is based on Abrahamsen (1987), Darden (1991), Bechtel (1988: 71–118), and the introduction by Bechtel (1986b) as well as contributions to *Integrating Scientific Disciplines* (Bechtel 1986a). It is also informed by Kincaid's more general analysis (1997), by Baigrie and Hattiangadi's discussion of consensus and stability in science (1992),

and by Hacking's account of self-stabilizing, consilient networks of "ideas, things, and marks" (1992a: 44).

14. Dupré cites Darden and Maull in this connection, arguing that their examples of interfield theories do not establish grounds for endorsing any very strong unity thesis. However, Darden and Maull present their account of interfield theories as a counterexample to Oppenheim and Putnam's working hypothesis; even in cases in which neighboring fields deal with just the kinds of boundary-straddling, part:whole relations that are most amenable to interfield reduction, reduction is often not realized.

15. See, for example, Schuyler's discussion in the preface (1978b: ix) to the first comprehensive reader in historical archaeology, *Historical Archaeology: A Guide to Substantive and Theoretical Contributions,* and in his introduction to part 1 of the reader (1978a: 1–2). He argues that historical archaeology—"an entirely new area of scholarly research and public concern" (1978b: ix)—took shape after World War II, but notes that the Society for Historical Archaeology was formed and began publishing *Historical Archaeology* only in 1967. Schuyler describes parallel developments in the United Kingdom and in Australia, although the disciplinary affiliations are different in these contexts from those in North America (e.g., industrial archaeology and the archaeology of Roman Britain, in the case of historical archaeology in the United Kingdom). As he notes, much of the impetus for the development of this fledgling field in North America came from federal and state or provincial programs of historic site preservation and interpretation.

16. Similarly Bechtel argues, with reference to psychology and neuroscience, physiology and chemistry, that an insistence on strict disciplinary autonomy can be as counterproductive as reductive unification schemes (1988: 79–81). And Darden shows how important a role interfield interactions play in the creative development of science (1991).

17. See the discussion in chapter 14 of recent developments in the archaeology of slavery, European : Native American interactions in the contact period, the changing roles and activities of women in the historic period, and the history of colonial oppression (e.g., contributions to Schmidt and Patterson 1995; Sued-Badillo 1992) and of capitalist systems (Leone and Potter 1999).

18. If a self-vindicating foundation cannot be found, one vindicated by physics is close enough (see chapter 7). This is sometimes the tenor of Binford's defensive responses to his postprocessual critics (L. Binford 1989). See also recent debates about the merits and limitations of evolutionary archaeology (e.g., Schiffer 1996: 650). In both cases the identification of a quasi-foundation is the opening move in an argument to restrict the scope of archaeological inquiry to those aspects of the cultural past that can be investigated using just the kinds of evidence that can be considered secure beyond reasonable doubt.

19. See Kosso (1992) and VanPool and VanPool (1999), as well as chapter 12, for parallel accounts of the similarities in research strategy that underlie the sharply drawn opposition between these archaeological positions.

20. It is sometimes suggested that if unificationist ideals were realized, the resulting body of scientific knowledge could not be subjected to systematic empirical evaluation: "the success of a Grand Unified Theory in contemporary physics would make science untestable (or only circularly testable)" (Stump 1991: 468). If, however, the broad outlines of Glymour's bootstrapping model of confirmation captures the practice typical of even a few cases (i.e., one need not embrace Glymour's more expansive claims), this consequence may not follow; see chapter 13 for an account of his theory in application to archaeology.

21. I construct this hypothetical example using the examples of physical dating and materials analysis that appear most often, in this connection, in L. Binford's discussions of epistemic independence, conceived here as two components of the evidence relevant to an archaeological problem like that posed by Allchin's dilemma (as discussed in chapter 3; L. Binford 1968a: 18).

22. To put this point in more general terms: the credibility of a hypothesis is enhanced far beyond the simple addition of another piece of evidence insofar as it is implausible that multiple lines of support could arise accidentally or as a result of compensating errors that compromise each line of evidence.

23. It is important to note, however, that the calibration of C14 dates depended on tree ring and design sequence dating. As Renfrew describes the debate that informed the calibration of C14, one of the questions central to this debate concerned the independence of tree ring sequences of the long bristlecone pine, given their high altitude, from variations in the rates of breakdown of C14 that they were being used to calibrate (1973b: 89–90). If these worries had been borne out, the strategy of relying on one line of evidence to correct another would have been confounded unless (as was the case) it was possible to counter the threat of com-

pensating error by using still other dating techniques (e.g., archaeomagnetism, varve, and other methods of geological dating) to cross-check and refine the accuracy of the calibration curves.

24. For an account of the differences between the evidential use of documentary records and to material culture as they arise within archaeology, see Patrik (1985).

25. In my formulation of these arguments for an archaeological audience (see n. 1 above), I drew the following conclusion:

> I urge a skeptical attitude toward claims about the scientific status of archaeological practices that depend on appeals to unifying features of science. It is important to think systematically, even globally, about the ways in which archaeological inquiry is embedded in an extended network of integrating and fragmenting relationships with other fields of inquiry (by no means all or only scientific fields). But where arguments of justification are concerned, it is crucial to act locally; it is the details of interfield relations that count in assessing epistemic independence, not the affiliation of a particular line of inquiry or method or set of auxiliaries with a corporate entity we valorize as science. The alternative to this admittedly uncertain and defeasible strategy of argument is not the security of self-warranting foundations and logical necessity. It is a dogmatic narrowing of horizons that is profoundly divisive and that undermines the one Enlightenment ideal that survives scrutiny: that of holding practice, as well as belief, open to revision in light of experience. (Wylie 2000b: 234)

CHAPTER 16. UNIFICATION AND CONVERGENCE IN ARCHAEOLOGICAL EXPLANATION

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1. Kitcher and Salmon (1989), Pitt (1988), and Ruben (1993a) assembled anthologies on explanation and provide overviews of the recent history of the postpositivist debate about explanation. Kitcher (1995) describes causal and unificationist options as the two main (philosophical) approaches to understanding explanation that have emerged in response to the problems of asymmetry and irrelevance identified in protracted debates over the problems inherent in Hempel's deductive-nomological (D-N) and inductive-statistical (I-S) models. W. Salmon (1984, 1989) identifies three broad categories—modal, epistemic, and ontic—that subsume the causal and unificationist theories (variants of the ontic and epistemic conceptions of explanation, respectively) that will concern me here, as well as the pragmatic theories I mention in passing.

2. Kitcher adds, in a note, that it may be "entirely possible that a different system of representation might articulate the idea of explanatory unification by employing the 'same way of thinking again and again' in quite a different—and possibly more revealing—way than the notions from logic I draw on here" (1989: 501 n. 18).

3. Robustly realist variants of this approach were articulated in the 1970s by Harré (1970) and Bhaskar (1978), among others (see Keat and Urry 1975), who insisted that the central aim of science is not systematization that affords explanation as a derivative virtue (as empiricists maintain), but rather explanatory modeling of underlying causal mechanisms.

4. Salmon developed this causalist account in response to difficulties that overwhelmed the statistical relevance model he had earlier proposed as an alternative to refined versions of the nomic covering law model. On the SR model, explanations are not arguments but simply accounts that identify factors, variables, that make a difference to the likelihood that the events or properties requiring explanation will occur. This shift was marked by his 1978 presidential address to the American Philosophical Association, "Why Ask 'Why'?" In it, he argued that concepts of statistical signifi-

cance are not sufficiently rich to capture what we mean by explanatory relevance; as he and Merrilee Salmon later put this point, an adequate understanding of explanation requires that we "put the 'cause' back in 'because'" (1979: 72).

5. Renfrew observes that "for more than two centuries," since Sir William Jones's famous paper of 1786 (1988: 437), historical linguists have recognized these affinities as puzzling, and sometimes he gives the problem a historical formulation: "If we look at the distribution of Indo-European languages in Europe when we first see them in the centuries shortly before or after the beginning of the Christian era (or, in the case of Greece, a thousand years earlier), virtually the whole of Europe seems to have been Indo-European-speaking [by 2,000 to 3,000 years ago]. . . . This is a vast area for such a degree of uniformity" (1987: 145).

6. Note the parallels with Binford's argument against Bordes's interpretation of Mousterian assemblages (L. Binford 1972b; see chapter 7 above).

7. Indeed, Anthony argues that "to agree with Renfrew, archaeologists must dismiss most of what linguists have learned about the PIE [Proto-Indo-European] lexicon in the past 200 years" (1996: 36; see also Anthony and Wailes 1988).

8. This estimate is disputed by various of Renfrew's critics; for example, Zvelebil and Zvelebil argue that the Neolithization of Europe is more likely to have taken 3,500 years, given the available archaeological evidence (1988: 578; see also 1990).

9. On the question of original colonization, Renfrew appeals to the "out of Africa" monogenesis hypothesis, according to which contemporary human populations are all descended from a species of modern humans that "emerged in Africa about 100,000 years ago," displacing earlier hominid forms as they diffused out of Africa; he sets the extinction of other protohominid forms at about 35,000 years ago (1992b: 12). Because these modern humans are presumed to have had a capacity for speech and symbol manipulation that earlier hominids did not, this species diffusion is characterized as the primary episode of initial (linguistic) colonization. A series of other (later) initial colonizations took the form of post-Pleistocene circumpolar dispersals; these account for the distribution of four macrofamilies in the arctic and subarctic and into Austronesia.

10. Renfrew (1992b) argues that his demic-diffusion model can be applied directly to at least three major language groups (Indo-European, Afro-Asiatic, and Elamo-Dravidian), and with some modification to several others (Niger-Kordofanian [Bantu] Austronesian, Sino-Tibetan).

11. Although Renfrew is sympathetic to the programmatic claims of New Archaeologists who invoked Hempelian covering law models of explanation, he is generally impatient with philosophical debates within archaeology (see his critique of "isms," Renfrew 1982a), and there is no indication that he is familiar with, or has been influenced by, these postpositivist theories of explanation.

12. The arguments Salmon describes as causal turn on this sort of convergence argument (1989: 152). In this connection he refers to Hacking, who discusses in some detail just the sort of miracle argument I describe here and throughout part IV (discussed by W. Salmon 1989: 153; Hacking 1981: 317).

13. See, for example, Anthony's summary of recent work in historical linguistics. He suggests that a complex evolutionary tree for Indo-European languages and a sequence of splits from Proto-Indo-European must be postulated to account for different kinds and degrees of affinity between the resulting daughter languages (1996: 38; see also Anthony and Wailes 1988).

14. Anthony postulates two episodes of migration and cultural diffusion (not invasion) by horse-mounted pastoralists, the Yamna culture, between 3100 and 2200 B.P., emanating from the western steppes and the Volga-Ural region (1996: 38–39).

15. Note a parallel between this line of criticism and more general arguments against the standard wisdom that "the response of a large interactive system [must be] proportional to the disturbance" that provokes the response—the events or states of the system invoked to explain the response (Bak and Chen 1991: 46). The advocates of "self-organized criticality" suggest that a range of complex natural and social systems may be better understood by starting with the assumption that if they are "weakly chaotic," they have a capacity to "perpetually organize themselves to a critical state" (52, 46) in which quite minor events can set off chains of interactions that have dramatic (even catastrophic) effects.

16. See, for example, the argument B. Smith has made regarding the development of agriculture in the Americas. In many (perhaps most) contexts, a developmentally complex transitional period involving mixed-strategy subsistence lasted many thousands of years (e.g., 6,000 years in Mexico). He argues that it is a mistake to treat this "'in-between' territory" as a "processually brief transitional interlude separating the steady-state solutions of hunting-gathering and agriculture" (1998: 1651).

17. In some areas of Europe there was apparently a quite rapid transition to organized mixed farm-

ing; in others, local sequences indicate that "agro-pastoral farming was added to the existing patterns of resource use by *the indigenous populations*" and was not associated with population movement or displacement (Zvelebil and Zvelebil 1988: 578, emphasis added; see also Barker 1988: 448). In still other areas, farming groups seem to have lived side by side with indigenous hunter-gatherers for long periods of time without having much impact on the subsistence practices of the latter; indeed, in many cases these foragers and farmers seem to have been mutually interdependent. This pattern of nonconversion/nondisplacement, or of long-delayed intensification and diffusion of farming, was not at all unusual. In the Americas, maize cultivation was viable long before it was intensified to become a transforming staple of life and diffused (unevenly) northward. In southern Africa, Bantu-speaking agriculturalists evidently lived in close, symbiotic proximity with Khoisan gatherer-hunters for several thousand years without the latter being displaced (linguistically or in subsistence practice). In many areas, the transition to farming was accomplished only with the expansion of imperialist and more recent capitalist powers, where the factors responsible for the diffusion of farming technologies (and, in some cases, associated languages and other cultural traits) are by no means reducible to agriculturally induced demographic pressure.

18. Ironically, the critics who raise these questions turn back on Renfrew's own model a version of his central objection to the elite dominance hypothesis: they ask whether farming technologies, language, and populations are so tightly interdependent that they must be assumed to diffuse or to change together.

19. Coleman continues: "most serious of all is the temptation, whenever a new model is developed, to apply it to the exclusion of all others" out of a zeal to compensate for, or overcome, the perceived inadequacies of existing models to account for a particular group of observations (1988: 451). See also Sherratt's objection that Renfrew's approach can "justly be described as Procrustean in that it consists of lopping off those reconstructions which do not conform to a small number of preconceived models[.] . . . [T]he answers which are finally proposed are essentially large-scale versions of the migrations sought by an earlier generation of scholarship" (1988: 459).

20. Alternatively, Renfrew observes that the role of an abstract demic-diffusion model such as Ammerman and Cavalli-Sforza's (and Renfrew's) is to offer "an intelligible mechanism by which a basic pro-cess can be understood" (1988: 463). He makes this statement while observing that the wave-of-advance model "was formulated by Ammerman and Cavalli-Sforza for a well-defined general case (involving an anisotropic landscape and a homogenous population of farmers) such as could never exist in the real world" (463).

21. I am here referring to three of the twelve types of false models that Wimsatt describes as functioning to generate "truer theories" (1987: 30–32).

22. See, for example, Cartwright's summary of discussions of idealization (1989: 354) and Kitcher's assessment of their implications (1989: 453).

23. Renfrew is inevitably negotiating a trade-off between theoretical virtues that is familiar throughout the social and life sciences; for example, as Levins declares, "there is no single, best all-purpose model[;] . . . it is not possible to maximize simultaneously generality, realism, and precision" (1968: 7). I am grateful to James Griesemer for directing me to Wimsatt's and Levins's discussions.

24. To anticipate the argument that follows, the factors of technology, subsistence, and demographic pressure are collectively the *deus ex machina,* as Barker refers to them (1988: 449), typical of the genre of explanation in archaeology associated with the processual or New Archaeology.

25. For a parallel argument concerning research in the life sciences, see Longino (1994: 476–479, and 1990; discussion in Wylie 1995a).

CHAPTER 17. ETHICAL DILEMMAS IN ARCHAEOLOGICAL PRACTICE: THE (TRANS)FORMATION OF DISCIPLINARY IDENTITY

The catalyst for this analysis of ethics issues in archaeology was the extended debate and discussion in which I was involved as a consequence of serving on the SAA Committee for Ethics in Archaeology; I thank Mark Lynott (who co-chaired this committee) and everyone involved in the work of the committee for their generosity as interlocutors and for the example they set in engaging difficult issues with such integrity and conviction. More formally, the research that resulted in this essay was supported by the Social Sciences and Humanities Research Council of Canada, and it

was completed while I was a fellow at the Center for Advanced Study in the Behavioral Sciences (Stanford, California). I am grateful for the support provided me while at the center by the University of Western Ontario and by the Andrew J. Mellon Foundation.

1. Elsewhere (Wylie 1999a) I trace three lines of development that have shaped current ethics debate and codes of conduct in archaeology: pressures to professionalize, concern with conservation issues (especially in face of commercial destruction of the record), and demands for public accountability. In that context, as a baseline for assessing the response of the SAA to these issues I use a report on the state of professional codes of conduct published by the American Association for the Advancement of Science (AAAS) in 1980 (Chalk, Frankel, and Chaffer 1980).

2. I quote Dixon's full statement in chapter 1: "The time is past when our major interest was in the specimen. . . . We are today concerned with the relations of things, with the whens and the whys and the hows" (1913: 565).

3. With G.I. Bill support, the demography of archaeology in the United States changed dramatically in the postwar years. Patterson reports figures indicating that the membership of the SAA and its sister societies doubled between the late 1930s and the early 1960s (1995b: 81–83).

4. See, for example, the heritage legislation of the 1970s that superseded the Antiquities Act of 1906 in the United States: the Archaeological and Historical Preservation Act of 1974 and the Archaeological Resources Protection Act of 1979 (reprinted as appendix B in Vitelli 1996: 266–271). The impact of this legislation has been enormous. As Stark describes it, in a review of "current trends in archaeological funding," "archaeology is no longer the exclusive domain of the scholar"; she reports that "the biggest single direct employers of archaeologists today are federal agencies and engineering firms" (1992: 53, 47). She estimates that federal spending on archaeology in 1986 amounted to $78.4 million, while the combined budget for archaeological research funded by the National Science Foundation and the Wenner Gren Foundation was $6.2 million: "over 20 times as much money is allotted to CRM as to institutional or academic research" (49). These figures are confirmed and refined by Zeder in a detailed analysis of survey data collected by the SAA in a census of its members. In the five-year period before 1994 (the year the census was taken), SAA respondents reported $300 million invested in CRM projects, with the majority of these funds secured by just sixty-three private-sector archaeologists (Zeder 1997: 200). Not surprisingly, Zeder found that archaeologists in all sectors (in academia and museums as well as government and the private sector) are now pursuing CRM funding.

5. In fact, there are several points at which the authors of the SOPA code of ethics seem to acknowledge conflicts between the contractual obligations of professional archaeologists and a commitment to scientific goals. For example, professionals are enjoined "not to enter into a contract which prohibits the archaeologist from including her or his own interpretations or conclusions in the contractual reports, or from a continuing right to use the data after completion of the project," although the preface to this clause also states that "contractual obligations in reporting must be respected" (SOPA 1991: 10). Elsewhere it is noted that the requirement to follow a "scientific plan of research" will always be open to qualification "to the extent that unforeseen circumstances warrant its modification" (9), a consideration that applies to all archaeological practice but seems especially salient where the exigencies of contract research are concerned.

6. A commitment to conservationist goals also figures in the SOPA code of ethics as the second item listed in an initial section on the professional archaeologists' responsibilities to the public: "an archaeologist shall . . . actively support conservation of the archaeological resource base" (SOPA 1991: 7). And it appears in the section titled "Standards of Research Performance," where the SOPA code requires professionals to develop projects in such a way as to "add to our understanding" while at the same time "causing minimal attrition of the archaeological resource base" and ensuring an "economic use of the resource base" (9).

7. This pessimistic assessment has been reiterated with increasing urgency by many subsequent authors. In 1991 S. Harrington concluded a special feature on looting that appeared in *Archaeology* with the observation that as the destruction of archaeological sites continues unabated, "archaeologists will worry that 98 percent of all (not just currently known) archaeological deposits dating to before the year A.D. 2000 will have been destroyed" (1991: 30).

8. Lipe offers five "positive arguments about the value of archaeology to society," four of which have to do with its potential to provide important anthropological and scientific insights about the cultural past, insights that promise "contemporary [people] with a vital perspective" on contemporary cultural life and on the scale and instability of human cultural history (1974: 217–19). In a sub-

sequent article, "In Defense of Digging" (1996), Lipe reinforces this emphasis on scientific goals and qualifies the most radical implications of his earlier argument for a conservation ethic. He argues that the principles of a conservation ethic should not be interpreted so stringently as to prohibit the use of destructive techniques like excavation on any but salvage projects.

9. See also the "Code of Conduct" adopted in the United Kingdom by the Institute of Field Archaeologists (1988).

10. The SHA is a smaller and younger society than the SAA, to which many of its members also belong. By contrast, the AIA represents a broad range of archaeological interests that are not North America–specific, particularly archaeology in the old world traditions of art history and classics with which anthropological archaeology is frequently contrasted.

11. The SHA endorses the goals of scholarly research (SHA 1992: 32), and the AIA "encourag[es] and support[s] archaeological research and publication" (AIA 1991: 285), but neither makes *scientific* goals the defining interest of responsible archaeological practice.

12. The AIA includes, in its guidelines for the submission of manuscripts, the statement that the official journal of the AIA, the *American Journal of Archaeology,* "will not serve for the announcement or initial scholarly presentation of any object in a private or public collection acquired after 30 December 1973, unless the object was part of a previously existing collection or has been legally exported from the country of origin" (AIA 1991: 285).

13. For prescient discussions of how these pressures were already transforming research practice by the late 1970s, see Paynter (1983) and Lacy and Hasenstab (1983). See also Mayer-Oakes (1982) for more general discussion of the implications of restructuring archaeology as a "client-oriented" profession.

14. Alexander (1980: 1074) refers to Carl Nagin, a critic of Donnan's practices who describes him as involved not only with collectors but also with dealers and smugglers of antiquities. In rebuttal, Donnan rejects the suggestion that he has been "severely criticized by the media," insisting that Nagin's is the only criticism in print (1991: 498). In *Lords of Sipán: A Tale of Pre-Inca Tombs, Archaeology, and Crime,* Kirkpatrick (1992) describes, for a popular audience, the complex web of interconnections between collectors, dealers, smugglers, heritage officials, customs officers, and archaeologists in which, he suggests, Donnan is implicated.

15. Alva has developed an impressive program of community archaeology designed not just to enlist local support but to address the conditions that make looting a critical source of income for many in the region: "the poverty that permeates the lives of the *campesinos* in this region is easy for the traffickers to exploit" (Alva 1995: 20).

16. In a letter to the editor published in the same issue of *Science* as Donnan's rebuttal, R. Adams commends Alexander for his critical treatment of Donnan's use of looted data and cites two further reasons for not condoning such practices: first, most looted data have lost "90% of [their informational] value by the time they reach a collector"; and second, the SAA's bylaws prohibit members from engaging in activities that "may promote the commercial value of artifacts" (1991: 498). The first principle captures the rationale for not participating in the market for antiquities, articulated in the 1961 SAA statement on ethics, while the second is actually closer to the wording of the AIA code, which specifies that members should "refrain from activities that enhance the value of [illegally traded antiquities]" (AIA 1991: 285); the SAA endorses a similar policy in its bylaws but, as indicated, it requires members more generally to "discourage commercialism and work for its elimination" (SAA 1995: 17). I share Adams's appreciation of the intent of the SAA's bylaws and the earlier "statements" but was struck, when I reviewed these ethics policies, by how ambiguous they are in crucial areas: e.g., on questions about what counts as commercialization (or, as "promot[ing] the commercial value of artifacts") and what follows for archaeological practice if privately held material does retain informational value. It was reflection on this letter that stimulated the analysis I present here of the consequentialist rationale underlying Donnan's "salvage principle."

17. Lynott has argued that there are lessons to be drawn for archaeology from debate over the ethics of citing and otherwise using medical data derived from Nazi concentration camp experiments (1997: 595–596). In a discussion of issues raised by the use of hypothermia data (Moe 1984), and in a subsequent Hastings Center "Case Study" (1989), some contributors to this debate make a case for publishing Nazi data under certain conditions: only if the data themselves are sound, which is a condition Nazi data largely do not meet (Altman 1990; Arnold Relman, as quoted by Moe 1984: 6); only if it is crucial to questions central to current research and there is no other source of data that could be used to address these questions (Moe 1984: 7); and only if the authors explicitly and in every publication address the fact that they are using Nazi data. More specifically, Moe argues that

any use of Nazi data must be accompanied by an explanation of the reasons for deciding to use it and a clear condemnation of the means by which they were collected; "a decision to use the data should not be made without regret or without acknowledging the incomprehensible horror that produced them" (1984: 7). In later commentary on this recommendation, Sheldon and Whitely consider the complexities of meeting this last requirement in a way that does not become merely pro forma and does effectively "sustain a sense of condemnation that keeps alive the memories of the victims and fights against a future that replicates the past" (1989: 17). Others conclude that no acknowledgment or rationalization can justify the use of Nazi data: "to use this 'data' is to give it, beyond credence, honor. . . . [W]e must not add our numbers to the multitudes of onlookers who slept peacefully through the nights of anguished cries while dreaming their sweet dreams of a better tomorrow" (Gaylin 1989: 18).

Lynott is careful to mark the disanalogies between the issues raised by the publication of looted archaeological data and those central to the debate generated by the use of Nazi medical data (1997: 596), but suggests that Moe's recommendations might be applicable in archaeological contexts. If an author concludes that the publication of looted data is warranted (perhaps on the basis of a version of the doubly conditionalized salvage principle I have described), then following Moe's guidelines he or she should be required to make explicit the sources of these data, to justify the decision to use them (in terms of the integrity of the data, their relevance to critical research questions, and the lack of any other sources of information that could be used to address these questions), and to make discussion of the destructive costs of looting and commercial trade in antiquities an integral part of his or her publication of the data. Even so, Lynott concludes that "as an ethical ideal, the use of looted data in research and publication should be avoided" (1997: 596).

18. See, for example, S. Harrington's discussion of the conditions under which the trade in antiquities operates (1991: 29).

19. Indeed, as Gill and Chippendale argue with respect to Cycladic antiquities, published provenance may prove to be quite plastic, shifting substantially as items are bought and sold (1993: 621–622).

20. See, for example, the debate over the involvement of the Oxford University Research Laboratory for Archaeology and the History of Art in dating and authenticating commercially traded terracotta fig-

urines from West Africa (Mali). Sharp criticism of this practice resulted in a resolution that now limits the material the laboratory will handle to specimens recovered in the course of "lawful archaeological excavations" and accompanied by "a verifiable certificate of export from the country of origin specific to the object," specimens held by recognized museums if submitted with full documentation of acquisition history, and specimens subject to litigation that involves the police or public prosecutors (Inskeep 1992: 114). The resolution also explicitly states that no authentication or dating will be undertaken for private individuals, dealers, commercial galleries, auction houses, or salesrooms.

21. Gill and Chippindale's analysis of the commercial and academic trade in Cycladic figures (1993) is especially telling in this connection. In this case, like that of Ban Chiang ceramics, a sudden growth of market interest precipitated massive looting of Cycladic sites (especially burials). With no reliable chemical or other method for distinguishing authentic from fake figures, the potential for (and the likelihood of) fraud has been enormous. The routine publication of privately collected Cycladic figures has resulted in a research assemblage in which so few figures have well-established provenance that it is virtually impossible to determine the authenticity of new figures when they appear on the market. Reviewing the corpus of published Cycladic figures, Gill and Chippindale estimate that, at best, 10 percent have secure provenance. This throws into question the elaborate comparative analyses based on this material by art historians who specialize in the study of Cycladic art. Gill and Chippindale conclude that there is no empirical basis for identifying "master sculptors" and regional schools (1993: 627–631). Elia (1993) summarizes Gill and Chippindale's analysis in a review in which he condemns the high-profile publication, by no less influential an archaeologist than Renfrew, of Cycladic material held in a prominent private collection (the Goulandris collection). He objects that Renfrew's collaboration with collectors legitimates precisely the commercial and aesthetic interests that are responsible for destructive looting. By contrast with Donnan's response to Alexander, Renfrew takes Elia's point in a reply titled "Collectors Are the Real Looters" (Renfrew 1993b).

22. See also Halsey (1991); Chase, Chase, and Topsey (1988); Kleiner (1990); Messenger (1989).

23. This memorandum was the outcome of legal proceedings brought against Clifford early in the project and was signed by a number of oversight commissions and government bodies concerned about

the conduct of the project: the U.S. Army Corps of Engineers, the Advisory Council on Historic Preservation, and the Massachusetts Historical Commission (Elia 1992: 106).

24. See, for example, Riess (1986); Beaudry (1990); Bradley (1990); Elia (1990); King (1985); Lees (1985); and the response from Hamilton (1991; see also 1995). Elia includes in his analysis of the case a provocative selection of statements made by whose who collaborated with Clifford in which the terms of a Donnan-type salvage principle are explicit (1992: 107–108). Another dominant theme in arguments for collaboration, which I do not consider here, is that insofar as Clifford did (eventually) secure a permit to salvage, the activity is not illegal (Hamilton, as cited by Elia 1992: 108).

25. In addition to this general principle Hamilton could also cite section 2 of the "Ethical Positions" adopted by the SHA as part of its constitution: "The society supports the dissemination of research results within its own profession, to other disciplines, and to the public . . . [and] encourages its members to communicate the results of research, without undue delay, to appropriate colleagues, employers, clients and the public" (SHA 1992: 36, article VII)

26. Elia notes a sobering consequence of the legitimation of commercial salvage that he sees as a direct outcome of professional involvement in the *Whydah* project: viz., that commercial salvors have begun to secure public funds for CRM projects, the results of which they can then use as a basis for applying for permits to conduct commercial salvage operations. In this case, the wrecks that the CRM process was intended to identify and protect are made vulnerable to commercial exploitation by publicly funded investigation:

> In late 1990, Clifford's marine salvage firm was awarded a public contract to conduct a CRM project—an underwater archaeological excavation of a portion of Boston Harbor that will be disturbed as part of a multibillion dollar cleanup of the harbor. . . . Public money is thus being paid to a marine salvage company in order to conduct an archaeological investigation that is required by federal preservation law. The unfortunate irony is that the salvage firm might discover underwater sites and, after the state agency avoids the sites in its construction (which is its stated intention), under state law there would be nothing to prevent the salvors from filing a permit to salvage the sites. (1992: 113–114)

27. In fact, as Fagan points out, we have little systematic understanding of the effectiveness of different strategies for countering the trade in antiquities.

Refusing to publish looted data may be crucial in some contexts but irrelevant in others; museum exhibits that condemn looting may deter some collectors but have no impact whatsoever on others or on the dealers and "acquisitors" for whom archaeological material is just a commodity. Some looters and dealers may be educable or, indeed, can afford to treat archaeological sites as a common heritage rather than a desperately needed economic resource, while others are irrevocably cynical and self-serving or have few other options for survival. Fagan recommends a diversion of at least some resources to research on questions about the psychology and political economy that drives the antiquities trade and that informs public response to it. For examples of research that provides important insights into the conditions under which looting is conducted, see Heath (1973); Staley (1993); Matsuda (1996, 1997).

28. The literature on these issues is growing rapidly. The discussions and overviews on which I rely include Downer (1997); Echo-Hawk (1993); Goldstein (1992); Goldstein and Kintigh (1990); Hubert (1989); Layton (1989a, 1989b); McBryde (1985); McGuire (1992); Pyburn (1999); Riding In (1992); Trope and Echo-Hawk (1992); J. Watkins (1999); World Council of Indigenous Peoples (1990); Yellowhorn (1996); Zimmerman (1989, 1992).

29. This last was a case described to me by an archaeologist working for the Ontario Ministry of Culture, in a discussion of an earlier draft of this paper presented at the Westminster Institute, the University of Western Ontario, in 1993.

30. An early and influential formulation of these critiques, directed at anthropology generally, appears in *Custer Died for Your Sins: "Anthropologists and Other Friends"* (Deloria 1969: 78–100). As Downer writes of conflicts between archaeologists and Native Americans, "Native Americans . . . simply could not believe that scientific curiosity was sufficient justification for the desecration of the graves of their ancestors. Nor could they believe that anyone, let alone archaeologists—who, after all, traced their intellectual lineage to the founders of American anthropology—could . . . so thoroughly dehumanize and *objectify* the people they studied" (1997: 23–24). See Thomas (2000) for a detailed account of the history of interaction between Native Americans and archaeologists that prefigures the contemporary debate. For discussion of parallel demands for accountability that have arisen in connection with indigenous and aboriginal activism outside North America, see Wylie (1999a: 327–330).

31. For a more recent formulation of these concerns, see McManamon (1991).

32. The first world archaeologists who have played a role in shaping WAC include many who have made a strong commitment to advocacy for nonarchaeological groups and for political responsibility. Among those from North America, Zimmerman played an instrumental role in the development of the WAC ethics code (see Zimmerman 1989, 1995). See also Hammil and Cruz's account (1989) of a presentation they made to WAC on behalf of American Indians Against Desecration.

33. For more detailed discussion of the impact and implications of external demands for accountability, see Wylie (1999a: 329–334; 2000a).

34. I co-chaired this committee with Mark Lynott. The "principles" described here were drafted at a workshop funded by the National Science Foundation (the "Ethics and Values Studies" section of "Studies in Science, Technology, and Society") and the U.S. National Park Service, and hosted by the Culture Resource Management Policy Institute at the University of Nevada–Reno. Participants included the nine members of the SAA Committee for Ethics in Archaeology and seven advisors to the committee who represented key interest groups and areas of expertise relevant to the issues under discussion (e.g., Native Americans, commercial interests, and representatives of other archaeological societies and committees of the SAA whose mandate overlaps that of the SAA Committee for Ethics in Archaeology). For further detail, see Wylie (1993b, 1994b, 1994c) and Lynott and Wylie (1995).

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