

## CHAPTER I

### Introduction

We shall not cease from exploration  
And the end of all our exploring  
Will be to arrive where we started  
And know the place for the first time.

—T. S. Eliot<sup>1</sup>

In this dissertation, I explore a selection of recent work in the philosophy and history of experiment, with an eye toward reformulating its focus and redirecting its future path. Specifically, I re-examine a traditional problem in the philosophy of experiment: how to make sense of scientists’ attempts to separate experimental “signal” or “entity” from background “noise” or “artifact.” This aspect of the analysis of the *practice* of scientists—the day to day task of getting one’s experimental equipment, techniques, and theoretical commitments to yield reliable results that will be accepted by prevailing scientific standards—requires further modifications in order to be made compatible with an adequate notion of historiography and with a philosophically and historically tenable view of the epistemology of science. I show that the concept of *narrativity* is a crucial, if not primary, construct in answering these questions about interpreting experimental practice. Particular historical narratives, and the historiographies that guide their construction, constitute the crucial evidence for any legitimate view of the epistemological and cultural significance of scientific experimentation. However, narrativity and historiography must be deconstructed before their conceptual significance for experimentation can be evaluated adequately. The metahistorical construct I implement in order to situate appropriately questions concerning scientific experimentation, one that meets the necessary theoretical requirements, is *the technological infrastructure of science*.

Joseph Pitt’s (1993a, 1993b, 1995, 2000) concept of the *technological infrastructure*

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<sup>1</sup> “Little Gidding,” (1942) lines 239-242, in *Four Quartets* (©1943, 1971), San Diego: Harcourt, Inc., p. 59; reprinted by permission of Harcourt, Inc. For more of T. S. Eliot’s (1888-1965) poetry, see *Collected Poems, 1909-1962*, New York: Harcourt Brace & Company, ©1963.

*of science* provides the theoretical foundation for my analysis of experimentation. In this dissertation, I extend and refine Pitt's concept of technological infrastructure in order to create a metahistorical tool that researchers in many fields, including Science and Technology Studies (STS), Philosophy of Science, Philosophy of Technology, Cultural Studies (of Science and Technology), History of Science, and History of Technology, may utilize when analyzing experimentation. As such, the technological infrastructure construct is a hybrid creation, one that draws on the work of many scholars working in several different fields. In addition, it has theoretical requirements that draw on scholarship from different fields, and the researchers in these fields do not always agree on even the most foundational issues, even if they share professional goals. Hence, I intend the technological infrastructure to be an alternative to previous attempts at specifying a broad theoretical construct for evaluating science, an alternative that incorporates some of the best features of previous attempts, yet rejects those that are untenable. To this end, I develop the technological infrastructure as an incorporation, extension and/or replacement of, for example, Kuhn's (1970) "disciplinary matrix," Latour's (1987) "network," Galison's (1987) "short-, middle-, and long-term constraints," Hacking's (1992) "coherence of thought, action, materials, marks," Rheinberger's (1992a, 1992b, 1994) "experimental system," Pickering's (1995) "mangle of practice," and Burian's (1996) "interaction of mechanisms, of structures and functions, at a great many levels."

Pitt (2000) defines the technological infrastructure of science as "*the historically defined set of mutually supporting sets of artifacts and structures without which the development and refinement of scientific knowledge is not possible*" (p. 122). One way to flesh this out is to view a technological infrastructure as a combination of material and social culture. By social culture, I mean not only social structures, institutional or personal power relations, and interests—but also things such as statistical methods, experimental techniques, and scientific theories. By material culture, I mean not only machines, but also the natural world, and this includes the materials, entities, and phenomena of experiments in science.<sup>2</sup>

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<sup>2</sup> Although I employ the term *technological* in specifying the construct "the technological infrastructure of science," I do not suggest that the traditional notion of "technology," i.e., machines and tools, is what is central to this discussion. Pitt (1995) defines technology as "humanity at work" (p. 5), and I here employ the term

For Pitt (2000), a rather strong thesis arises out of analyses of scientific and technological change, the technological infrastructure thesis: “[P]rogress in science is a direct function of increasing sophistication not merely in instrumentation, but in the technological infrastructure that underlies and makes mature science possible” (p. 123). Pitt argues that “the development of new information in a mature science is, by and large, a function of its technological infrastructure” (p. 125). Pitt believes that the technological infrastructure construct can provide us with an alternative to scientific realism on the one hand, and the extremes of social constructivism, on the other hand (ch. 8). Furthermore, it can bring technology into discussions of scientific change in a way that has been neglected. However, a crucial issue for Pitt, and for this dissertation, is what is normally termed “reality.”

A current issue in analyses of science and technology is what to make of the natural world—reality (cf. Grene 1985, Latour 1993). Since the works of Hempel (e.g., [1945] 1965b, [1950-1] 1965b, 1965a), Goodman (1951, 1955), Quine (1951), Sellars (1956), Hanson (1958), Feyerabend (1962), Kuhn (1962), and others, which questioned positivist principles for studying science and/or modeling scientific change (e.g., Carnap 1936-7), many researchers have rejected positivism as an adequate account of scientific change, and the positivists were no realists. Post-Kuhnian scientific realism has fared no better; one sees few attempts to rehabilitate the realist arguments of the 1970s and 1980s (cf. Fine 1984). And now many even in the sociology-dominated Science and Technology Studies community, long permeated by the paradigm of the Sociology of Scientific Knowledge (SSK), are declaring the death of social constructivism.<sup>3</sup> What these positions have in

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‘technology’ in a similarly broad fashion. Hence, this dissertation is not primarily about traditional notions of technology, nor will it concentrate on evaluating specific machines, as in the discipline of the History of Technology. It is about re-conceptualizing our notions of technology, especially as they relate to science. Perhaps, as Marjorie Grene suggested to me at a seminar in 1996, I should use the term ‘technique,’ for it seems more to capture the notion of humanity at work. However, I prefer ‘technology,’ because it brings the focus of scientific change on traditional views of technology, and this I believe is a positive step. Rather than broaden the term ‘technique’ to include the use of machines, I prefer Pitt’s strategy of broadening ‘technology’ to include many kinds of human activities, including statistical methods, policy specifications, experimental techniques, and even narrative strategies.

<sup>3</sup> Steve Fuller’s (1996a) editorial in *Technoscience* is an attempt to argue that social constructivism is still the dominant position among the members of the Society for Social Studies of Science (4S, which considers itself to be the primary organization for the field of Science and Technology Studies), even though a recent meeting in

common is the lack of a coherent position on the natural world and how it should be incorporated into analyses of science and technology. In addition, when considering experimentation, determining what is “real” signal or entity, from artifactual “noise” or impurity, is a practice among scientists that must be incorporated into accounts of science and technology. I argue in this dissertation that the technological infrastructure of science construct, as I re-develop it, will allow researchers to do this. As Pitt (1995) put it, “in this age of increasingly theoretical science, the technology behind the science may be our only contact with reality, and even so it is at best a tenuous one” (p. 3).

In addition to incorporating into accounts of science and technology a coherent notion of the natural world, the technological infrastructure construct also presupposes that historical and historiographical concerns must be taken into account when considering scientific and technological change, in general, and experimental developments, in particular. That is, researchers must grapple with the view that not only is research in the *study* of science and technology a fundamentally *retrospective* activity, but also that *all human activity* has a fundamentally retrospective orientation, and it is subject to a variety of epistemological problems (cf. Rouse 1990, 1996b, Seltzer 1995a, Rheinberger 1994, 1997). As Pitt (1995) argues,

if we want an explanation for the development of science, we need to offer more than a recitation of the sequence of ideas produced by scientists. We need an account of

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Bielefeld, Germany had many proclaiming the “intellectual bankruptcy of ‘social constructivism’” (p. 1). Fuller contends that he “could not help but notice that one of 4S’s less admirable tendencies has returned through the backdoor. Many of the same people [at the Bielefeld meeting] were to be found speaking at the most prominent panels . . . , often saying the same sort of thing they usually say. It is not surprising, then, that over the past few weeks, several people who were present at the meeting (NOT Gross and Levitt!) have proclaimed the intellectual bankruptcy of ‘social constructivism.’ While I think this is an unfair characterization of STS generally, if one only attended the bigger sessions at the Bielefeld meetings, one could easily get that impression. Much of the truly innovative work was tucked away in the smaller sessions that often contained no more than a few postgrads and recent PhDs” (*ibid.*).

Fuller seems to suggest that if STS work goes beyond social constructivism, then it is not innovative. I claim that there has been a turf battle among those in 4S and the wider STS community on basic methodology, and that social constructivism has lost. Furthermore, it is, in part, prominent social constructivists’ attempts to justify their positions to scientists and the general public that has, in part, motivated scientists such as Gross and Levitt (1994) to publish their ill-informed attacks against those who study how science works. Barnes (1991) criticizes the Sociology of Knowledge for tending to adopt an “idealist metaphysic” (p. 333), and he argues that its “over-reaction to realism” (p. 332) amounts to a reification of discourse (pp. 331-3). Rouse (1991b) provides insightful criticisms of Fuller’s “social epistemology.” In addition, Rouse (2002b) suggests that the future of science studies may be “post-constructivist.” Ann La Berge was the first person I heard use the term ‘post-constructivist,’ in 1995 at Virginia Tech (personal communication).

how those ideas were developed and why they were abandoned and/or refined. We are thus dealing with an issue in historiography. An *explanation* of scientific progress and discovery requires appeal to some mechanism. . . . [T]he *mechanism* which makes the discoveries of science possible and scientific change mandatory is the *technological infrastructure* within which science operates. In short you can no longer do philosophy of science, history of science or even sociology of science without the philosophy and history of technology. (p. 10)

Another crucial construct I use in this dissertation is Joseph Rouse's (1993a, 1996a) notion of *epistemic sovereignty*, which helps provide a theoretical apparatus for viewing science as a combination of material and social culture. More than perhaps any other philosopher of science writing today, Rouse (1987, 1993b, 1993c, 1996a, 1996b, 2002a), influenced significantly by Heidegger and Foucault, provides a compelling theoretical blueprint for the future of the philosophy of science in particular, and for those who study science and technology, in general. In his article "What Are Cultural Studies of Scientific Knowledge?" Rouse (1993b) argues that "Cultural Studies of Scientific Knowledge," or CSSK, ought to replace the Sociology of Scientific Knowledge (SSK), on the one hand, and traditional philosophy of science, on the other hand. What distinguishes CSSK from SSK and from traditional analytical philosophy of science is the former's forthright rejection of *epistemic sovereignty* and its willingness to see epistemic matters as intertwined inexorably with matters of politics (p. 6). Epistemic sovereignty, as Rouse (1993a, 1996a) specifies in detail, is the view that the scientific enterprise can be "understood in terms of the legitimation of a unified regime of knowledge" (p. 149). Those who advocate epistemic sovereignty presuppose science can and should be understood from an explanatory framework that is privileged epistemically (Rouse 1993b, p. 4; 1996b, p. 241).

For epistemic sovereigns, then, *some* construct that can be identified—whether from a philosophical, historical, or sociological perspective—is attributed to scientific theory, methodology, or practice. That construct is endowed with the power to legitimate epistemologically the knowledge that results from the utilization of procedures or practices undertaken under the guidance of that construct. Traditionally, in philosophy of science, various logical principles such as verification, confirmation, falsifiability, or testability have served as constructs by which scientific knowledge is legitimated and epistemic sovereignty

is established (see, e.g., Carnap 1936-7, Hempel 1965a). In the philosophy of experiment, researchers have invoked various experimental, statistical, or metastatistical constructs in order to locate epistemic sovereignty, such as controlled experiments, statistical techniques, confidence levels, and even the technical apparatus used to investigate nature (see, e.g., Radder 2003).<sup>4</sup> On this account, once one has identified the construct that will do the knowledge-legitimizing work, one is then in a position—a privileged position—to analyze philosophically, historically, or sociologically an episode in the history of science. Furthermore, the construct that ensures that the condition of sovereignty is safeguarded will do most of the hard work in explaining the episode, for it is what, according to this position, legitimates the resulting knowledge.

Rouse (1993a) argues that there are four main issues that we need to explore when analyzing epistemic sovereignty. These are (1) where a researcher has located epistemic sovereignty; (2) the level at which a researcher has employed epistemic sovereignty; (3) how epistemic sovereignty will purportedly unify knowledge; and (4) what results as the proper form of exercising sovereign epistemic judgment (pp. 148-9). I consider these issues below in my critical assessment of the New Experimentalism and further in the case study on radiation genetics (see Chs. VI, VII, and VIII below). We should note, however, that this discussion of epistemic sovereignty presupposes, or even requires, that we consider historiographical concerns when evaluating science—and this includes experimental science. That is, the problematic of epistemic sovereignty, and hence its rejection, is fundamentally an historiographical exercise, for it bears on how to conceptualize the epistemic/cultural structure of past (and present) science (see Chs. II, III, IV, and V below for a deeper analysis). What Rouse (1993a) is saying is that the starting point for the evaluation of the *practices* involved in the natural sciences must be not only a rejection of epistemic sovereignty, but also an adoption of the position that science (or technology, or whatever) *has no* global epistemic structure. Such a structure is imposed from without; it is not

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<sup>4</sup> I should make it clear here that I am not arguing that scientists who use these statistical techniques, technical apparatus, and experimental designs are doing bad science by virtue of their using these techniques. The argument for rejecting epistemic sovereignty has to do with how we should *conceptualize* the practice of science. This is fundamentally an historiographical and philosophical exercise, yet its results are the basis upon which one could eventually answer questions concerning “good” and “bad” science.

inherent in “nature” or in “history,” but is imposed on history in a retrospective manner by those (re-)constructing particular narratives.

Hans-Jörg Rheinberger (1994) goes further than Rouse in arguing that science has no global epistemic structure. He argues as follows:

Within the tradition of a general history of science, the view of science as a continuous, accumulative process has seriously and lastingly been challenged by the model of a series of more or less radical breaks [e.g., Kuhn (1962) 1970, 1979]. However, [common to] both the revolutionary and the gradual conception of scientific change, is that they assume a global epistemic structure, called “science,” that as a whole either continuously grows—toward truth—or is periodically reconstructed according to a new paradigm. Although there is a heavy dose of relativism in the second view, a paradigm, at a given time, is assumed to have enough power to coordinate and make coherent the activity of a whole—and potentially *the* whole—scientific community. But even in the denial of a continuum of rationality there remains an element of “totalization.” Science remains a normative process encompassing the ensemble of participants and their practices in a common endeavor. And there remains the general view of an overarching chronological coherence to the process of gaining scientific knowledge. (p. 67)

For Rheinberger (1994), science has no such global epistemic structure, and research in the study of science and technology must consider this.<sup>5</sup> What one must do is look carefully at “the microdynamics of scientific activity” (p. 67), and the illusion of science’s apparent “monolithic, macroscopic appearance” (p. 68) will fade (see, as examples, Rheinberger 1992a, 1992b). Instead, argues Rheinberger, we find networks of “experimental systems,” each with its own time structure. Rheinberger (1994, 1997) presents in detailed complexity his view of “experimental systems” and his epistemology of time; I consider these in more detail in Chapter IV below. A salient conclusion Rheinberger draws from his analyses is the following: “There is no global frame of theory or political power, or social context strong enough to pervade and coordinate this universe of merging or bifurcating [experimental] systems” (p. 69). Instead, we should view scientific experimentation as embodied in complex *local* contexts, and how this is carried out, I argue, depends crucially on a careful consideration of historiography and narrative contexts.

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<sup>5</sup> Hacking (1983) made this point quite strongly when he argued that scientific practices are not becoming more unified over time, but are actually fragmenting into more and more specialized sub-practices, each having no tight epistemic relation to the other (see ch. 12, esp. pp. 217-19). For more on this issue, see Ch. V below.

To a large extent, many traditional philosophers of science *and* recent social constructivists share many of the same fundamental assumptions concerning the global epistemic structure of science, and of history and historiography. Both groups generally employ, whether explicitly or implicitly, some construct that presupposes or attempts to ensure science's global epistemic structure.<sup>6</sup> In addition, both groups generally fail to consider issues pertinent to a critical deconstruction of empirical histories (stories) and the historiographical concepts that guide their construction. In this dissertation, I explore these failures and offer the technological infrastructure construct as a concept that satisfies the main historiographical and philosophical criteria that Rouse, Rheinberger, and others set forth.

### **The Focus on Practice**

Recent scholarship that purports to avoid the pitfalls and inadequacies of traditional positivist philosophy of science's obsession with scientific theories focuses on the *practice* of science. This growing movement toward describing and analyzing the workbench activities of scientists has come to the forefront of research on science and technology,<sup>7</sup> and this

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<sup>6</sup> One relevant and recent example of this is Weber's (2005) book on experimental biology. While Weber considers Rheinberger's (1997) work on experimental systems, he explicitly argues that "modern science, even though it exhibits a considerable amount of internal diversity, is a well-defined entity from an epistemological point of view" (p. 2). He then develops a view of biology based on the notion that "general methodological norms" (p. 11) are used in many areas of science, one that suggests that since such epistemic norms run across experimental systems, that a form of "realism about theoretical entities and not directly observable structures" is warranted, based on an inference to the best explanation argument (for a critique of this strategy, see van Fraassen 1989). It is interesting to note that Weber (2005) claims to be indebted to Rheinberger, whose "background in French postmodernist philosophy" has caused him to be "viewed as a radical departure from Anglo-American empiricist philosophy of science." Weber claims his chapter 5 is "an attempt to show how this approach to the analysis of scientific practice could be integrated with empiricist philosophy of science" (p. 306, fn. 4). In addition, Weber claims to be giving a normative naturalist view of epistemic norms (pp. 150-2), yet he ignores Rouse's (2002a) major work on this subject. Ultimately, Weber's (2005) argument is dependent on locating epistemic sovereignty in methodological norms; Rouse's (2002a) work systematically undermines that project.

<sup>7</sup> Some notable examples of influential works that have aided the movement in studies of science and technology toward a focus on practice are Hacking's (1983) *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*; Ihde's (1991) *Instrumental Realism: The Interface between Philosophy of Science and Philosophy of Technology*, with its excellent discussions of Foucault, Heidegger, and Latour; and Pickering's (1992) edited volume, *Science as Practice and Culture*, which includes contributions from Hacking,



research often includes a focus on experimentation. This movement can be conceptualized as a growing debate among three groups (see Table 1, Chapter V, below), which I shall call (1) social constructivists, (2) practice scholars, and (3) epistemic sovereigns. Providing an overview of this debate will help to situate some of the issues central to this dissertation.

First, the Science and Technology Studies (STS) community has long been dominated by an influential group of *social constructivists*,<sup>8</sup> who generally deny any role to nature, or to the natural world, in constraining what comes to be accepted scientific knowledge. For these scholars, all scientific knowledge, all scientific and technical results, are the outcome of processes that are irreducibly social, political, and/or sociopolitical. In other words, there is nothing but the social, and what is to be searched for is the correct social “explanation” of some event in the history of science (e.g., Bloor [1976] 1991; Collins 1985; Collins and Yearley 1992a, b; Collins, de Vries, and Bijker 1997).

Second, partly in response to the extremes of constructivism, there has been the emergence of a group of *practice scholars*,<sup>9</sup> who reject epistemic sovereignty, and who also deny many of the claims of the constructivists. Epistemic sovereignty, again, is the claim that science, by virtue merely of its nature (e.g., its theories, methods, and/or practices), is

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Sharon Traweek, Bruno Latour, Michel Callon, Harry Collins, Steven Yearley, David Gooding, and Steve Woolgar.

<sup>8</sup> In my criticism of social constructivism, I do not intend to indict all those who consider themselves social constructivists. I reserve this criticism for those who defend the view that “there is nothing but the social,” (e.g., Collins and Yearley 1992a) or for those who require that “explanations for the genesis, acceptance, and rejection of knowledge claims are [to be] sought in the domain of the social world rather than in the natural world.” (Pinch and Bijker 1987, p. 18) Bijker and Law’s ([1992] 1997) volume is an attempt to broaden the constructivist methodology and argue that it has compatibility with actor-network theory and systems theory; it includes chapters by Latour, Thomas J. Misa, and Michel Callon. The prototypical social constructivist methodology can be traced at least as far back as Bloor’s (1991 [1976]) *Knowledge and Social Imagery* (see ch. 1, esp. pp. 5-7). Pinch and Bijker (1987, p. 48, fn. 5) claim the methodology can be traced back even farther to Fleck’s (1979 [1935]) *The Genesis and Development of a Scientific Fact*. However, Little’s (1995) analysis of Fleck’s work, including her analysis of the claims of many sociologists of science that they owe debt to Fleck, shows that this claim is at best *post hoc*.

<sup>9</sup> In this category of *practice scholars*, I do not mean all those scholars who attempt to focus on the practice of science, as opposed to the traditional focus on scientific theories. I mean only those who, in addition, deny epistemic sovereignty of any kind, for example, Sharon Traweek (1988), Donna Haraway (1991), Joseph Rouse (2002a), Hans-Jörg Rheinberger (1994), Andrew Pickering (1995), Ian Hacking (1992), and Peter Galison (1987, 1997). There are constructivists who focus on practice, such as Harry Collins (1985), yet they locate epistemic sovereignty in the social realm. In addition, there are neo-positivist empiricists, who focus on practice, align themselves as enemies of the social constructivists, and who locate epistemic sovereignty in some aspect of scientific methodology or practice. This latter group includes Allan Franklin (1990, 1995, 1997, 2002), Philip Kitcher (1993), Deborah Mayo (1996), Wesley Salmon (1998), and Marcel Weber (2005).

epistemically privileged. That is, if one does science in accordance with these methods, one is guaranteed an outcome—scientific knowledge—that puts one in a position that is globally privileged (in some way or another) to say something about the way the natural world is (cf. Pickering 1992). Practice scholars reject this view, yet they want to retain some notion of the epistemic *authority* of science, where epistemic authority is to be distinguished from epistemic sovereignty in that the authority is local and culturally context-bound. Moreover, many practice scholars believe that the material “world,” or “material culture” plays some role in the generation of scientific knowledge, and that scientists have *some* degree of (epistemic/cultural) authority to claim to say things about the way the world is (e.g., Haraway 1991). Practice scholars variously identify themselves as doing “Cultural Studies of Scientific Knowledge” (e.g., Rouse 1993b) or as focusing on the micro-level “practices” of scientists (e.g., Rheinberger 1992a, 1992b), and they question the efficacy of social/historical “explanation,” which is essential to the research agenda of so many of the social constructivists (cf. Traweek 1996, Fine 1996).

Third, there remains in studies of science and technology a group of *epistemic sovereigns*, those scholars (e.g., Franklin 1990, 1997, 2002, Kitcher 1993, Culp 1995, Salmon 1998) who continue willingly to maintain the epistemic sovereignty of science (practice scholars reject epistemic sovereignty; many social constructivists *claim* to reject epistemic sovereignty). In addition, there are those who claim to have an alternative that stands outside of the context of this debate, but who nevertheless advocate a form of epistemic sovereignty (e.g., Fuller 1992, 1996b; Mayo 1996).

In order to review some of the fundamental issues involved in the study of the practice of science that bear on the technological infrastructure construct, it is useful to examine the dynamics of this professional debate. These issues include relativism, postmodernism, global legitimation, reflexivity, and narrativity.

### I. *Relativism*

Some social constructivists have accused, ironically, practice scholars of advocating extreme relativism, and of not being able to say anything about “real life,” or about what

scientists do—that is, the “practice” of science. Similarly, some social constructivists who do not take seriously reflexivity have charged practice scholars with being “hyper-relativist,” and with degenerating into a position that can be described as “conservative.”<sup>10</sup> This results, apparently, because practice scholars are willing to grant *some* authority, however limited and qualified, to scientists’ claims, whereas many social constructivists find this notion unpalatable (e.g., Collins and Yearley 1992a, b). Hence, for the extreme constructivists, it seems *any* granting of authority to the scientific worldview is met with a kind of political challenge (again, all is sociopolitical for so many social constructivists), wherein the views of the practice scholars are compared to the logical positivists or right-wing politicians, against whom the normally left-wing constructivists see themselves battling. It seems that this reaction results in part because many social constructivists ignore reflexivity issues,<sup>11</sup> and this results in their reifying the social realm. As Collins (1994) states:

The battle is between society and the individual, not humans and things. To invite inanimate objects into the debate adds nothing but confusion because inanimate objects cannot be social except when the term is used in such a recondite way as to avoid the crucial issues. (p. 674)

The charge of “conservative” seems to come from the constructivists’ view of the history of the sociology of scientific knowledge (SSK), according to which SSK scholars emerged historically as a challenge to, and ultimately a superior replacement of, the archetypal epistemic sovereigns, the logical positivists and empiricists (e.g., Carnap, Hempel) and post-positivist philosophers of science (e.g., Kuhn, Lakatos, Laudan, Shapere),

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<sup>10</sup> The ironies here abound. When constructivists charge practice scholars with “hyper-relativism,” the ostensible purpose of this to paint them as too “postmodern” and therefore politically conservative, for the constructivists see their work as politically otiose, with little or no relevance for social activism. However, postmodernists have also faced the charge of “hyper-reflexivity.” For example, British Marxist science studies scholars, such as Hilary and Steven Rose, have painted some of their Marxist colleagues, such as Barry Barnes and Paul Feyerabend, as “hyper-reflexive” (Ashmore 1989, pp. 55-57). In this case, the charge is meant to signify that one takes reflexivity *too seriously*, resulting in the destruction of even ideologically desirable scientific knowledge; for an analysis of this in the context of literary criticism and Continental philosophy, see Currie’s (1998) book. However, here the indictment is that one is too left wing, not too conservative. As Ashmore (1989) argues, the Roses’ “rationalistic” (p. 55) position, “which refuses both critique and auto-critique, despite all its radical affirmation, reaches out with unseen hands towards an old enemy” (p. 57). Clearly, whether one who takes aspects of postmodernism seriously is labeled “conservative” or “left-wing” depends upon who is charging whom with ossification, and why.

<sup>11</sup> Ashmore (1989) provides a comprehensive analysis of reflexivity and its role in research in the Sociology of Scientific Knowledge (SSK); see also Woolgar’s (1988) edited volume.

replete with their claims for the epistemological superiority of scientific knowledge. Any granting of authority to science or scientific knowledge the constructivists consider a “backward step,” a retreat away from the advancements of SSK (e.g., Collins and Yearley 1992a, pp. 321-2). In addition, some practice scholars’ uses of some of the tools of postmodern theory are also met with the charge of leading to a politically conservative view of science. While the concept of “relativism,” which many scholars take to be an inevitable and negative result of postmodern theory (cf. Laudan 1990), has been a key methodological tool for SSK theory,<sup>12</sup> many constructivists have been unwilling to relativize *their own* prescriptions regarding work in the study of science and technology (see the discussion of reflexivity below). Hence, when other scholars make efforts to relativize their own work, and when this work offers no explicit political agenda (at least according to the constructivists), the constructivists label such work as “conservative” (cf. Ch. II below).

## II. *Postmodernism*

Some of the extreme views of postmodernists—or is it extreme *readings* of certain postmodernists?—have led some constructivists to construe *any* gesture toward a postmodern worldview (e.g., some of the views of Michel Foucault, Jacques Derrida, Jean-François Lyotard) as an attempt to claim that “everything is text,” or that no judgments can be made about the past (Collins and Yearley 1992, pp. 323-4). Such a theoretical apparatus the constructivists deem “conservative,” since they view it as incapable of offering positive recommendations for social change (cf. the discussion of relativism above). In many cases, however, such charges stem from ignorance of or unfamiliarity with basic aspects of postmodern theories and of historiography (see, for example, White 1991, Currie 1998).

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<sup>12</sup>Harry Collins’s (e.g., 1981, 1983) “Empirical Programme of Relativism” (EPOR) is testimony to the significance of relativism for SSK. According to EPOR, both nature and rationality are not to be taken as self-justifying universals. That is, we should abandon the epistemic sovereignty of the positivists, according to which decisive experimental results are taken to be unproblematic reflections of the natural world. Many social constructivists hold this position, although most of the archetypal positivists, such as Carnap, Schlick, and Neurath, were not scientific realists in the contemporary sense of the term (cf. Coffa 1991). Nevertheless, as I argue here, one kind of epistemic sovereignty is replaced by another: social realism. Collins and other constructivists are quite willing to relativize natural scientific knowledge, but *not* their own sociological knowledge (cf. Ashmore 1989, ch. 4). Collins and Yearley (1992a) unapologetically advocate social realism; they claim that the “big job of sorting out the relationship between [sic: among] cultural enterprises has to be done from the level of social realism. The work can be done from no other level” (p. 309).

For example, many scholars too often have framed the issue of “postmodernism” as a debate between polar extremes of the following sort: postmodernity/modernity; semiotics/representation; irrationality/rationality; textuality/reality; fiction/truth; and others. On the left of each of these dualisms, we have what many constructivists take to be the standard postmodern view: a position advocating that there is no connection between language and the use of language, on the one hand, and anything external to language—including the past—on the other hand. On the right of these dualisms is what many constructivists take to be the outmoded, positivist, modern worldview the postmodernists have sought to replace: a worldview according to which ultimate truth, fixed objectivity, and a given, unalterable past are all possible.<sup>13</sup>

Having judged postmodernism as an extreme hyper-reaction to the excesses of the positivists, many constructivists feel justified in ignoring the many aspects of postmodernism from which they might benefit. One crucial example is the issue of historiography. Subscribing to elements of a postmodern historiography need not result in the position that there is no past, or that history is not fundamentally different from literature (see Ch. II below). There is a “theory of the middle ground,” a theory that provides, according to Gabrielle Spiegel (1992), “the only ground on which . . . history and post-modernism can hope productively to interact with one another” (p. 197).

For Spiegel (1992), postmodern historiography must take into account both the historian’s role as interpreter and chronicler of the past, on the one hand, and the past’s fundamentally “mediated” character, on the other hand. That is, postmodern historiography ought not to result in the view that histories, or texts, refer *only* to other texts, and that *nothing* can be said about the “real” past. However, we must shed permanently the view that histories “transparently reflect reality” (p. 197) and that we can produce objectively grounded

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<sup>13</sup> Cf. Bono’s (1990, pp. 63-7) critical analysis of Richard Boyd’s (1979) distinction between scientific and literary metaphors. For Boyd, scientific metaphors point to real, or potentially real, yet undiscovered aspects of the natural world. Literary metaphors, in contradistinction, point only to other authors and their discourse, and as such say nothing meaningful about the natural world. As Bono (1990) states, this “distinction between literary and scientific metaphors rests, in part, upon a caricature of literature and literary creativity bristling with assumptions that have been undermined by postmodern theorists. The notions of authorship, of the autonomous creativity of the ‘individual talent’ or Cartesian ‘mind,’ have been subjected to close scrutiny and severe criticism by philosophers and literary theorists such as Michel Foucault, Jacques Derrida, Paul de Man, Fredric Jameson, Edward Said, and Stephen Greenblatt” (p. 65).

(in the sense of definitive) historical work (cf. Bono 1990, pp. 59-63). Instead, Spiegel calls for “a shift from the notion that texts and documents transparently reflect past reality, as positivism believed, to one in which the past is captured only in the mediated form preserved for us in language. . .” (pp. 197-8). This does not entail the notion that the past is only that which is produced in historical texts. Spiegel believes that such a view “conflates two horizons of knowledge and action, namely what happened and how we know about it. . .” (p. 201). The historian can still maintain a descriptive (constative) function for historical discourse (pp. 200-202), but the historian will be describing an historical reality from which s/he is culturally/ epistemically/temporally removed. Hence, all such descriptions we must see as circumscribed by cultural, social, and political contexts, contexts we must attempt to reconstruct on the basis of limited evidence and of evidence of limited significance.

Moreover, the historical (or narrative) contexts that historians attempt to represent give meaning and authority to the discursive practices embodied in the textual evidence of the historian (Spiegel 1992, pp. 203-4). Indeed, there is still room here for the material, for *the real*. But, as Spiegel (1992) notes, we still “have no givens—no ready-made chronicle of events or histories—and must construct [our] narratives on the basis of some degree of positive (if ideologically impressed) vision of the past” (p. 202). We can formulate arguments for how and why the texts and other evidence the historian uses reflects the materiality of the past, but these arguments and the representations that result must remain problematized. The problematization must remain, again, because of the indirect, indeterminate connection between the evidence of the past and the past itself. In this sense, the very distinctions between “social and discursive practices,” between “linguistic and material realities,” (p. 206) are blurred, for they are all “interwoven into the fabric of the text, whose analysis as determinate artefacts . . . grants us access to the past” (p. 206). Clearly, notes Spiegel, we must reject the “confident, humanistic assumptions of nineteenth-century positivist historiography,” but we must also

reject the tendencies of an extreme post-structuralism to absorb history into textuality [yet also] learn to appreciate and employ what it teaches us by and in its enactment of the complex tensions that shape the post-modern world. (p. 207)

Just as there are other, more complex options besides materiality and language, there are

other options besides historical reality and textuality; we need not accept the logic of the dualisms modern thinking so often presupposes.

One of the negative results of taking seriously the constructivist position, as I have characterized it above, is that one ends up relocating epistemic sovereignty in the social realm (cf. Latour [1991] 1993). One major reason for this is that the constructivists do not problematize their “empirical” historical narratives. Furthermore, it seems the constructivists realize this and react negatively when confronted (cf. fn. 1 above; Collins 1994).<sup>14</sup> Therefore, we also find social constructivists criticizing practice scholars for not engaging in “empirical” work, but instead recycling the same futile debates.<sup>15</sup> However, as indicated above, one of the crucial factors social constructivists ignore is the very connection between their “empirical” narratives they use as hard evidence for their social theories, and the social determinism they seem to advocate (cf. Rouse 1990, 1993b). In order to maintain their privileged, disembodied position from which they engage in political and social criticism of the natural sciences, the constructivists appeal implicitly to a view according to which a fixed and definitive historical account, based on a fixed and definitively knowable past, will uphold decisively the particular social theory that is serving as the sovereign epistemic construct. In contradistinction, for postmodernists, this disembodied perspective is at best a function, perhaps an artifact, of the narrative structure of many histories; that is, it is a *myth*.

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<sup>14</sup> One example of such a hostile reaction is Fuller’s (1996b) defense of Collins and Yearley (1992). Fuller aims his polemic at Bruno Latour (Callon and Latour 1992) and Steve Woolgar (1992), who both take seriously reflexivity, and who both criticize traditional social constructivism. Fuller refers to these scholars as “STS radicals” (p. 170) and to their efforts at avoiding the sovereignty of social realism as “the extremist methodologies of radical STS. . .” (p. 172). His view is that Latour and Woolgar add “nothing significant to understanding the social conditions of knowledge production” (pp. 170-1). From the sovereign perspective of privileged social science, to which Fuller clearly adhered (at least at this point), these statements are understandable, even if they are overly polemical and devoid of adequate defense. What Fuller apparently fails to realize, or at least fails to present in his defense of his allies, is that Latour and Woolgar (among others) reject the very ground on which Fuller stands. Rouse (1991b) provides an illuminating account of what is wrong with Fuller’s (1988) “social epistemology.” Rouse (2002a) also criticizes Fuller’s accounts of normativity (pp. 166-8, 181-2), and he provides an account of what is wrong with social constructivism and how it still remains with us as one of the “philosophical undead” (Rouse 2002b).

<sup>15</sup> Trevor Pinch, in an evaluation of the strengths of SSK over other fields, characterizes “the empirical case study” as follows: “I think it’s very dangerous once you get away from empirical work because the whole, this whole field is characterised by good solid empirical work. A lot of people have done it and that’s one of the things that made it a better field than most fields of sociology” (quoted in Ashmore 1989, p. 120).

### III. *Global Legitimation*

Practice scholars correctly indict extreme constructivists with upholding, ironically, science's *need* for global legitimation—that is, epistemic sovereignty (Rouse 1993a). In other words, having satisfied themselves that science has failed in its endeavor to provide objective knowledge and therefore *lacks* any sort of positivist or empiricist global legitimation, some constructivists feel justified in claiming that science *must* be all sociopolitics (e.g., Collins 1985). It is the *lack* of epistemic objectivity that seems to justify this move. The complaint of the constructivists that science lacks global legitimation thereby upholds implicitly science's *need* for global legitimation. That is, the constructivists' revelation that science lacks global legitimation implies that science *ought to* have such legitimation, and since it does not, one is free to move to the other extreme and suggest that science is “nothing but the social.” This lack of legitimation the constructivists delight in demonstrating repeatedly. It has put them in business, and has even prompted Collins (e.g., 1985) to proclaim that sociology is privileged precisely because its content *is* replicable, while natural science's is not (cf. Ashmore 1989, ch. 4). It seems that there is no room here for middle ground.

The constructivists' implicit view that science requires, yet lacks, global legitimation, is problematic. For one thing, it avoids the issue of why science ought to need such global legitimation in the first place. Thus, this viewpoint remains trapped within the confines of the metanarratives of modernity. Requiring science to possess such legitimation fails to problematize sufficiently a dichotomy, the maintenance of which is merely presupposed, not argued or deconstructed.

### IV. *Reflexivity*

Constructivists accuse practice scholars of engaging in futile debates that have not resolved any important issues, and that leave no room for political action. While in most cases practice scholars are not political or community activists, those in the emerging movement in Cultural Studies of Scientific Knowledge (CSSK) presuppose that the epistemic and the cultural are intertwined inextricably (e.g., Haraway 1991 [essays from 1978-89],



1981-82, 1997; Rouse 1987, 1991c, 1993b, 2002a; Latour [1991] 1993; Rheinberger 1995a, 1997). Furthermore, two prominent examples of bold moves away from extreme social constructivism toward cultural studies are Latour's ([1991] 1993, 1992; cf. Collins 1994) and Pickering's (1993, 1994, 1995a, b) conversions from traditional constructivism (e.g., Latour and Woolgar [1979] 1986, Pickering 1984) to a focus on practice. One must have a viable theoretical foundation—one that can engage and take into account the views of scientists, the public, and politicians—before one can expect members of those groups to pay attention (cf. Rouse 1991b, Seltzer 1995b).

As mentioned above, an important distinction between social constructivists and practice scholars is that, while constructivists take the import of their historical case studies on social theories about how science works to be unproblematic, practice scholars tend to pay attention to narrative contexts and their cognitive significance. That is, many constructivists fail to take seriously *reflexivity*, the notion that one's methodological prescriptions ought to be applicable to oneself, or to one's own practices. This failure is ironic, since reflexivity was a major prescription David Bloor ([1976] 1991) put forth in *Knowledge and Social Imagery*, taken by many constructivists to be the blueprint for the Sociology of Scientific Knowledge (SSK). According to Bloor ([1976] 1991), the new “strong programme in the sociology of knowledge” ought to contain the following philosophical principles:

- 1 It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
- 2 It would be impartial with respect to truth and falsity, rationality and irrationality, success or failure. Both sides of these dichotomies will require explanation.
- 3 It would be symmetrical in its style of explanation. The same types of causes would explain, say, true and false beliefs.
- 4 It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories. (p. 7)

Ironically, it seems Bloor's scientific tendencies, with which he aimed to make the sociology of knowledge no different from the natural sciences in scope and methodology,

compelled Bloor to include the reflexivity principle.<sup>16</sup>

The problem with Bloor's scientific view and with much scholarship in the Sociology of Scientific Knowledge (cf. Woolgar 1991, 1992) is that claiming objectivity and privileged status for sociological knowledge violates reflexivity and locates epistemic sovereignty in the social realm. Yet Bloor (and others) does not find this to be a problem:

I am more than happy to see sociology resting on the same foundations and assumptions as other sciences. This applies whatever their status and origin. Really sociology has no choice but to rest on these foundations, nor any more appropriate model to adopt. (Bloor [1976] 1991, pp. 160-1)

These scholars privilege sociology because their aim is to produce the definitive social explanation for episodes in the construction of scientific knowledge. Their "data" are "empirical" histories or ethnographies of science, and these accounts they take as unproblematic justification for adhering to some particular social theory. This privileging of social reality, however, calls into question their foundations: what is the argument for adhering to social realism, on the one hand, and rejecting scientific realism, on the other hand? Many constructivists apply the relativism and symmetry principles to natural scientific knowledge claims, but not to sociological claims. That is, they have abandoned reflexivity, in some cases because it would stifle their political agendas that seek to deflate the authority of science. However, by not problematizing sociological claims and the historical arguments used to buttress them, the constructivists ignore a whole spectrum of problems—problems that detract from the cogency of their accounts of science and technology.<sup>17</sup>

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<sup>16</sup> Bloor ([1976] 1991) makes no apologies for adhering to a scientific methodology. He seems not to realize the implications of his position for reflexivity, objectivity, and privileged knowledge. Bloor seems quite content, on the one hand, to hold the position that natural scientific knowledge is not objective, but is the product of social forces: "[T]o believe in a material world does not justify the conclusion that there is any final or privileged state of adaptation to it which constitutes absolute knowledge or truth" (p. 160). So, Bloor is quite willing to invoke relativism in this way. On the other hand, Bloor is not willing to be reflexive and apply the same relativistic move to the science of sociology, for he believes that his sociological knowledge can attain at least a degree of justified objectivity: "[Objectivity] is real but its nature is totally different from what may have been expected. It is other theories of objectivity which are denied by a sociological account, not the phenomenon itself. Those who elect to be champions of scientific objectivity might reflect on the following: a sociological theory probably accords objectivity a more prominent role in human life than they do" (p. 160).

<sup>17</sup> Arthur Fine (1996) put it this way: "To put it bluntly, constructivists write a good deal of nonsense on these topics . . ." and they "tend to rely more on polemics than on careful argument" (p. 232).

## V. *Narrativity*

Clearly, the very notion of narrative context must be explicated adequately if we are to avoid the social realism of the constructivists and the historical realism of positivist historiography (and other problems, as well). Constructivists take narratives, or stories told retrospectively about the past, to be the fundamental “empirical” evidence supporting their assessments of the nature of science and technology. But what they fail to realize is that their “stories”—their texts, their social theories of science, their professional work—are stories within which they themselves are embedded as the narrators (cf. Rouse 1990). That is, *they* exist, as do scientists, within changing narrative contexts.

In traditional positivist historiography, a *narrative* is defined as a story, an ostensibly true account of selected events of the fixed past. In the extremes of positivist historiography, we can equate a narrative with an historical *explanation*, where the explanation is modeled on the traditional positivist views of scientific explanation and prediction, with the goal of making history into a science:

The explanation of an occurrence of an event of some specific kind *E* at a certain place and time consists . . . in indicating the causes or determining factors of *E*. Now the assertion that a set of events—say, of the kinds  $C_1, C_2, \dots, C_n$ —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 kind *E*. (Hempel [1942] 1965b, p. 232)

In this passage, Hempel advocates the view that historical events are linear, that prior events cause later events, and that the historian’s goal should be to determine the general, universal, or probabilistic historical laws that will explain the event in question (cf. Pitt 1959).

Furthermore, Hempel ([1942] 1965b) is quite clear that the proper goal of the historian is to discover these universal (or probabilistic) historical laws in order that later events can be *predicted* from earlier ones:

Historical explanation . . . aims at showing that the event in question was not “a matter of chance,” but was to be expected in view of certain antecedent or simultaneous conditions. The expectation referred to is not prophecy or divination, but rational scientific anticipation which rests on the assumption of general laws. (p. 235)

Clearly, this view of narrativity and of the goal of the historian we must reject (cf. Danto 1956). If historical events are not matters of chance, then we must do away with the contingency of history—the view that any given historical event “could have been otherwise.” To reject the contingency of events invites historical determinism—the view that future events are determined by earlier ones. However, even where scientific experimentation is concerned, we find that significant discoveries defy anticipation or prediction of the sort Hempel (1965b) dissected (e.g., Rheinberger 1992a, 1992b, 1994). Moreover, even if certain experimental discoveries could be predicted, with what historical laws can we predict the “effects” that discovery will have on future events? Take, for example, Otto Hahn and Friedrich Strassmann’s discovery of nuclear fission in December of 1938 (Rhodes 1986). After Hahn and Strassmann published their paper on fission in January of 1939, many scientists realized the possibility of a chain reaction and an atomic bomb. A few months later, Leo Szilard, Enrico Fermi, and others confirmed at Columbia University that each nuclear fission of Uranium-235 releases enough neutrons to sustain a chain reaction, and hence that an atomic bomb of immense energy was more than just a theoretical possibility. Szilard, Fermi, and Eugene Wigner immediately planned how they would convince the American government to fund an experiment to verify a chain reaction was possible. Thus were born the efforts toward the building of the first atomic bomb, and toward the subsequent dropping of atomic bombs on Hiroshima and Nagasaki in August of 1945. Now, do we conclude, following Hempel, that Hahn and Strassmann’s discovery “caused” the dawn of the atomic age, and that all these events could have been predicted by astute historian/scientists? What are the scientific and historical laws to which such a prediction would appeal? At what point in time would it have been possible to predict these events?

Attempting to answer these questions, that is, taking seriously positivist historiography, fails to consider major conceptual problems in conceiving of narratives as fixed, definitive accounts of the past. For example, this view of narratives does not problematize the connection between the fixed, static account of the past, on the one hand, and the evidence on which it is based, on the other hand. Short of time travel, all attempts to

write a history are *reconstructions* based on necessarily limited evidence. Moreover, this historical evidence is subject to the same epistemological problems as is scientific evidence, which Hempel (1965b) himself helped clarify. Historians disagree on the connections between historical evidence and historical explanations or interpretations, and there is no neutral historiographical method that can decide objectively (definitively) among the historical accounts (see Ch. II below). And if historiographical grounds are deconstructed in this manner, then the methodology of using fixed, static accounts as presumably determinate causes for subsequent events is undermined, for the descriptions of events, and hence these events themselves, are ineluctably indeterminate. What we need is an alternative notion of narrativity.

The view of narrativity and its significance for experiment that I use in this dissertation I derive mainly from Rheinberger (1994, 1997) and Rouse (1990; 1996b, ch. 6). According to Rouse (1996b), we should not view the “epistemic significance of narrative in terms of completed narratives, with their established beginning, middle, and end and with their unitary point of view” (pp. 160-1). Instead, we should view scientific research as “situated within narratives in construction (or perhaps better, in continual reconstruction)” (p. 161). Hence, Rouse believes that narrative “should not be thought of as a scheme imposed on an unnarrativized sequence of happenings” (p. 160). Rather, the very “intelligibility of action and of the things we encounter or use in acting depends on their already belonging to a field of possible narratives. On [this] view, we live within various ongoing stories as a condition for our being able to tell them or for doing anything else that can count as acting or participating in practice” (p. 160). For Rouse, there is a deep fundamentality to narrative that makes meaningful action possible. That is, Rouse suggests that any agent’s taking action already presupposes the existence of a narrative or a narrative field (a set of interrelated narratives) to which the agent already belongs.

Rouse’s (1990; 1996b, ch. 6) view of narrative, derived in part from Heidegger, immediately raises problems for the view of historiography I develop here. I consider these problems in more detail in Chapters III and V below. However, it is instructive to raise one crucial issue that must be addressed: this is the representational (or constative)/ performative

dichotomy (e.g., Rouse 1996b, pp. 153-5, ch. 8). In his efforts to extricate himself from the dichotomies of modernity, Rouse (e.g., 1991a) labors laudably to steer analyses of science and technology away from the extremes of each pole of any given dichotomy. In the case of representation and narrative, however, Rouse's position seems, at times, to be anti-representational. All attempts at taking action, including the use of language, are for Rouse *not* to be understood representationally, but performatively. Instead, Rouse (1996b, ch. 8) takes the view, adopted from Davidsonian semantics, that "deni[es] that language works representationally" (p. 210). Yes, we cannot get "outside" of language, but there is also no "prelinguistic 'component' of our interaction with the world" (p. 210, fn. 13). According to Rouse, this interpretation of Davidsonian semantics

presumes that one already speaks a language, and it takes interpretation within an unanalyzed home language to be the most fundamental level of linguistic understanding. One's own language does not thereby become a privileged frame of reference, for it no more supplies a fixed "frame" than does any other way of speaking. Expressions in one's own language can be interpreted in turn, but only by construing them within a language that must remain unexamined for that occasion. (p. 210)

So, for Rouse, neither can we extricate ourselves (our actions, practices) from language use, nor can we presuppose any kind of fundamentally prelinguistic connection between ourselves and the world. Nevertheless, in terms of understanding the *practices* of scientists or historians, this view of narrative context raises some problems.

One problem raised by Rouse's view of narrative context is how to square his anti-representationalist stance with the scientist's and the historian's understanding of the goal of his/her practices: that is, to *represent* the "world" and the "past," respectively. Spiegel (1992), as noted in the section on postmodernism above, views the past as "mediated" by the indeterminate evidence available to the historian, yet she clearly does not believe the past is "directly present."<sup>18</sup> Rouse (1996b), however, claims that "[n]othing mediates our

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<sup>18</sup> In his "denial of mediation" (p. 209), Rouse (1996b), following Donald Davidson, does not claim "that the world or its objects are directly and immediately present; rather, nothing is directly present, not even experience, thought, or meaning. Understanding any one thing (an object, an utterance, some other perceived event, or a pattern of objects and events) requires understanding many others. A noise cannot be an utterance or a movement an action unless they belong to a larger pattern of utterances and actions within which they are intelligible" (pp. 209-210). This larger pattern, for Rouse, is the narrative context or field of narrative contexts.

interaction with the world, not experience, thought, language, meaning, or representation. Talk and perception are just further interaction, not a medium through which interaction is filtered” (p. 209).

How we should ultimately square these apparently divergent views I consider in Chapter III below. For now, I merely note that we can maintain the view that “nothing is directly present, not even experience, thought, . . . meaning” (Rouse 1996b, p. 210) or “the past,” yet also realize that attempts at taking meaningful action are also predicated upon a realization of meaningful events having taken place *in the past* (cf. Heidegger’s 1962 notion of *Geschichtlichkeit*). That is, we must *interpret* (not necessarily consciously or deliberately) the past to take action in the present, to create the future. This is not a static and fixed past, but it is known “only through a continuing partial reconstruction of a shared sense of what the [relevant] community has been about and where it can and should proceed” (Rouse 1996b, p. 165). Any such reconstruction involves an *interpretation* of some spatiotemporal slice of the past, and that interpretation is a fundamentally *retrospective* practice—a practice in which an agent attempts to *represent* that spatiotemporal slice. Moreover, that representation—that attempt to take meaningful action, that intervention in the world—is itself mediated by spatiotemporal distance.<sup>19</sup> Furthermore, this spatiotemporal distance is characterized by a fundamental indeterminacy: that is, we cannot definitively capture the (actual, real) past, precisely because our knowledge of that past is mediated by the narrative field to which we belong.<sup>20</sup> In this sense, the past is created in the act (conscious or not) of

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But even if we want to maintain all this, must we not also maintain that this “larger pattern of utterances and action,” the narrative context, must itself be (re)constructed by particular agents? And if this is so, such a reconstruction is an attempt to represent, to describe, past utterances and events. These utterances and actions, in turn, are not “immediately present,” yet surely they are not purely fictional. The past must be reconstructed based on limited, contestable evidence—evidence that mediates our understanding, our “experience” of the past.

<sup>19</sup> Rheinberger (1992b) notes that “Hacking [1983] distinguishes between “representing” and “intervening” as basic modes of scientific activity.” For Rheinberger, however, “representing *is* intervening” (p. 394, fn. 15).

<sup>20</sup> For Rheinberger (1992b), this indeterminacy, however, is not unconstrained: “A scientific object investigated by an experimental system is deployed and articulated within a space of material representation. How is it shaped? The structure of the scientific object contained and contended about in the experimental setting constitutes a model” (p. 390). Yet Rheinberger does “not regard models as primarily ‘theoretical’ representations. The model is *implemented* in the structure of the experimental arrangement itself. It is materialized as the scientific object under investigation. This is what renders it resistant against the forms of logical coherence one would like to bestow or impose on it” (p. 390). Hence, Rheinberger’s notion of experimental system, if we

recreating (attempting to represent) it—but the historian’s (or other actor’s) practice of attempting to represent the past, in an attempt to take meaningful action, is also retained.

Now, I must be careful to distinguish my practice of interpreting Rouse’s efforts at interpreting interpretation, on the one hand, and what we ought to say, for example, about historians’ or scientists’ attempts to interpret, or represent, on the other hand. Here is where Hans-Jörg Rheinberger (1992b, 1994, 1995b) is helpful. Rheinberger (e.g., 1992b, pp. 390-5) maintains, as I believe Rouse does, that representing and taking action are not fundamentally distinct. For the experimental scientist, the activity of representation is ubiquitous. But, as Rheinberger (1992b) states, “the comparison is definitely not between ‘nature’ and its ‘model,’ but rather between different graphematic traces that can be produced. Their matching gives us the ‘sense’ of ‘reality’ we *ascribe* to the scientific objects under study. The ‘scientific real’ is a world of traces” (p. 394). So, on the one hand, Rheinberger is faithful to scientists’ views of their own experimental practice, and he incorporates willingly into his accounts of science these valuable perspectives. On the other hand, Rheinberger is not blind to his own activity as an historian (and scientist), nor to the retrospective views of research scientists.

To capture this view that considers, yet deconstructs, both the scientist’s and historian’s perspectives, Rheinberger (1994) incorporates Derrida’s ([1967] 1976) notion of *historiality* into his framework for analyzing experimental science. According to Rheinberger (1994),

what might be called *historial* thinking not only has to accept and even postulate a kind of recurrence inherent in any hindsight—hence interpretation, or hermeneutic action. It has to assume that recurrence works in the differential activity of the [experimental] *system* that is *itself* at stake, and in its time structure. What is called its history is “deferred” in a rather constitutive sense: The recent, so to speak, is the result of something that did not happen. And the past is the trace of something that will not have occurred. Such is the temporal structure of the production of a trace. (pp. 66-7)

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consider it to be an experimental narrative, is resistant to Rouse’s (1996b) objection to Hayden White’s view of narrative, which for Rouse suggests that narratives are “indefinitely open to different narrative configurations or emplotments” (p. 165). For Rheinberger, the experimental system can be seen as having material agency. I believe, in addition, that Rouse is misreading White; I believe White would agree with Rheinberger and others who believe our actions are constrained by our being in the world. Ankersmit (1998) defends White’s historiography against what he takes to be common misreadings of White.



The present “is the result of something that did not happen,” because all attempts to reconfigure narratives (to take action) are fundamentally retrospective. Moreover, to reconfigure the present narrative (or experimental) context “requires the product in order to assess the conditions of its emergence” (p. 66). That is, our current desire to assess a past event, say an experimental “discovery,” requires that we *recur*: we must turn our thoughts and discourse backward temporally and consider again.

However, all recurrence is contaminated, for any attempt to go back to a time before the event or trace was *not yet* a novelty (e.g., a discovery worthy of being reconstructed), is necessarily from a context after which the trace became novel. But that earlier time was a time when the trace was “the trace of something that will not have occurred,” that is, a time, a narrative context only accessible to us through the medium of a spatiotemporal/cultural/epistemic discontinuity. Hence, any attempt to capture the past contaminates or distorts it. Similarly, for Rheinberger (1994), the production of at least some experimental scientific knowledge follows this *historial* character. In the ongoing research process of experimentation within an experimental system, the system *itself* displays this notion of recurrence. The representations of experimental scientists—“material, graphic entities”—always “contain the possibility of an *excess*” (p. 71). That is, these representations

contain more and other possibilities than those to which they are actually held to be bound. The excess embodies the historial movement of a trace: It is something that transgresses the boundaries within which the [experimental] game appears to be confined. As an excess, it escapes any definition. On the other hand it brings the boundary into existence by cutting a breach into it. It defines what it escapes. The movement of the trace is recurrent. The present is the future of a past that never happened. (p. 71)

The significance of this reflexive exercise is that viewing a narrative as an ongoing cultural/epistemic phenomenon within which a narrator (e.g., author, actor) is situated, results in a dynamic view of history and of the interrelations of narrative context and the content of writings *about* this history. The result is a non-sovereign, partial perspective, coming from *within* the cultural context of science, one that gives credence to the radical contingency of history. In contrast, the result of viewing narratives as static retrospectives about the unproblematic past is that one adopts the traditional anthropological position of privileged

outsider, making pronouncements about some delimited subject matter. This static view of history and historiography presupposes access to historical “reality” and the possibility that a definitive account of the past can be had (Seltzer 1995a). The result is epistemic sovereignty and the removal of oneself (when studying science and technology) from a narrative context, that is, from culture, from history. It is an objectivist move, one that at least implicitly dehistoricizes the story it tells, for it removes its narrator (the author) from the constantly changing, dynamic narrative context. The problems that result from this, I show in this dissertation, limit severely the scope and nature of accounts of science and technology.

The benefits, then, of focusing on practice over advocating extreme constructivism or global legitimation are the following:<sup>21</sup> (a) historical events are contingent events, open to later reconstruction and reinterpretation, and not determined by a reified social realm, as Collins and others require; (b) the supposed rationalist epistemic import that results from the coherence of the discourse of traditional narrative history is exposed for what it is: mythical, and *not* an unfalsifiable neutral medium for conveying *a priori* meanings (cf. White 1987, Mali 1994); (c) the complexity of history is revealed: no longer static, irrefutable, definitive narratives, historical narratives become complex combinations of reconstructed material and social cultures; and (d) scientific experiments—which are, after all, *contingent historical events*—are ready to be historialized.<sup>22</sup>

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<sup>21</sup> As Foucault ([1969] 1977) asked during a lecture in 1969: “Is it not necessary to draw a line between those who believe that we can continue to situate our present discontinuities within the historical and transcendental tradition of the nineteenth century and those who are making a great effort to liberate themselves, once and for all, from this conceptual framework?” (p. 120). In other words, we might read Foucault as suggesting that there are sharp methodological and political differences between those who continue to advocate some form of epistemic sovereignty—whether in science or historical practice—and those who wish to reject it in all forms. Such a rejection also requires that we abandon the view that the “present” is the unproblematic “result” of the events of the “past” (cf. Rheinberger 1994). Certain events, for example, World War II, the Holocaust, and the atomic bombings of Hiroshima and Nagasaki, resist “explanation” solely on the basis of previous events, and should be seen as “discontinuities” (cf. Pickering 1993, 1995b). For Rheinberger (1994), certain scientific experimental systems should also be seen as discontinuities, with their own internal time structures. See Ch. IV below for more on this.

<sup>22</sup> I deliberately resist using the term “historicized” here to signify the break with the traditional view of history and historiography, according to which subsequent historical events are to be “explained” (often unproblematically) on the basis of prior events (see, for example, D’Amico 1989). The term “historialized” I borrow from Rheinberger (1994), who adapts it from Derrida ([1967] 1976). According to this view, we must not see the present as the unproblematic result of the past. Instead, argues Rheinberger (1994), we must “accept and even postulate a kind of recurrence inherent in any hindsight—hence interpretation, or hermeneutic action” (p. 66). The “recurrence,” or turning of one’s thoughts back in time, refers to the reconstruction of past events at some later

## **The New Experimentalism**

### *I. Telling Stories of Separating Signal from Noise*

A critical problem for scientists conducting experiments is distinguishing experimental “signal” or “entity” from background “noise” or experimental “artifact.” When asked about her task as a researcher, the experimental scientist will likely claim it to be something akin to one of the following: (a) to discover (facts about) “real” phenomena: for example, superconductivity, the effect of ionizing radiation on skin cancer induction, the cardiovascular health benefits of taking aspirin daily; or (b) to discover “entities” in the world, in “Nature:” for example, transfer-RNA, the gene(s) that cause Alzheimer’s Disease, the top quark, or the Higgs boson. Furthermore, the experimental scientist strives to distinguish such “real” effects and entities from artifacts of the experimental arrangement, for example, a hair on a microscope slide, a biochemical impurity, a spurious correlation resulting from a background variable not controlled for, or a set of mathematical equations that has no empirical connection to the real world. In short, one of the major goals of scientists, perhaps the main goal, is to separate signal from noise (cf. Galison 1987, 1997).

To illustrate this aspect of the practice of scientists, I present two brief examples. In the first example, involving the “discovery” of the 3K microwave background radiation, the scientists involved believed they had isolated experimental noise in their experimental apparatus. Nevertheless, they eventually won the Nobel Prize for having “discovered” the radiation remaining from the “big bang” at the creation of the universe. In the second example involving the “discovery” of a hormone that triggers human growth hormone, the scientists involved believed they had isolated a real entity—an actual hormone. However, other scientists later showed that this supposed hormone was actually an artifact of the isolation process—it was a biochemical contaminant. The goal of this dissertation is to show

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time. The result of recurrence is this: “The present is the future of a past that never happened” (p. 71). The “past that never happened” is the scientist’s linearization of research work, according to which “the present appears as the straightforward result of the past pregnant with what is going to be” (p. 78). However, in scientific research, one cannot predict with certainty what the result of an experiment will be, and experimental systems, if they are significant and survive long enough to be reconstructed historically, often have gone through numerous reconfigurations. Yet, in scientific papers, textbooks, and in scientist’s histories, the process of discovery is often presented in a linear fashion, with each step leading logically and inevitably to the present (cf. Medawar [1963] 1990).

how and why the following prescription is compelling: in order to interpret events such as those mentioned above, and other attempts to separate signal from noise, one must identify and explicate a technological infrastructure. Indeed, in order to consider adequately the several issues I laid out in the previous section, one ought to utilize the technological infrastructure construct to interpret/(re)construct the experimental/epistemic/cultural context in which the “discovery” or failure at discovery occurred. The technological infrastructure is needed to help frame and appropriately situate a complex set of questions that invariably arises when actual scientific activity, or stories about actual scientific activity, are probed philosophically, as the following examples illustrate.

In 1963, at Bell Laboratories in Holmdel, New Jersey, the radioastronomers Arno A. Penzias and Robert W. Wilson were trying to eliminate the microwave background noise they believed they were experiencing when attempting to use their radiotelescope to detect radiation from space at the Crawford Hill Laboratory (Bernstein 1984, chs. 14, 15; Kragh 1996, pp. 346-55). That is, they were trying to separate signal (real radio waves from deep space) from noise (interference with their equipment, apparently caused by pigeon droppings). Their initial assumption was that the homogenous, isotropic, and season-independent noise they were receiving at about 3K had to be an artifact of their instrumentation or experimental set-up. From their practical experimental perspectives as astronomers, it just did not make sense for there to be a uniform background radiation in space, emanating from all directions, and not originating in the Milky Way Galaxy (Bernstein 1984, pp. 200-3).

As it turned out, Penzias and Wilson are credited with discovering, in the period 1963-65, the 3K background radiation left over from the origin of the universe, or the “Big Bang.” Their discovery is generally held to be the decisive evidence for the expanding universe of the Big Bang Theory, developed by scientists such as George Gamow, and against the Steady State Theory, developed by Thomas Gold, Fred Hoyle, and others. Penzias and Wilson consulted theoreticians in astrophysics at Princeton University, led by Robert Dicke; they discovered Dicke’s group had predicted the 3K radiation based on the Big Bang Theory and were planning themselves to build an instrument to detect it. Penzias and

Wilson beat them to it, accidentally, and got a theoretical interpretation for their results from the Dicke group, who shared their results (Bernstein 1984, pp. 203-5; Kragh 1996, ch. 7, esp. pp. 347-55). The “discovery” made front-page headlines in *The New York Times* on 21 May 1965, and the groups simultaneously published their results, which appeared in July of 1965.<sup>23</sup>

How to interpret historically the story of the “discovery” of the microwave background radiation is still contested by scientists and historians alike, even if the scientific consensus is that it signaled the confirmation of the Big Bang Theory and the death of the Steady State Theory (see, for example, Kragh 1996, chs. 7, 8).<sup>24</sup> What started as an effort to eliminate background interference, possibly caused by pigeon droppings on their apparatus, eventually led to a Nobel Prize in Physics for Penzias and Wilson in 1978 (and not for the Dicke group) for the discovery of an ostensibly real phenomenon of the universe. I recall

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<sup>23</sup> Walter Sullivan, “Signals Imply a ‘Big Bang’ Universe,” *The New York Times*, 21 May 1965, p. A1; A. A. Penzias and R. W. Wilson (1965), “A Measurement of Excess Antenna Temperature at 4080 Mc/s,” *The Astrophysical Journal* 142: 419-421; R. H. Dicke, P. J. E. Peebles, P. G. Roll, and D. T. Wilkinson (1965), “Cosmic Black-Body Radiation,” *The Astrophysical Journal* 142: 414-419.

<sup>24</sup> Historiographically, Kragh (1996) downplays the significance of Penzias and Wilson’s efforts, and implies that the Dicke group at Princeton should be given more credit (ch. 7, esp. pp. 347-57, 373-88, and epilogue, esp. p. 391). Moreover, Kragh (1996) argues that the “discovery”—whether it is held to have taken place in 1963 when Penzias and Wilson first detected the background “noise”; or when in 1964 Penzias telephoned Dicke, followed by the Dicke group’s visit to Bell Labs and their sharing of their data with Penzias and Wilson; or when in 1965 it was announced to the public in the press and in scientific journals—was only tentative in 1965, and required several years to be fully confirmed, especially since it was not initially clear that the black-body spectrum of the radiation would hold for other wavelengths not measured by Penzias and Wilson (ch. 7). Indeed, the Dicke group acknowledged this in their 1965 paper (Dicke, *et al.*, 1965, p. 416). However, Kragh’s (1996) book on the “controversy” between the “competing” theories of twentieth century cosmology—the Steady State Theory and the Big Bang Theory—focuses almost exclusively on scientific theories, and not on experiments or other activities of science as a practice. Therefore, it is not surprising that he comes to the conclusion that “epistemic reasons” resolved the controversy, and not nonepistemic factors (p. 394), even though he did not investigate the latter in a critical or comprehensive way. As he concludes: “Nonepistemic factors played a role all along, but the controversy did not terminate because one side lost research opportunities, was treated unfairly by referees, or was found politically unacceptable by those in power” (*ibid.*). Clearly, when science is reconstructed in terms of its (dominant) theories to illustrate how they are chosen rationally by a privileged epistemic method, implying an increase in truth-content over time, it should be no surprise that experimentalists’ efforts are marginalized or that “controversies” are held to be adjudicated solely by epistemic criteria. After all, this was Kragh’s intention from the beginning: “The present work has little to say about observational techniques and astronomical instruments, although it can be argued that it was in fact advances in technology that led to the termination of the cosmological controversy. Like all historical works, mine is selective, and I have given high priority to theory rather than experiment. My excuse is that most of the debate took place within a theoretical or conceptual context. The importance of observations was always admitted, but the role played by the technical details of instruments was subordinate to that of theory” (p. xii). This dissertation is committed to showing that this orientation ought to be changed.

initially hearing this story told in the way Hacking (1983, pp. 159-61) reports having found it in a textbook on electromagnetic theory: Penzias and Wilson, two radioastronomers at Bell Labs, set out to detect the 3K background radiation from the Big Bang in order to *test* whether the Big Bang Theory was true. This version of the story is an example of what Rheinberger (1994) means by the linearization of research results: our traditional view is that science marches on, via the testing of hypotheses and confirmation of theories, toward a more unified set of theories that have increasing truth-content over time; science is the privileged, rational pursuit of truth. As it happened, Penzias and Wilson were initially just trying to get their equipment to work; they consulted “theoreticians” only after much effort to eliminate all known background, and only after this effort resulted in consistently anomalous results. Their “discovery” was accidental, or at the very least, serendipitous, as they both seem to acknowledge (Bernstein 1984, pp. 200, 226). This view of the story suggests that looking at the microdynamics of actual scientific practice might lead to a view of science that departs significantly from the traditional view. That is, focusing carefully on *practice* as an historiographically important unit of analysis—without privileging it in the sense of rejecting out of hand the importance of theories, hypotheses, or other units of analysis—ought not only to result in a different view of science from that of traditional philosophy of science, for example, it ought to lead to accounts of the history of science that are more faithful to the past *wie es eigentlich gewesen ist*, or as it really happened.

Consider now a second story of separating signal from noise, or in this case, entity from artifact. Latour (1987) tells a partly fictionalized story of two competing endocrinologists, Roger Guillemin and Andrew V. Schally, who both made important discoveries involving hormones and their roles in regulating physiological processes. They shared the Nobel Prize in Physiology or Medicine in 1977 with Rosalyn Yalow, who helped develop the radioimmunoassay (Wade 1981).<sup>25</sup> In Latour’s (1987) story, Schally is presented

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<sup>25</sup> Latour (1987) uses the Schally-Guillemin rivalry to illustrate his points regarding scientific literature and laboratories in chapters 1 and 2 of his book, *Science in Action* (pp. 23-62, 80-1, 86-8, 95-6); he fictionalizes some of the incidents to make his points. Latour takes the historical facts of the episode from Nicholas Wade’s (1981) book, *The Nobel Duel: Two Scientists’ 21-year Race to Win the World’s Most Coveted Research Prize*, Garden City, NY: Anchor Press. Unlike Kragh’s (1996) view of Penzias and Wilson, Wade’s (1981) book presents a view in which competition, personal and professional animosity, funding resources, and

as the head of a research group that claims to have isolated a new hormone, Growth Hormone Releasing Hormone (GHRH), a hormone that ostensibly regulates Human Growth Hormone, thus making it a possible key to treating dwarfism (p. 23). In his analysis of what is necessary to counter a scientific claim, and hence to subvert a network, Latour (1987) tells the story of a rival research group, led by Guillemin, who later show that the Schally group's supposed isolation of GHRH—their attempt to separate signal from noise—was actually nothing more than the detection of a common byproduct of the techniques used in isolating hormones from brain tissue: hemoglobin.<sup>26</sup> In this story, what was reported in the scientific literature as a successful attempt, although with tentative qualifications, at separating entity from artifact, turned out to be nothing more than the detection of an artifact of the processes used to isolate hormones from brain tissue—in this case, a chemical byproduct. Again, the question remains, how should we tell this story? Do we agree with Latour (1987) and conclude that scientific facts are nothing but that which resist “trials of strength” (pp. 74-9)? Do we agree that scientific facts are power networks constructed of social and material components, yet it is clearly not “Nature” that is the cause of the network or the knowledge claim (e.g., p. 89)?

In a previous book, Latour and Woolgar ([1979] 1986) had presented their defense of the social construction of scientific facts. Prior to this, Latour had spent about two years (1975-77) as an ethnographer in Roger Guillemin's lab at the Jonas Salk Institute for Biological Studies in La Jolla, California, funded by a Fulbright grant and then a NATO

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the drive for credit and reputation, in addition to other factors, play a significant role in scientific activity, including what research programs to pursue. He presents the rivalry between Schally and Guillemin, and their respective research collaborators, as an intense, nasty, and bitter competition, one in which the perceived history—at least the recent history—of their field played an important role. Wade (1981) argues that “Guillemin and Schally frequently distorted the historical record so as to exaggerate their personal achievements and minimize those of their colleagues and rivals. How did they get away with it? What is it about the structure of the scientific community that not only condoned but made profitable the campaigns of relentless promotions?” (p. 277). The significance of scientists' views of the history of their field is explored in this dissertation, esp. in Chs. II, III, and IV.

<sup>26</sup> The articles Latour (1987) analyzes in a fair amount of detail in his story are: Andrew V. Schally, Yoshihiko Baba, R. M. G. Nair, and Carl D. Bennett (1971), “The Amino-acid Sequence of a Peptide with Growth Hormone Releasing Activity Isolated from Porcine Hypothalamus,” *Journal of Biological Chemistry* 216: 6647-50; and Roger Guillemin, Paul Brazeau, Peter Böhlen, Frederick Esch, Nicholas Ling, and William B. Wehrenberg (1982), “Growth-hormone Releasing Factor from a Human Pancreatic Tumor that Caused Acromegaly,” *Science* 218: 585-7.

fellowship. Latour and Woolgar's initial defense of social constructivism in the 1979 first edition of *Laboratory Life*, with its unreflexive and unsymmetrical reliance on using the social to explain the natural, was tempered by the time of the 1986 second edition, in which the term "social" was dropped from the subtitle. As Latour (1987) explains,

our method would gain nothing in explaining 'natural' sciences by invoking the 'social' sciences. There is not the slightest difference between the two, and they are both to be studied in the same way. Neither of them should be believed more nor endowed with the mysterious power of jumping out of the networks it builds. (p. 256)

What is significant about Latour's use of his data on Guillemin's laboratory is that by the time of *Science in Action*, Latour is no longer a social constructivist in the sense of "there is nothing but the social"—the social and the natural are both to be explained in the same way. Nevertheless, one could argue that Latour's (1987) strategy becomes even more radical, at least from the perspective of the traditional view of science. That is, in rejecting social realism for actor-network theory, Latour's (1987) view of the role of the natural world in constraining what comes to be scientific knowledge becomes even more problematic in the sense that Latour is all but silent on the matter:

Behind the texts, behind the instruments, inside the laboratory, we do not have Nature. . . . What we have is an array allowing new extreme constraints to be imposed on 'something.' This 'something' is progressively shaped by its re-actions to these conditions. (p. 89)

Yes, he does seem to be granting these nonhuman actors agency. He indicates that "the 'things' behind scientific texts . . . are defined by their performances" (p. 89), yet it is not clear that Latour (1987) is willing to grant these (usually unobservable) scientific entities (or provisional entities; they *become* entities if they continue to resist trials of strength) the same kind of agency he has granted to humans. Facts—hypotheses and/or theories that purport to say something, or represent, reality or Nature—are clearly *made to be facts* by people, on Latour's (1987) view. But Latour is not entirely clear on how the entities or phenomena scientists study serve as actors in helping to constrain the knowledge we eventually come to accept about them. In short, do these entities and phenomena have agency, and if so, how do we describe that agency?

Latour (1992) later suggests, however, in his paper on how ordinary artifacts



(technologies in the traditional sense) can be “social” (p. 227), that when considering agency, we should not only accept symmetry between humans and nonhumans, we should also accept their *interchangeability*—that is, human agency and nonhuman agency are in some sense the same, and they can be “substituted” or “delegated” for each other, in practice and in accounts of sociotechnical change (see, e.g., pp. 229, 231). On the one hand, it seems as if Latour is quite willing to give doors and seatbelts agency, but hedges on whether the scientific entities and phenomena of the microworld should be given the same status. On the other hand, in his attack on the “sociologism” (p. 239) of social constructivists (e.g., Harry Collins; see p. 255) who try to explain the natural on the basis of a reified social realm, Latour (1992) concludes that those studying technology should not bifurcate people and things, for one cannot be understood without the other:

Students of technology are never faced with people on the one hand and things on the other, they are faced with programs of action, sections of which are endowed to *parts* of humans, while other sections are entrusted to parts of nonhumans. . . . What appears in the place of the two ghosts—society and technology—is not simply a hybrid object, . . . but a *sui generis* object. . . . It is too full of humans to look like the technology of old, but it is too full of nonhumans to look like the social theory of the past. (p. 254)

While Latour’s (1992) description of “substitution” and “delegation” may work for macroscopic technologies—doors, seat belts, and keys—it is not clear that this strategy will work for many of the “technologies” of science. How a liquid helium-cooled radiotelescope, a complex statistical technique, the peer-review process (which itself is already partly human), or an electron microscope, can be taken either to substitute for a human or be something to which one delegates human work or agency, in the sense that Latour employs ‘agency,’ is not clear. However, the prescription that humans and things ought to be considered in a context in which one does not make sense without the other (echoing Heidegger and Foucault), is a useful one; this is analyzed in more detail when the work of Joseph Rouse is explored in Chs. III-V below.

Some important questions remain: does it matter how these stories are told? If so, why? I argue that it *does* matter how these stories, and similar stories, are told, and that the technological infrastructure of science, as I develop it in this dissertation (see Chs. IV and V

below), can help answer why it matters. It matters at least for the following reasons:

(1) A major goal for the telling of historical stories ought to be getting it right, that is, telling the “truth” about what happened, with ‘truth’ appropriately defined. If historians and others do not have an accurate representation (appropriately defined) of the past as a methodological goal, then history is in danger of becoming a mere tool of ideology or propaganda. Moreover, even if all historical interpretation (History-3) is unavoidably ideologically contaminated, it does not follow that historians should abandon the goal of representing the past *wie es eigentlich gewesen ist* (for more on this, see Ch. II below).

(2) The stories we tell about science, the stories scientists tell each other, and the stories scientists and others believe about science, all *matter*. They matter in the sense that the *actions* scientists (or others, including historians and philosophers) take are in part linked to these stories; people tell stories for a reason. Hence, studying *how* those stories are told, *why* they are told various ways, and *who* is telling them, have significance for analyzing historical truth, present action, and for what kind of future one hopes to create.

## II. *A Critique of the New Experimentalism*

In the 1980s, philosophers, historians, and sociologists of science and technology began to take a fresh interest in the signal/noise problem with an eye toward studying more closely what actually goes on in scientific experiments (cf. Ihde 1991, ch. 6). The “New Experimentalism,” as Robert Ackermann (1989) has dubbed it, is one example of the current trend to rehabilitate historically the experimentalist and to re-evaluate philosophically the role of experimental practice in the epistemology of science.<sup>27</sup>

In response to the perceived failures of positivism, post-positivism, Whig history of science, and scientific realism, the New Experimentalists offered a promising new perspective on how to conceptualize science and scientific change. However, I argue that

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<sup>27</sup> The New Experimentalists include Ackermann (1985), Cartwright (1983, 1989), Franklin (1986, 1990), Galison (1987), Giere (1988), and Hacking (1983). In Chapter V below, I analyze Hacking’s (1983) and Galison’s (1987) books. Hacking (1983) is a pragmatic realist concerning some of the entities of science, believes these entities have agency, and he grants the contingency of history (cf. Hacking 1992). Galison (1987) gestures toward the contingency of history, but is silent on the agency of the natural world in constraining scientific knowledge. Both reject positivist *and* constructivist views of science.

with the possible exceptions of Hacking (1983) and Galison (1987), the New Experimentalists—for all their gestures toward analyzing actual experimentation and hence, the practice of science—succeeded merely in relocating epistemic sovereignty in experimental practice. Furthermore, most of these attempts to reconceptualize the notion of a global epistemic structure of science, whether explicit or implicit, fail to take seriously the issues of historiography and narrativity. And further, they manage, in most cases, to smuggle in some form of realism—usually some form of convergent epistemological realism—based on the argument that the only way to explain the success of science is to invoke a mechanism by which it moves, over time, closer to the truth (cf. Laudan 1981).

Take, for example, Allan Franklin. Franklin, a physicist, philosopher, and competent historian, has written extensively on episodes in the history of high-energy physics (e.g., Franklin 1979, 1983), and his 1986 book, *The Neglect of Experiment*, did much toward prodding practitioners of the philosophy of science and of the history of science toward re-evaluating the significance of experimental practice.<sup>28</sup> Franklin's desire, however, is to show how various episodes in the history of physics can be reconstructed rationally according to Bayesian probability theory. Even if scientists are not Bayesian agents, consciously calculating prior probabilities and then plugging them into the equation for Bayes' Theorem, Franklin believes one can do the history of science and show that physicists, at least, make theory-choices in accordance with a Bayesian methodology. Furthermore, the singularly important historical record that the good Bayesian historian/philosopher must employ in demonstrating rational theory-choice is, for Franklin, the published scientific paper. Indeed, Franklin (1986) goes as far as to say:

I think that the information acquired by an experimenter, by any means, is essentially that contained in the published work, and I think that the published reasons given both for the motivation of the experiment and for the acceptance or rejection of hypotheses are those that in fact determined the course of the work. Whatever an experimenter's private reasons for believing in a result, I think that only those that the author is willing to state publicly should be considered in discussing the validity of those results. (pp. 5-6)

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<sup>28</sup> Ackermann (1989) coined the phrase, "The New Experimentalism," in a review article in the *British Journal for the Philosophy of Science*. His article is a review of Franklin's (1986) *The Neglect of Experiment*. Other New Experimentalists Ackermann identifies are Hacking (1983) and Galison (1987).

Clearly, Franklin's aim is to relocate epistemic sovereignty in experimental practice using Bayesian probability, a traditional tool of the philosophy of science. His gesture toward detailed historical analysis, which is characteristic of some of his earlier work in the history and philosophy of science (e.g., Franklin 1979, 1983), turns out to be empty.

Another prominent New Experimentalist is Robert Ackermann (1985), who, as mentioned above, coined the phrase 'the New Experimentalism.' Ackermann locates epistemic sovereignty in what he calls "data domains." Data domains, according to Ackermann, are "factual niches to which theories adapt. . ." (p. 126). Extending this usage of Darwinian metaphors, Ackermann develops a complex, and often confusing, view of survival of the fittest facts. He attempts to formulate a dialectical view of scientific practice, with theory and experiment involved in a dynamic relationship, and with the relationship to be specified by the particular historical episode.<sup>29</sup> Data domains, and the role in instruments in defining them, however, are what ensure that epistemic sovereignty is retained. According to Ackermann (1985):

An instrument breaks the line of influence from interpretation to observation, or from theory to fact. (p. 128)

And further:

Instruments function to break off the influence of assumption on personal observation. If they did not exist, the fact of the influence of theory on perception might mean that shared data would be impossible. Where they do exist, a level of objective fact . . . is more likely to be achieved. (p. 129)

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<sup>29</sup> Ihde (1991) provides a somewhat more sympathetic reading of Ackermann's (1985) book. In his defense of "instrumental realism," which is an attempt—with much conceptual debt to Foucault and Heidegger—to outline how the philosophy of technology and the philosophy of science might profitably interact to inform an account of how the instruments of science can lead to a realism about the natural world, Ihde (1991) notes Ackermann's (1985) commitment to progress (pp. 90-3), his "vestigial empiricist strand" (p. 94), and his ultimate "instrument-revealed natural reality" (p. 94). Ihde apparently approves of the moves toward progress and realism; his main criticism of Ackermann is that he "does not want to develop a totally hermeneutic approach to natural reality. . ." (p. 94). Ihde's (1991) realism, though developed through a thoroughly Continental approach to scientific practice, costs too much metaphysically. It could benefit from a re-evaluation in the light of narrativity, especially that of Rheinberger (1994, 1997). Davis Baird (2004) and James Woodward (2003a, b) are two philosophers who have more recently argued for what they call "instrumental realism." Baird reifies instrument knowledge by locating instruments in Popper's "third world," thereby ensuring the sovereignty of experimental practice. Woodward's views I consider in Ch. V below. Ihde (1991) claims the term 'instrumental realism' originated with him in 1977 (p. 150, n. 1).

Clearly, Ackermann's goal is to show that focusing on "actual scientific practice" will preserve some form of epistemic sovereignty. In his 1989 review article of Franklin's (1986) *The Neglect of Experiment*—entitled "The New Experimentalism"—Ackermann praised Franklin's book and stated that "it should be required reading for philosophers of science" (p. 185). As for the significance of the New Experimentalism in general, Ackermann (1989) claims the following:

The philosophy of science seems to be in a state of flux, and the possibilities opened up by the new experimentalists seem to offer genuine hope for a recovery of some of the solid intuitions of the past about the objectivity of science, but in the context of a much more detailed and articulate understanding of actual scientific practice. (p. 190)

Indeed, while good historical analysis is deemed essential to an adequate understanding of the nature of scientific change, for Ackermann and most of the New Experimentalists, the aim of such detailed historical investigation is not to illustrate the contingency of history, the locality of scientific results, or the cultural context of scientific practices and methods. Their aim, instead, is to extract from the historical record evidence that will support the view that proper scientific practice leads to intrinsically objective results, and that these results are characterized by an increase in truth content over time (see also Ackermann 1994). That is, they hold that there is something about this practice itself that is privileged in a globally epistemic way.

In addition, Ackermann (1985) pays little, if any, attention to self-consciously evaluating his historiography and how such a concern might affect the cognitive significance of his own narrative situation and reconstruction. Instead, his data domain construct and his historical interpretation of selected case studies in the history of experimentation are used to buttress a version of convergent realism:

A new theory will incorporate major features of older theories, but it will modify some features in extending its applicability to wider domains of data. (p. 153)

And further:

The fact that some theories can replace older theories, or eliminate them from science, may seem incompatible with the idea that any theory that achieves community acceptance at some time in the development of science captures an aspect of reality. (p. 157)

Clearly, Ackermann believes that this is the case. His convergence, however, is circumscribed by the data domain. It is not convergence toward one unified scientific truth, but to truth within a data domain. The fact remains, however, that epistemic sovereignty is retained. Ackermann has specified what he takes to be the correct way to characterize the global epistemic structure of science, according to which scientific knowledge moves closer to a data domain-constrained notion of truth—truth about some slice of reality. And *if* science is done in this way—if its normative, hence epistemic structure is not violated in practice—then one’s efforts are objective, and “legitimated scientific fact” (p. 132) is likely to result. Instruments, in the end, function to specify the limitations of the data domain and to “locat[e] the facts within them” (p. 128).

Another example of “New Experimentalist” philosophy is that of Nancy Cartwright. Cartwright (1983) argues persuasively that the concept of scientific law is fraught with many difficulties, not the least of which is that laws require one to “abstract” from the actual experimental contexts in science (cf. van Fraassen 1989). In short, the abstract claims presupposed by scientific laws do not describe or characterize what goes on in actual science, at least physics, on which Cartwright focuses. In *Nature’s Capacities and their Measurement* (1989), Cartwright continues her argument for a “stringent empiricism” (p. 6), this time focusing on “capacities” and their role in accounts of causal explanation. For Cartwright (1989), scientific laws come at too high a metaphysical price; besides, real science does not need them (p. 8). However, to preserve the concept of causality, which “is an objective feature of our scientific image of nature” (p. 2), Cartwright believes we need initially a “structure with three tiers.” (p. 228) As she states:

At the bottom we have individual singular causings. At the top we have general causal claims, which I render as statements associating capacities with properties—‘Cash-in-pocket can inhibit recidivism’, or ‘Inversions have the capacity to amplify’. In between stands the phenomenal content of the capacity claim—a vast matrix of detailed, complicated, causal laws. These causal laws, which stand at level two, provide no new obstacles for the Humean beyond those that already appear at the bottom level. For these are just laws, universal or probabilistic, about what singular causings occur in what circumstances what percentage of the time. (p. 228)

Now, in this structure one still finds laws—why? This is because, Cartwright (1989)

explains, if her construction of an appropriate image of science is correct, the practice of science—experimental testing—will eventually, in any given case of scientific practice, render the middle level, with its laws, expendable and eliminable (pp. 179-82, 228-30). Ultimately, as with most of the New Experimentalists, this strategy rests on an argument for realism, thus subverting Cartwright's (1989) avowed "radical empiricism" (p. 7) and its aversion to metaphysics.

For Cartwright (1989), the most important feature of science that her neo-Popperian empiricism must retain is *testability*. Testability and measurement are the keys to the objectivity of scientific knowledge, and they are where Cartwright locates epistemic sovereignty:

Each scientific hypothesis should be able to stand on its own as a description of reality. It is not enough that a scientific theory should save the phenomena; its hypotheses must all be tested, and tested severally. This, then, is an empiricism opposed at once to wholism and to the hypothetico-deductive method. . . . Scientific hypotheses should be tested, and the tests should be reliable. They should be powerful enough to give an answer one way or another. The answers will only be as sure as the assumptions that ground the test. But it is crucial that the uncertainty be epistemological and not reside in the test itself. That is why I call this empiricism a kind of operationalism, and stress the idea of measurement. Measuring instruments have this kind of ability to read nature. If the measuring instrument operates by the principles that we think it does, and if it is working properly, and if our reading of its output is right, then we know what we set out to learn. A measurement that cannot tell us a definite result is no measurement at all. (pp. 5-6)

Hence, in order to escape from the extremes of the positivists and the advocates of deductivism, Cartwright (1989) constructs an inductive empiricism that retains the major components of Popperian testability. In doing so, however, she eliminates the need for scientific laws and causal laws (singular causes will do), while retaining as more basic the notion of *capacity*—measurable capacities as those objective features of nature that "are tendencies to cause or to bring about something" (p. 226). Furthermore, these capacities are "more than modalities; they are something in the world" (p. 181). As she argues:

It is not the laws that are fundamental, but rather the capacities. Nature selects the capacities that different factors shall have and sets bounds on how they can interplay. Whatever associations occur in nature arise as a consequence of the actions of these more fundamental capacities. In a sense, there are no laws of associations at all.

They are epiphenomena. (p. 181)

Indeed, Cartwright (1989) ultimately believes that capacities ought to be taken as real, at least if they survive tests. Moreover, hers is an empiricism that retains a notion of causality without recourse to counterfactuals; it is causality underwritten by capacities, which are—if they are measurable and pass the stringent tests of science—real features of the physical world.

Cartwright's (1989) radical empiricism, for all its gestures toward actual scientific practice, the elimination of speculative metaphysics, and toward a re-evaluation of the positivist worldview, retains an underlying epistemic sovereignty located in the ostensibly powerful methods science possesses to measure and test hypotheses with unequivocal results. Similarly, her commitment to the reality of capacities—not quite entities of nature, but not to be taken as “mysterious . . . or occult powers” (p. 229)—seems itself a commitment with too high a metaphysical price: a price paid, it seems, in order to ensure the testability she so urgently needs for her image of science. But testability, one could argue, is still what is at stake. Separating signal from noise—knowing how to eliminate all background; knowing when background has been eliminated; making a decision to end an experiment; judging that you have adequately performed an experiment, which in some sciences, for example population genetics, is not always the testing of specific hypotheses (Burian 1994a)—these are the activities of scientific practice that need elucidation; they are what is at stake in this dissertation. Presupposing any dictums of how those tests work, or what constitutes the furniture of nature, especially in the absence of detailed historical investigation, are strategies against which this dissertation is arguing.

Finally, consider Ronald N. Giere, another philosopher of science who offered in the 1980s an account of scientific change that was motivated toward escaping both the problems of social constructivism and those of traditional philosophy of science. Giere's (1988) book, *Explaining Science: A Cognitive Approach*, begins with a commitment to philosophical naturalism, which for Giere is “the view that all human activities are to be understood as entirely natural phenomena, as are the activities of chemicals or animals” (p. 8). Giere presents his naturalism as a way to “abandon” (p. 12) the usual views of rationality (p. 7) and



correspondence realism (p. 6) inherent in traditional epistemology, because they do little justice to the actual practice of science (e.g., pp. 12-17, 104-5). Similarly, he believes we should reject social constructivism, because it does not capture what scientists actually do (e.g., pp. 132-3). Instead, we should look at the “causal interaction between scientists and the world” (p. 4). For Giere, this means using the resources of the cognitive sciences, broadly construed (p. 2), to study primarily the experimental practices of scientists (chs. 5, 7, 8), but also their strategies of decision-making (ch. 6). The main goal of accounts of science, for Giere, is to use cognitive science to show “how people deploy various sorts of schemata in giving explanations—and in understanding them” (p. 105).

For all his efforts to explain what scientists actually do, Giere (1988) ends up advocating a form of realism he calls “constructive realism” (ch. 4). This realism is neither a correspondence between scientific theories and the natural world, nor between a social theory or social account and some empirical social reality. Instead, it is a “similarity” between a “model” constructed or proposed by a scientist and a “real system” (pp. 92-3). Giere considers this similarity to be a relation between “two nonlinguistic entities” (p. 20), thereby dispensing with talk of correspondence or truth; hence truth “plays only the relatively trivial role of being a relationship between language, which may be used to characterize models, and the models so characterized” (*ibid.*). In this realism, we reject the “strict causal necessity” of traditional empiricism in favor of the notion of “causal tendency” as a “propensity”—and such propensities are, “for the modal realist, taken to be features of the real world” (p. 102). Indeed, like Cartwright (1983, 1989), Giere rejects universal laws and causal laws in favor of capacities (tendencies or propensities) that cause something to happen in the world. However, the justification of scientific decisions on when a model has been shown to be adequately similar to a real system is not a matter of correspondence or rationality; it is, for Giere, an empirical matter to be studied naturalistically with the tools of cognitive science.

What seems ultimately to motivate Giere (1988) to distance himself from traditional empiricism and social constructivism is an argument that hinges on the empirical adequacy of those accounts of science. On the one hand, empiricists are wrong, because they worry

about the rationality of the scientific enterprise and the problem of the correspondence of theories to the real world. Giere believes that these worries are misguided, because theories of rationality cannot accurately describe what goes on in scientific practice, and because scientists do not compare theories to the real world using logical necessity as their guide; what they do is compare models (embodied sometimes in hypotheses) to real systems to see if they are similar enough. On the other hand, the constructivists are also wrong, because their accounts presume that scientists are wrong to take their models to be similar to real systems, and hence (by inference) real; scientists do in fact take some models and entities to be real. As Giere (1988) argues:

Empiricist philosophers, such as van Fraassen, would argue that . . . scientists are not really justified in believing that there are such things as protons. Constructivist sociologists would claim that these scientists, through their social practices, have deceived themselves into thinking that their own social constructs have an independent existence. My view is that these scientists are more or less correct in their beliefs about protons and neutrons and that we, as interpreters of science, must invoke the reality of things like protons and neutrons if we are to provide an adequate scientific account of their activities. (p. 124)

Clearly, in his constructive empiricism, Giere (1988) has utilized a form of realism as a guide for studying science scientifically, in addition to privileging the methods of the cognitive sciences—this is where he locates epistemic sovereignty. Nevertheless, Giere does not think his realism is viciously circular (pp. 10-12); he wants the scientific resources of cognitive science to judge whether the judgments of scientists—those specifying what aspects of their models are similar to real systems—are warranted. However, when Giere asserts that we “must invoke the reality of things like protons and neutrons” (p. 124), does he mean we invoke the *similarity* of physical models to the phenomena associated with protons and neutrons (which seems to suggest agnosticism on whether they are as real as chairs), or do we invoke the reality of these entities as things in the real world? Similarly, when he states that the modal realist takes causal tendencies and propensities as features of the real world, do we invoke a similarity relation, a pragmatic relation, or a necessity relation?

It seems that how one answers this question ought to make a difference for Giere, since he criticizes the constructivist position for not being able to make sense of the success

of science, “and particularly the success of science-based technology, since the seventeenth century” (p. 4). If scientists merely compare aspects of their models to the world, and those models do not capture something of its real features, it is not clear how Giere has gone beyond constructivism.<sup>30</sup> How the agency of the natural world acts to help constrain what comes to be scientific knowledge is unclear in Giere’s account, largely because he grounds his account in theories of the (cognitive) sciences of how humans perceive the world, rather than in the notion of scientific practices themselves being examples of causally interacting systems already in the world (Rouse 2002a). Ultimately, it is a body of scientific theories that grounds Giere’s account; hence, his account fails to be reflexive, despite his claims to the contrary (p. 16), for he has privileged the very subject he is attempting to explain. Only a naturalism that evades this problem, one that grounds itself in ordinary experiences of being in the world, rather than in particular scientific explanations for that experience, will pass the test of reflexivity that the constructivists have failed.

### **The Principal Case Study**

The issues raised in this dissertation are best considered in concrete terms set within an historical framework. By telling stories for which the technological infrastructure construct provides a coherent, evidence-based, and postmodern guide, I can show how its use

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<sup>30</sup> Giere’s (1999) more recent book, *Science Without Laws* (Chicago: University of Chicago Press), is a collection of previously written articles and chapters, except chapter 4, which is another defense of naturalism and realism. When constructing accounts of successful science, Giere argues that “agreement between data and prediction is a good indicator of fit between model and world, or, more cautiously, a good indicator that the proposed model *fits better* than any others regarded as plausible rivals” (p. 75). This fit between a model and the world, he argues, “may be thought of like the fit between a map and the region it represents” (p. 82). These maps, however, “provide access to reality, access that is, nevertheless, always partial” (p. 81). These maps do not mirror reality, as they are incomplete, but they do refer to things in the real world. While Giere explicitly rejects convergent realism (pp. 77-8), he again seems to be assuming realism from the beginning; indeed, he again rejects the notion that he has violated his own prescription against *a priori* metaphysics. His retort is this: “Proceed as if the world were this way. As such [his view] needs no *a priori* justification. Its vindication, if any, would be in its success” (p. 79). As mentioned above, it is not clear how this account is superior (or even fundamentally different) from Latour’s (1987, 1992) view, yet Giere claims his version of realism is necessary to make sense of the success of science, something he believes constructivists cannot do. But Latour (1987, 1992) is no constructivist; it seems he would agree with much of Giere’s account, yet would want him to include more analyses of “external” factors. Nevertheless, the inference from success to reality (rather than from reality to success) seems to be a feature of both accounts.

is superior to those in which the metanarratives of modernity are presupposed. The main case study I consider in this project is radiation genetics in the Cold War (see Chs. VII and VIII below). The discipline of radiation genetics became politically polarized after World War II with the advent of atomic bombs and the atmospheric testing of atomic weapons. A central problem for radiation genetics was to determine the genetic effects of radiation—that is, the extent of damage to the human genome which may be inflicted on those exposed or, more significantly, inherited by future generations—particularly that caused by low-level ionizing radiation, as might be produced, for instance, by fallout from weapons testing. Clearly, radiation genetics as a field intersects in important ways with other fields of genetics, namely population genetics, evolutionary genetics, and human genetics (cf. Seltzer 1993). In this project, I focus mainly on a controversy initially central to population genetics, the classical/ balance controversy.<sup>31</sup> The theoretical and experimental implications of this controversy, however, had bearing on most areas of genetics.

By the early 1950s, two predominant positions had emerged in theoretical population genetics. According to the classical view, whose main proponent was the geneticist Hermann J. Muller (1890-1967), most genetic loci (genes) in natural populations are homozygous for “wild-type” genes. That is, identical alleles (the gene components from each parent) exist at these loci, and the particular alleles in the homozygous condition may be taken to be the “normal” and even “optimal” condition in terms of fitness (a measure of the organism’s ability to survive and reproduce in a particular selective environment). For Muller, this boiled down to the position that favorable mutations are *rare* in natural populations, and that evolution operates to eliminate deleterious mutations from the population. Hence, natural selection must wait for the occasional favorable variation, as most mutations would be harmful. In terms of the genetic effects of radiation, the implication is clear: any increase in the mutation rate would be harmful to future generations. Hence, any amount of increased radiation exposure would be expected to

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<sup>31</sup> Beatty (1987), Crow (1987), Dietrich (1994), Lewontin (1987), and Seltzer (1993) provide historical accounts of the classical/balance controversy. Lewontin (1974), a former student of Dobzhansky who held (and still holds?) a position closer to the classical view, presented the classic account of the epistemological problems of the controversy, many of which still remain today.

increase the “genetic load” of deleterious mutations in the population, resulting in future generations with an increased number of individuals with varying amounts of genetic damage. Muller (1950) set forth his position in his now classic paper “Our Load of Mutations.” There Muller originated the “genetic load” concept<sup>32</sup> and gave his detailed arguments against increased exposure to radiation based on that theoretical position. Muller, who won the 1946 Nobel Prize for his work on heritable mutations in *Drosophila melanogaster* (Muller 1927), could not have presented his arguments in a more emotionally charged social and political context: the Soviet Union had exploded its first atomic bomb in 1949, Truman ordered the crash-program on the hydrogen bomb in 1950, and the era of programmatic atomic testing was to begin in 1951.

The main adherent of the balance view was the theoretical population geneticist Theodosius Dobzhansky (1900-1975). It was Dobzhansky (1955) who outlined and labeled the opposing positions the “classical” and “balance” positions at the 1955 Cold Spring Harbor Symposium, an international gathering of population geneticists. According to the view that Dobzhansky held by that time, most genetic loci in natural populations are heterozygous. That is, the genes have different allelic contributions from each parent. Such variation in natural populations provides the raw material upon which natural selection can operate. Dobzhansky believed that heterozygotes were better protected against changes in the environment than homozygotes; therefore, in changing environments they were more fit (Lewontin 1987, pp. 344-5). Organisms with such genetic variability would have an advantageous “phenotypic plasticity” that would serve as a buffer from adverse environmental stresses (Lerner 1954). The implication for the genetic effects of radiation is again clear. If, in the extreme interpretation of the balance position, most new mutations are of the balanced type, one could predict that an increase in heterozygosity caused by mutations resulting from increased exposure to radiation might lead to increased fitness of the population as a whole. Several of Dobzhansky’s students, including Bruce Wallace, had

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<sup>32</sup> Wallace (1970) credits Muller with originating the term ‘genetic load’ (p. vii). Muller first used the term in his Presidential Address before the American Society of Human Genetics in New York City on 29 December 1949. Muller’s (1950) paper, “Our Load of Mutations,” is based on this address. However, Wallace (1991) later suggests the concept of genetic load goes as far back as Haldane’s (1937) paper, “The Effect of Variation on Fitness” (pp. 1-2).

performed experiments that seemed to confirm this interpretation of the balance view. These results seemed to suggest that a modest increase in radiation exposure might actually be populationally *beneficial* to future generations. Obviously, such an interpretation was antithetical to the classical view and the genetic load argument. In terms of justifying increased radiation exposure, the balance view seemed to offer positive support.

In fact, the United States Atomic Energy Commission (AEC) used this interpretation to deflect increasing public opposition to atomic testing (cf. Beatty 1987, pp. 301-11; Seltzer 1993). Moreover, AEC officials publicly offered the argument that low levels of radiation might be genetically beneficial to the human race. This reassurance was offered at the height of the international fallout controversy of the 1950s when the candor of the AEC was in question. With the experiments of Bruce Wallace (Wallace and King 1951; Wallace 1956, 1958), Mikhail Vetukhiv (1953), and others—both students of Dobzhansky—these arguments had at least indirect experimental evidence, and they became standard components of the tactics of the AEC in arguing the harmlessness or benefit of radioactive fallout (Seltzer 1993).

In order to tell this story, or any compelling story about science or technology, I argue that adopting the postmodern perspective I call *postmodern naturalism* and telling the story utilizing the construct developed in this dissertation—*the technological infrastructure of science*—will result in a fuller, more coherent, and more evidence-based interpretation of the events, than if the story were to be told from the perspective of modernity in which epistemic sovereignty is retained. However, I must first develop this postmodern perspective further; this I do in Chapters II and III, in which I explore postmodern historiography and postmodern philosophy of science, respectively. In Chapter IV, I revisit Pitt's concept of the technological infrastructure in more detail and provide criticisms in the light of the postmodern principles developed in the previous chapters. Then, in Chapter V, I extend and (re)synthesize Pitt's construct by considering the views of other researchers and exploring how those views can benefit its development. In Chapter VI, I explore some of the epistemological and political issues involved with the development of the adaptationist research program underpinning many disciplines, including population and radiation

genetics, in order to lay the groundwork for the case study presented in later chapters. Then, in Chapters VII and VIII, I tell a story (a set or field of interrelated narratives), set in the 1950s and 1960s, of the Atomic Energy Commission, population genetics, radioactive fallout, and radiation policy, using this construct. At the conclusion of Chapter VIII, I indicate how what I have argued could be improved by future research, and I explore future possibilities for research using the technological infrastructure of science and the historical narratives begun in this dissertation. Again, the goal in this dissertation is to show how the technological infrastructure of science construct, with its postmodern naturalist “foundation,” allows stories to be told that are superior to those in which epistemic sovereignty, privileged scientific rationalism, social realism, or other construct(s) of the metanarratives of modernity are presupposed, whether explicitly or implicitly.

## CHAPTER II

### Toward a Postmodern Historiography of Science

The present as the future of the past is not a “result”—  
whatever that means—of the past; the past is the result  
of a future—its presence as a surrogate.

—Hans-Jörg Rheinberger<sup>33</sup>

In this chapter, I review and apply some elements of postmodern theory to the historiography of science and technology in order to provide the conceptual foundation for the technological infrastructure of science construct. Works by Friedrich Nietzsche ([1883-5] 1954, [1885] 1955), Michel Foucault ([1969] 1972), Jacques Derrida ([1967] 1976, [1972] 1981), Jean-François Lyotard ([1979] 1984), Joseph Rouse (1990, 1991a, b), Hayden White (1981a, 1987), Frank Kermode (1967), Paul Ricoeur (1981, 1984), and Hans-Jörg Rheinberger (1994, 1997) provide the theoretical foundation for this chapter. I incorporate aspects of these scholars’ works into a formulation of a postmodern historiography. The arguments and recommendations I formulate apply to the writing of history, to doing history. Even if most historians and historians of science have come to accept some version of postmodern historiography, it still remains that certain metanarratives may be implicit in their historiographical stances. In addition, scholars in other fields, such as Philosophy of Science and Sociology of Science, depend upon empirical histories as evidence for their substantive theories of how science works, and whether their historiographical stances stand up to postmodern scrutiny has bearing on their views of science and its place in culture. Moreover, I argue that the degree to which scholars adhere to postmodern historiography also has crucial relevance for broader cultural narratives and for education, including education in the schools and universities, and in national museums. Finally, in order to explicate fully my position on the technological infrastructure of science, it is necessary to articulate further my argument for the significance of historiography and for how it bears on narrativity.

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<sup>33</sup> Rheinberger (1994), p. 70.



Postmodern historiography will be a key component of the technological infrastructure construct; it will help guide its use in (re)writing historical accounts and for (re)conceptualizing properly how the sciences and technologies are situated as significant cultural practices.

I begin first by outlining what I mean by postmodern historiography. I then consider how the pre-constructivist debates in the 1960s and 1970s on the history and philosophy of science of Lakatos, Laudan, Kuhn, and others were dominated by and constitutive of an adherence to the modern metanarratives of rationality and progress. Next, I show how the debate, as reconfigured by the social constructivists, retained essential elements of the metanarratives of rationality and progress. Only with the advent of the focus on scientific practices in the 1990s, as indicated in Chapter I, did these modern metanarratives begin to loosen their hold on scholarship in the history, philosophy, and social studies of science, although this hold is still strong in some disciplines, for example, the philosophy of science.<sup>34</sup> Then, I consider the case studies of scientific creationism and the Smithsonian Museum's 1995 *Enola Gay* exhibit as examples that illustrate why adopting a postmodern historiography is compelling, and how it can make a difference. Finally, I revisit the notion of postmodern historiography in order to raise some issues in the philosophy of history that

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<sup>34</sup> Paul Forman (2007) indicates that a major shift in thinking took place around 1980, when instead of viewing technology as subsumed under science or as applied science, a reversal occurred, and science became viewed as subordinate to technology. Forman (2007) criticizes the discipline of History of Technology—because of its ignorance of science, of how science works, for not adopting a practice-oriented historiography, and for playing disciplinary politics—for having missed out on this major cultural demarcation, by declaring the science-technology relationship as “a problem better ignored than addressed” (p. 59). Indeed, Forman states that this change “involves nothing less than an inversion of our mode of being, and is therefore effectively a demarcator, both of state of mind and of point in time, between modernity and postmodernity” (p. 13). Unfortunately, for an historian who would seem, at least through his work, to accept a version of postmodern historiography, Forman (2007) goes on to lament the loss of modernity and argue that while historians should attempt to find the “reigning cultural values of the historical era” (p. 68), historians can perform cultural explanation “only if [the cultural] is not postmodernized into a multiplicity of coexisting incoherent cultures; can perform this service only if, as modernity would have it, culture is conceived as a constellation of presuppositions integrating the outlooks of actors in diverse social situations over an extended period of time; can perform this service only if the historian's task is understood to be delineating that constellation of cultural values by connecting the ‘dots’ of temporally, socially, and intellectually ‘scattered’ articulations. Without this, admittedly quite old-fashioned, conception of the historian's task we would be hard pressed to conceive of, let alone define, distinct historical eras. If, furthermore, we reject the notion of historical eras as a modernist illusion, or, worse, oppression, we are inevitably also rejecting history as a scholarly discipline” (pp. 68-69). For all his acceptance of cultural influence on the course of science, Forman (see esp. 1991), and ultimately many other historians, do not make the jump to postmodern historiography and postmodern naturalism advocated in this dissertation.

have relevance for the technological infrastructure of science, which I address in the next chapter.

## Postmodern Historiography

To illustrate what I take to be postmodern historiography, I start with Lyotard's ([1979] 1984) "incredulity toward metanarratives" (p. xxiv). Metanarratives are modes or structures of discourse that characterize discursive and non-discursive activities, and provide the basis of the legitimation of knowledge. That is, the game of metanarratives is a knowledge-legitimizing game. In a particular culture at a particular time, only certain types of knowledge will count as "legitimate"—those characterized by the prevailing metanarrative(s).<sup>35</sup> However circular this definition may be, it will be historical and historiographical argument that will provide the basis for any particular metanarratives being at work in history. In this dissertation, the metanarratives of modernity are the particular structures I explore, for they have particular relevance for the knowledge disciplines of science, the history of science, the philosophy of science, and related disciplines. At this point, it is useful to consider what I call the various levels of history:<sup>36</sup>

- History-0:** "actual" or "real" history of the past *wie es eigentlich gewesen ist*,<sup>37</sup> our epistemic access to this is limited, for many reasons
  
- History-1:** the evidence we have of History-0, whether written documents, films, oral histories, material artifacts of the past, etc.
  
- History-2:** a chronology of events of the past without interpretation

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<sup>35</sup> Stephen K. White (1991), for example, views Lyotard's metanarratives as "those foundational interpretive schemes that have constituted the ultimate and unquestioned sources for the justification of scientific-technological and political projects in the modern world. Such narratives, focusing on God, nature, progress, and emancipation, are the anchors of modern life" (p. 5).

<sup>36</sup> Laudan (1977, p. 158) and Kragh (1987, pp. 20-24) point out the distinction between History-1 and History-2, although their distinctions correspond to History-0 and History-3, respectively, as I employ them in this chapter. Hayden White (1987) analyzes the distinction between History-2 and History-3 (as defined above), a distinction, as he sees it, between a "chronicle" of events and a narrative interpretation (pp. 5-13).

<sup>37</sup> Leopold von Ranke (1795-1886), *Geschichten der romanischen und germanischen Völker von 1494 bis 1514*, Sämtliche Werke vol. 33, Leipzig, 1855, p. VII. First published in 1824. On the significance of von Ranke's historiography and his efforts to make history into an empirical scientific discipline based on documentary sources, see Rösen (2005), ch. 3.

•**History-3**: what historians normally call history; an interpretation, explanation, or interpretive narrative about the past

•**History-4**: what historians call historiography; the norms, implicit or explicit, utilized by historians to interpret history; possibly a metanarrative

•**History-5**: the reasons used to justify choosing or implementing an historiography; what I am discussing in this chapter

So, this position then is really a History-5, a meta-metanarrative: a way of structuring appropriate historiographies, or metanarratives. Hence, there *will* be metanarratives—though Lyotard, Foucault, Nietzsche, and others instruct us not to take them too seriously, or to doubt their veracity—although they will be of a certain kind. Indeed, they will not be the metanarratives of modernity; they will be instead *postmodern*. Building on the analyses in Chapter I above, the following are characteristics of the historiographical stance an scholar should adopt when implementing a postmodern historiography:

1. The first I have stated above: *incredulity toward metanarratives*, and, in addition, narratives. Holding to the “truth” or finality of one historical interpretation or historiography amounts to the acceptance of a totalizing metanarrative. The reasons for rejecting the notion of a definitive or totalizing, History-3 or History-4—that is, narratives and historiographies—I discuss below.<sup>38</sup>

2. The postmodern scholar should *question the epistemic authority of science* as a

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<sup>38</sup> Foucault ([1968] 1991) stated that his “criteria make it possible to substitute differentiated analyses for the theme of totalizing history. . . . They make it possible to describe, as the *episteme* of a period, not the sum of its knowledge, nor the general style of its research, but the divergence, the distances, the oppositions, the differences, the relations of its various scientific discourses: the *episteme* is not a sort of grand underlying theory, it is a space of *dispersion*, it is an *open and doubtless indefinitely describable field of relationships*” (pp. 54-5). The *episteme* is, for Foucault, “*not a general developmental stage of reason, it is a complex relationship of successive displacements*” (p. 55). As Foucault states, his aim is to do the following: “Nothing, you see, is more foreign to me than the quest for a sovereign, unique and constraining form. I do not seek to detect, starting from diverse signs, the unitary spirit of an epoch, the general form of its consciousness, a kind of *Weltanschauung*. Nor have I described the emergence and eclipse of a formal structure destined to reign for a time over all the manifestations of thought: I have not written a history of a synocopated transcendental. . . . I have studied, in turn, ensembles of discourse: I have characterized them; I have defined the play of rules, of transformations, of thresholds, of remanences. I have collated different discourses and described their clusters and relations. Wherever it seemed necessary, I have been prepared to add to the *plurality* of distinguishable systems” (p. 55). For critical analyses of the evolution of Foucault’s positions on archaeology, genealogy, and truth, see the essays in the volume edited by Hoy (1986) and David R. Shumway’s (1989) *Michel Foucault*, Boston: Twayne Publishers.

starting point for historical analysis, lest she adopt the metanarratives of modernity, according to which science is unquestionably superior epistemically. This should be replaced with a serious consideration for historical contingency—it could have happened otherwise—and how that contingency will be revealed by the historical evidence, that is, History-1.

3. The postmodern scholar should be *reflexive*. That is, the historian must constantly be evaluating his stance and position as author, in a reflective way, in order to reveal purposefully and candidly *how* the historical evidence is supporting the History-3, the interpretation. Moreover, she must evaluate critically *how* the narrative or History-3 is structuring and using the evidence, History-1. Many scholars succeed at these tasks. But many scholars, including many historians, do not explicitly reveal *how* the History-4, or historiography, is guiding the narrative, the interpretation, or History-3, nor do they worry about History-5, or that which I am in the process of worrying about right now (cf. White 1995).

4. Finally, the postmodern scholar should *reject internalist historiography and internalist history*. As the dominant metanarrative of twentieth century writings on the history of science (cf. Forman 1991), internalism can be viewed as stemming from the nineteenth-century ideals of high modernity: this is the notion that science is a rational, method-based enterprise; that it produces objective knowledge about the real world; that over time this knowledge is increasing in truth-content; and that hence this convergence toward the truth constitutes an increasing unity of the different branches of science and knowledge.

Internalism as the proper way to do history of science seems to have remained all but unchallenged by scientists and historians before Stephen Brush (1974) asked: “Should the History of Science Be Rated X?” Brush, a scientist and historian of science, was responding to the ongoing debates in the history and philosophy of science among Kuhn (e.g., [1962] 1970), Lakatos (papers dating back to 1962; 1978), Feyerabend (e.g., 1962), and their critics, on how to conceptualize science, its history, and its ostensibly privileged epistemic status (see, e.g., Lakatos and Musgrave 1970). With postmodernism, internalism meets its death.

Perhaps the most extreme example of internalist history comes from the philosopher

of science Imre Lakatos (1978a-d), whose notion of the rational reconstruction of the history of science—his metanarrative, or History-4—directs that there be *no connection* between History-1 and History-3, or between the available historical evidence and the interpretation of that evidence. Instead, Lakatos instructs us to adopt the metanarratives of modernity, according to which transcendental principles of rationality and progress will determine how history should have occurred, even if it did not turn out that way. In other words, History-1 is irrelevant; the historical *evidence* is irrelevant. To provide examples of this strategy, I show that this scheme for doing history lies hidden behind not only attempts to write histories of the controversies over evolution and creationism, but also behind the attempts—successful attempts—to squash the Smithsonian’s interpretation of the atomic bombings of Hiroshima and Nagasaki that was to be displayed in the Air and Space Museum’s 1995 *Enola Gay* exhibit. I turn first to a discussion of Lakatos’ “history.”

### **Internalism**

Internalism presupposes that a definitive history can be written to account for past events. While few if any internalists would be willing to admit this up front, it is virtually a necessary consequence of their view of science and change in science. Now, by internalists, I do not limit the discussion to those who admit only “internal” factors to the development of science. Those who admit, as did Lakatos (1978c), that “external” factors can influence the course of science, yet regard such infractions as limited and irrational, still uphold the internalist program. The key is rationality. Having an *a priori* definition of what constitutes rational, the internalists write their histories based on how rationality guarantees progress. In this way, they purport to write definitive narratives.

Such narratives mask serious historiographical problems, even assuming we would want to claim the epistemic sovereignty of science. For instance, it is evident that we cannot relive history, nor can we travel back in time. Therefore, our “connection” to the past is in principle indirect. Moreover, since our evaluation of the past is through evidence, such as persons, texts, manuscripts, physical artifacts, and so forth, if this evidence—indirect evidence—is not “complete” in some objective way, then our knowledge of the past is

incomplete. Further, even if the available evidence were “complete” in some ideal fashion, how would we know this? In other words, for us to *know* the point at which we had attained “complete” evidence, we would have had to previously form some *a priori* idea of what constitutes “completeness.” However, our knowledge of the past is available only through indirect evidence—so how could we possibly know beforehand what constitutes complete evidence?

Furthermore, even if we could reach agreement on completeness, what would guarantee convergent agreement on the *assessment* of this “complete” evidence? This problem again points up the distinction between History-2 and History-3. History-2 is, put crudely, the “facts” of the past that constitute some chronology of events. History-3, on the other hand, is some *interpretation* of a set of events by an historian. Clearly, History-1 is our evidence of “actual” History-0, for which we have only indirect access, as well as incomplete evidence.<sup>39</sup> History-3, then, is what the historian (in the general sense of the word, i.e., one practicing the writing of history) *does* with this evidence—s/he provides some sort of interpretation or explanation of the events in History-2. Now, because of the fact that such evidence, even if “complete,” is embodied within texts, archives, person’s memories, films, and so on, there is nothing *inherent* in this evidence that forces the scholar into one, unproblematic interpretation. Each scholar defines the scope of the analysis, and even what constitutes an appropriate explanation. To conclude that a definitive history is possible is simply to impose an *a priori* structure on History-2 (a History-4) requiring historical explanations to be *determinate*.

What I am arguing for (History-5) is that such a History-4 cannot coherently succeed; certainly it cannot be demonstrated. It can be argued for, but we should hope that such an absolute requirement be necessarily required by History-1 and -2. In any case, how could we *know* that History-2 is determinate? We might try *reconstructing* History-2 using determinate History-4 methods and see what results. But the question remains: is this

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<sup>39</sup> Some postmodern theorists are read as denying that we can get beyond the historical text, or any text, to historical “reality,” or History-0; Derrida is one notable example. Thus, they are read by some as claiming that History-0 is not a useful category, that it is a possibly dangerous category, or that it is fictional. In Chapter I, I gave an outline on how possibly to overcome this problem. Later in this chapter, I continue that argument.

*history?*

Such a program of reconstruction was undertaken, to an extent, by the philosophers of science Lakatos (1978c) and Laudan (1977). Their methodological norm (History-4) for historical explanation (History-3) is *rationality*, a rationality that guarantees progress in scientific knowledge. Both object to Thomas Kuhn's (1970) account of scientific revolutions, whereby, they believe, discontinuous breaks in the development of internal scientific knowledge render progress, rationality, and historical explanation tenuous, perhaps unattainable. As Lakatos (1978b) said of his own methodology in contrast to Kuhn's: "I shall try to explain . . . [my] stronger Popperian position which, I think, may escape Kuhn's strictures and present scientific revolutions not as constituting religious conversions but rather as rational progress" (p. 10). Laudan (1977), in unfairly criticizing Kuhn and Feyerabend, characterizes the problem this way:

. . . Kuhn and Feyerabend conclude that scientific decision making is basically a political and propagandistic affair, in which prestige, power, age, and polemic decisively determine the outcome of the struggle between competing theories and theorists. Their mistake seems to be one of jumping to a premature conclusion. They start from the premise that rationality is exhaustively defined by a certain model of rationality (each of them takes Popper's model of falsifiability as the archetype). Having observed, quite correctly, that the Popperian model of rationality will do scant justice to actual science, they precipitately conclude that science must have large irrational elements, without stopping to consider whether some richer and more subtle model of rationality might do the job. (p. 4)

Clearly, both Lakatos and Laudan are committed to imposing a rigid requirement (History-4) on what constitutes an historical explanation (History-3) of internal scientific knowledge, or "disembodied science," as Lakatos (1978c, p. 114) called it. The problem here is that Lakatos and Laudan offer their theories, accompanied with corresponding methodologies for applying them to historical cases, *as* historical explanations—that is, *in place of* historical interpretation. Lakatos's "research programme" and Laudan's "research tradition" are ostensibly presented as historical structures, to be imposed on internal scientific knowledge in order to guarantee some notion of rationality and progress. As Laudan (1977) put it:

When a thinker does what is rational to do, we need inquire no further into the causes

of his action; whereas when he does what is in fact irrational—even if he believes it to be rational—we require some further explanation. . . . Of course, this proposal—that rational behavior is the rule rather than the exception—is open to debate, but . . . it is preferable to the alternatives [especially cognitive sociology of science]. Precisely because it is preferable, normative evaluations—as opposed to purely descriptive ones—must play a role in historical explanations, for those evaluations tell us when our explanatory task is at an end. (pp. 188-9)

This view is problematic, because such an “explanation” does *not* explain or interpret History-1—the historical evidence—at all. It amounts to merely a quasi-chronological scheme for organizing scientific knowledge—“disembodied science.” As indicated above, such a purported “explanation” is not History-3 interpreting History-1 and -2; it is History-4 *organizing* History-2 and then seeing how History-3 happens to turn out. In the end, what does this say about historical evidence? Is this not the “manufacturing” (or ignoring) of the evidence of History-1 to construct a History-3, an interpretation? Are these *both* history?

It is interesting to note that Lakatos (1978c) admits that his rational reconstructions of the history of science are somehow alien to “actual” history; yet he tries to reassure us: “The rationalist historian need not be disturbed by the fact that actual history is more than, and, on occasions, even different from, internal history. . .” (p. 134). Apparently, it does not trouble Lakatos that he is not explaining actual history—history in which people in various places at certain times took action—but only logico-rational relations in Popper’s third world, that is, “the world of articulated knowledge which is independent of knowing subjects” (Lakatos 1978b, p. 92). Popper’s second world—the world of consciousness, and therefore, of human thought—and first world—the material world, including the *evidence* of historians (History-1)—are apparently irrelevant to a good historical explanation. Should not an historical explanation bear *some* relation to the first and second worlds (assuming we would want to take Popper’s demarcation of worlds seriously)?

This is not to suggest that the third world is irrelevant to history. The third world, however, in a sense is not completely independent of the second world, or even the first. The components of knowledge in the third world are generated by the consciousness of humans—the second world—at particular times and places in history, and for various reasons. To maintain that the knowledge of the third world is sharply independent of its birth process



removes its historical character. Similarly, historians must utilize artifacts in the first world—texts, manuscripts, films and other physical objects—in order to process evidence from them so as to create a history (History-3) that may come to reside in the third world of knowledge. Indeed, all three worlds are related, and all are relevant to historical interpretation.

Nevertheless, what Lakatos and Laudan propose is that we neglect the first and second worlds. They locate their historical “explanations” *solely* within the third world, the world of disembodied knowledge, without serious regard to their necessary connections to the conscious humans who generated the knowledge, nor to the nature of the evidence (artifacts) left behind by those humans. No wonder Kuhn criticized Lakatos’ conception of history as “not history at all but philosophy fabricating examples” (quoted in Lakatos and Zahar 1978, p. 192). Lakatos’ reply, at first glance, seems convincing: “I hold that all histories of science are *always* philosophies fabricating examples. Philosophy of science determines to a large extent, historical explanation. . .” (p. 192). But this holds true only if one equates “history of science” with “internal scientific knowledge severed from its origins.” Yes, if one’s philosophy is a third world rationality principle that is forced on an explanation (History-3) *despite* the evidence of History-1, or perhaps with no relation whatsoever with History-1, then Lakatos’ reply holds. However, as I have argued, it is difficult to see how Lakatos’ rational reconstructions constitute “historical” interpretation, since they ignore the very components and characteristics that constitute their historical nature.

What, then, are we to say about internalist historiography of this sort? If we are not to be constrained by *a priori* principles of rationality and progress that force an “explanation” that ignores the proper relationship between explanation and History-1 and History-2—one which purports to guarantee a final, definitive explanation in accordance with its very demands for rationality and progress—then one must give up hope of a definitive history of science. Furthermore, so-called internal history of science must be seen for what it really is: *not* history, in any meaningful sense. These attempts all have in common the deployment of the metanarratives of modernity, in order to save (at the expense of history) the cherished

notion of scientific progress. They are enterprises conducted almost exclusively in Platonic Heaven, with virtually no connection to the spatio-temporal events of the past.<sup>40</sup>

### **From Scientific Progress to Social Realism**

Literature in Science and Technology Studies (STS) and allied disciplines over the past twenty years or so recasts the debate over historiography in a new light. As outlined in Chapter I above, one way to get a handle on the debate is to conceptualize it as a conflict among those who wish to retain *epistemic sovereignty* and those who, as a matter of principle, reject it.

A major development in post-Kuhnian/Lakatosian/Laudanian scholarship has been the advent of *social constructivism*. Social constructivists, again, deny any role to “nature” in the development of scientific knowledge. For them, processes that are irreducibly social, political, and/or sociopolitical produce all scientific knowledge. History of science, then, amounts to uncovering the correct social explanation for the development of scientific knowledge (Bloor 1991, Collins and Yearley 1992a).

Partly as a reaction to social constructivism and the social realism that follows from it, a movement emerged in the 1990s that focused attention less on disembodied knowledge—whether supposedly objective scientific knowledge or the sociopolitical *real* of the constructivists—and instead focused on what history tells us about the actual practices of scientists. For the most part, the *practice scholars* in this vein deny both epistemic sovereignty and many of the methodological norms of the constructivists (cf. Pickering 1992). Again, epistemic sovereignty is the belief that science and/or scientific methodology, by virtue of their nature (e.g., content), are privileged epistemically (cf. Rouse 1993a). That

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<sup>7</sup> Palmer (1993) skillfully argues that Lakatos’s rational reconstructions have been unfairly judged by many, including Kuhn, Feyerabend, and Hacking. However, in arguing that his internal historiography may lead to meaningful historical interpretation, Palmer uncritically accepts certain precepts, including the use of privileged rationality as an historiographical guide, progress as a feature of the growth of scientific knowledge, and the notion that scientists themselves know “much better than philosophers what is and what is not scientific. . .” (p. 617). A key to Palmer’s philosophy of science can be seen in his contention that we may find “fuller conceptions of scientific practice” in Kitcher (1993) and Laudan (1984) (p. 620, n. 18). Fuller (1994) harshly criticizes Kitcher’s book, which unapologetically advocates progress and realism in science.

is, if one does science in accordance with this methodology, one is guaranteed an outcome—scientific knowledge—that is in a privileged (in some way or another) position to say something about the way the world is (Rouse 1993a). Practice scholars tend to reject this view, yet they want to retain some notion of the epistemic *authority* of science. That is, these scholars believe that the material “world” or “material culture” plays some role in the generation of scientific knowledge, and therefore that scientists have *some* degree of authority to claim to say things about the way the world is (cf. Pickering 1992a). Some practice scholars identify themselves as doing “cultural studies of scientific knowledge” and they question the usefulness of traditional historical “explanation” (e.g., Rouse 1993b, Rheinberger 1992a, b).<sup>41</sup>

What are the dynamics of this professional debate? First, some social constructivists ironically accuse practice scholars of advocating extreme relativism, and of not being able to say anything about “real life,” or about what scientists do—that is, practice! Those practice scholars who take reflexivity seriously some constructivists charge with being “hyper-relativist,” or with degenerating into a position that can be described as conservative, or even

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<sup>41</sup> Dear (1995) attacks Rouse’s (1993b) construction of the practice of “cultural studies of science” as indicative of a “confused state of affairs” (p. 155) in specifying what “cultural history of science” should mean. At stake seem to be at least two issues, neither of which Dear seems to be aware (pp. 156-7). First, what constitutes the definition of “culture”? Second, what criteria does Rouse use to demarcate those doing cultural studies of science from those doing something else? Rouse’s conception of culture (see Rouse 2002a and the analyses in Chapters III, IV, and V below) does not match Dear’s. Dear (1995) wants culture to be subservient to whatever “social” is taken to be, whereas Rouse takes culture to be the more foundational unit of analysis, yet Dear seems unaware of this. In addition, Rouse’s criteria for demarcation involve interpretation of the stance of the practitioner: does the practitioner advocate a form of epistemic sovereignty; does s/he advocate traditional historical “explanation” of the events of the past; does s/he advocate the bifurcation of nature and normativity, of the epistemological and the political? Dear (1995) focuses his criticisms through the lens of his view of the cultural, a view influenced by social history and the sociology of scientific knowledge (SSK), according to which “cultural meanings that are not also social meanings are exactly like colors that are not colors *of* anything—they are properties without a subject. Culture is real; but it is not a thing” (p. 166). For Rouse (2002a), as for Foucault, culture involves how to conceptualize *practices*, including scientific practices, yet those practices (discursive and nondiscursive) do not have meaning apart from the natural phenomena (in the world) and the norms involved with those practices. Hence, Rouse’s view does not bifurcate nature/social or nature/culture, as does Dear’s; culture is not a property of social groups. It is a way of making sense of the practices of groups of people, who themselves are always already in the world; those practices should be understood as patterns of causal intra-action in the world, in much the same way as natural phenomena. It is a strategy closer to Foucault’s genealogy than to the reductionism and social realism of SSK. Culture, as I extend Rouse’s analyses in this dissertation (see Chs. III and IV below), is constituted by the stories we tell ourselves (about the present or the past) about the world, but also by the real world, i.e., by our being in the world. It is narrative in nature (pun intended).

reactionary. This results, apparently, because practice scholars are willing to grant *some* authority, however limited and qualified, not only to scientists' claims, but also to agency in the material world, whereas most constructivists find this notion vacuous. Hence, it seems *any* granting of authority to the scientific worldview is met with a kind of political challenge—again, all is sociopolitical for the constructivists—wherein the views of the practice scholars are compared to the logical positivists or to right-wing politicians, against whom the normally left-wing constructivists are battling. It seems that this reaction results partly from the fact that many constructivists ignore or reject reflexivity issues, some so much so that their positions reify the social realm (e.g., Collins 1985).

As explored in Chapter I above, the constructivist position is one that advocates a kind of social epistemic sovereignty that reifies social explanations. After all, their primary concern is with taking sociopolitical action (or at least criticism), and not doing philosophy. Therefore, we also find these scholars criticizing practice scholars for not engaging in “empirical” work, that is, work based on historical investigation. However, one of the crucial factors the constructivists ignore or gloss over is the very connection between their empirical narratives and the social determinism they seem to advocate (cf. Rouse 1990, 1993b).

In addition, practice scholars correctly charge the constructivists with ironically upholding science's *need* for global legitimation—that is, epistemic sovereignty. In other words, having satisfied themselves that science lacks epistemic privilege, the constructivists feel justified in claiming that science *must* be all sociopolitics, yet their move is toward a privileged and disembodied social realism (cf. Fine 1996). It is the *lack* of epistemic sovereignty that seems to justify this move, thereby implicitly upholding the *need* for global legitimation, yet it is a lacking that the constructivists delight in repeatedly demonstrating; it has put them in business. Why, one might ask, is it a problem that science lacks epistemic sovereignty? Why should science need such legitimation? Maintaining that science requires sovereign epistemic legitimation, even if only implicitly, places the constructivists within the metanarrative of modernity. The postmodern position, as I interpret it and develop it in this dissertation, requires the dissolution of the sovereignty/lack of authority dichotomy. Scientists can, at the same time, lack a sovereign epistemology *and* maintain some version of

cultural authority to make knowledge claims. To maintain that because scientists have no privileged epistemology does not entail that their epistemology is vacuous, nor does it mean that “there is only one kind of thing—power granted by human agency” (Collins and Yearley 1992b, p. 386).

What is at stake here? While constructivists take to be unproblematic the import their historical case studies have on their social theories about how science works (that is, their failure to take reflexivity seriously), practice scholars tend to pay attention to narrative contexts and their cognitive significance. More significantly, the very notion of narrative context is what is in need of pinning down. Constructivists take narratives, or stories retrospectively told about the past, to be the fundamental “empirical” evidence supporting their assessments of the nature of science and technology. But what they apparently fail to realize is that their “story”—their text, their social theory of science, their professional work—is a story in which they themselves are embedded as the narrators (Rouse 1990). That is, they exist, as do scientists, within changing narrative contexts. Adopting this stance, realizing this embeddedness, is neither, as the constructivists contend, a slide into “self-destruction” (Collins and Yearley 1992a, p. 308), nor does it amount to “complete skepticism regarding the very matter of argumentation” (*ibid.*, p. 309). It is, however, a position that has ramifications for how stories about the past can and will be written.

More importantly, how these stories are told will have bearing on what our cultural understanding of science will be. Our cultural understanding of science is intertwined not only with the knowledge products of science, but is also intertwined with our educational practices and with how they maintain or shift that cultural understanding. The technological infrastructure of science, insofar as it is a research tool, will consciously and reflexively strive to maintain a view of the sciences and technologies as cultural practices that are embedded in historical, local, and *actual*, yet contingent, contexts. Strategies that seek to place the type of *a priori* metanarratives on the development of past knowledge, as do the constructivists with their reified social realm and the post-Kuhnian philosophers with their progress and rationality before them, should be rejected (cf. Woolgar 1992). Ultimately, it seems that the constructivists are no better off than were Lakatos and Laudan.

## Postmodernism and the Evolution/Creationism Debates

I now turn to the evolution/creationism debates of the twentieth century as an historical/historiographical case study in order to illustrate how postmodern historiography can make a difference in the stories historians create. I argue that the metanarrative of modernity has guided not only our cultural understanding of this contentious debate, but also has guided much of our historical understanding of it. Historiographically, metanarratives of modernity are found explicitly or implicitly in a wide selection of historical accounts of this ongoing controversy. While most contemporary historians are sensitive to cultural influences and resist polemical debate, when contentious cultural issues are at stake, historiographical standards sometime suffer. Furthermore, this debate exemplifies the significance of history and historiography, as it centers on education and what our culture deemed (and deems) to be of value to teach to our children. That is, it focuses on a major mode of the propagation and change of cultural norms—education.

The evolution/creationism controversy of the twentieth century can be divided roughly into two parts.<sup>42</sup> The first is the controversy over the teaching of evolution in public schools; this controversy was ignited by the 1925 Scopes trial in Dayton, Tennessee, in which the high school substitute biology teacher John Thomas Scopes was put on trial for teaching evolution.<sup>43</sup> The second began in 1961, when Henry M. Morris and John C.

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<sup>42</sup> In Chapter VI below, I briefly consider the Bush (George W.) era reincarnation of the debate over the teaching of creationism in public schools. In this twenty-first century version of the debate, the proponents of “Intelligent Design” repackaged Biblical creationism into a supposedly a-religious version in an attempt to bypass the U. S. Constitution’s provision for the separation of church and state. In the first test of the legality of teaching intelligent design, a federal judge in Harrisburg, Pennsylvania categorically rejected the defense’s position that intelligent design should or could be taught in public schools. Interestingly, Steve Fuller (identifying himself as a philosopher of science) was a witness for the defense; he claimed that “it will be a shame if a result of this decision is that we can’t question Darwinism, which is not just a theory but an entire secular world view that flattens the distinction between humans and other life” (Michael Powell, “Judge Rules Against ‘Intelligent Design’: Dover, Pa., District Can’t Teach Evolution Alternative,” *The Washington Post*, 21 December 2005, p. A1).

<sup>43</sup> On the Scopes Trial, see Edward J. Larson’s (1997) Pulitzer Prize-winning *Summer for the Gods: The Scopes Trial and America’s Continuing Debate over Science and Religion*, New York: Basic Books; John T. Scopes and James Presley (1967), *Center of the Storm: Memoirs of John T. Scopes*, New York: Holt, Rinehart, and Winston; and the trial transcript, *The World’s Most Famous Court Trial: Tennessee Evolution Case*, Cincinnati: National Books Company, 1925. Larson’s (1989) previous book on the Scopes Trial and scientific creationism I criticize below. His recent (Larson 2004) overview of the theory of evolution is a more even-handed, learned discussion of the history of evolution and its connections to wider cultural debates.

Whitcomb, Jr. published *The Genesis Flood: The Biblical Record and Its Scientific Implications*, and, to some extent, still continues today (cf. Numbers 1992, Numbers 1986, pp. 394-403, 407-15). Both debates presuppose the epistemic superiority of scientific methodology by the important players in *both* sides of the debates. More importantly, historical narratives on the subject have consistently taken this position as well.<sup>44</sup>

The emergence of organized opposition to the teaching of evolution (as the traditional story of creationism goes) was largely due to one person, three-time presidential candidate, William Jennings Bryan (1860-1925) (cf. Numbers 1986, pp. 394-8; Szasz 1982, pp. 107-16). Bryan, a respected public figure, became disillusioned over the negative effect the teaching of evolution seemed to be having, as he saw it, on the young people of America. He feared a generation of unbelievers was in the making. Bryan took it upon himself to galvanize support against the teaching of evolution, and he successfully assisted in the case in Tennessee against the high school substitute biology teacher, John Thomas Scopes. While the sensationalism of Scopes' trial and conviction initially resulted in bad press for Bryan's movement (he died a few days later), by the end of the 1920s over twenty states considered anti-evolution laws. Switching tactics, the creationists thereafter focused on putting pressure on local communities instead of state legislatures. As the story goes, before long, evolution all but disappeared from high school biology textbooks (Numbers 1986, pp. 394-402; Grabiner and Miller 1974, pp. 832-7).

From our standpoint, what is interesting here historically is that the tactics of the creationists did *not* involve bashing science. Bryan and his followers did not claim that religious knowledge was on a par with scientific knowledge, and hence "scientific." More importantly, they did not argue that scientific knowledge was not as legitimate as scientists claimed it to be. They *did* claim, however, that evolution was *not* scientific (Numbers 1986, pp. 395-401).

While some recent historical scholarship has moved away from the traditional

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<sup>44</sup> For relevant discussions on "internal" and "external" history of science, and on the demarcation between "science" and "nonscience" (or "pseudoscience"), see Popper (1963), Brush (1974), Lakatos (1978a, 1978d), Shapin (1982, 1992), Pitt (1983), and Palmer (1993). Laudan (1983) and Dolby (1987) explicitly take up the evolution/creationism debates in this context.

historiographical stance of characterizing the evolution/creationism controversies as “warfare” or confrontation between science and religion,<sup>45</sup> there remains implicit in these analyses the presupposition that science (including evolution) is on privileged epistemic ground. In her book on the creation controversy, Dorothy Nelkin (1982) identifies the origin of the 1920s controversy as an attack on “modernism”<sup>46</sup> by fundamentalists threatened by social changes brought about by the industrial revolution (pp. 30-1). Evolution was one focus of the attack, if not the main one, since it seemed to challenge most directly fundamentalist belief. Therefore, it seems Nelkin is attempting to identify the social context within which the 1920s controversy arose. However, Nelkin makes two claims that one should question critically. First, she slips into talk that characterizes the controversy as a “revolt against science” (pp. 31-2). Second, she claims that the Northern response to the Scopes trial should be seen in its modernist context in which Northerners attempted to *reconcile* evolution, and science in general, to their faith. Nelkin therefore states:

Indeed among Northerners who had reconciled religion and evolution, the old assumption about the incompatibility of science and religion seemed almost absurd by the time the Scopes trial brought the issue to national prominence. In fact, the trial was not intended to raise this issue at all. Rather, it was provoked by the American Civil Liberties Union in order to show that Tennessee’s anti-evolution legislation violated the First Amendment. (pp. 30-1)

Who were these Northerners who had already reconciled evolution to their faith? Nelkin seems to come to this unsupported conclusion partly to account for the lack of horror on the part of these “Northerners” in responding to Scopes’ conviction.<sup>47</sup> Nelkin’s way out

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<sup>45</sup> See, for example, the analysis by Brooke (1991) and Moore’s (2002) excellent analysis from the standpoint of science education. For a critical analysis of the “warfare” historiography, exemplified by Draper (1875) and White (1896), see Moore (1979), esp. chs. 1-4.

<sup>46</sup> The term ‘modernism,’ as Nelkin uses it, should not be confused with ‘modernity,’ as I use it in this dissertation. Modernism is, among its many definitions, a cultural attitude, an artistic and literary movement, and perhaps, a metanarrative that emerged roughly at the turn of the twentieth century. What these all have in common is a questioning of some of the ideals and cultural norms of the nineteenth century. I go into more detail on modernism later in this chapter.

<sup>47</sup> Nelkin (1982) provides little documentation for her conclusions here, although she may be empirically correct in her analysis of Northerners’ reconciliation of evolution; my criticism hinges on empirical support as opposed to rational assumption. Her historiographical stance places her firmly within the modern, internalist perspective, according to which science proceeds relatively autonomously by virtue of its epistemically superior grounds. Her motivation to defend evolution and to attack the teaching of creationism is likely a reaction, at least in part, to the polemics caused by the Reagan-era debates over the teaching of



of this modern, internalist quandary was to suppose, without empirical documentation, that it was not a problem for the Northerners since they had long incorporated evolution into their belief system. Clearly, Nelkin deploys this historiographical escape hatch to rescue some semblance of rationality from this historical episode. For the internalist historiographer, it would be virtually a logical necessity for those who accepted evolution to react in horror when someone is convicted of a crime for teaching it. Here we have, the internalists would say, an obvious case of the influence of infiltrating “external” factors on the rational course of scientific thought. The proponents of science must act accordingly. To suggest otherwise would be tantamount to rejecting the epistemic sovereignty of science—and internalist historiography.

One would hope that Nelkin’s interpretation would be supported by at least some compelling historical evidence, and it may be that she is partly right. However, she offers no historical evidence to support this contention. Furthermore, she is not the only one to account for the 1920s controversy in this manner. In his recent account of twentieth century creationism, Ronald L. Numbers (1986) makes a similar assumption.<sup>48</sup> Numbers assures us that at the time of the 1925 Scopes trial, evolution was firmly established in high school biology textbooks (p. 403; see also Grabiner and Miller 1974, pp. 832-7; and Larson 1985, chs. 1-3, esp. pp. 15-27 for his attempt to trace the role of evolutionary theory in high school biology textbooks). Such an assumption fits in well with the internalist line only if the creationists were endowed with great powers to affect changes in the content of textbooks, and if evolution was already established in the rational north. This interpretation fosters the view that the “irrational” creationists were battling “rational” science as an established institution.

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evolution in schools. She is clearly on the pro-science side of the debate. That she buys the internalist historiographical perspective, see, for example, her comments in ch. 12, esp. pp. 188-189. She even goes as far as to say: “Historically and methodologically, much of science developed in opposition to the dogmatism of religion. . .” (p. 189).

<sup>48</sup> I use the chapter by Ronald Numbers (1986) as a guide for this section. His chapter was originally prepared for the Carner Foundation/University of Wisconsin Conference on “Christianity and Science,” held from 23 to 25 April 1981. This places the conference squarely within the midst of the Reagan era debates over the teaching of evolution and creationism in schools; Reagan made teaching creationism a plank in his 1980 presidential campaign. A shorter version of Numbers’ chapter appeared in 1982 as “Creationism in 20th Century America,” *Science* 218: 538-544. Numbers’ (1992) book on scientific creationism provides a wealth of historical detail on the various episodes.

Subsequent historical scholarship has shown that several of the above assumptions are misleading at best. In particular, consider the work of Philip Pauly (1991), who notes that historians have generally assumed, but not shown, that by 1925 evolution was firmly established in high school biology curricula (pp. 663-4, 685-8). It is constructive to consider, briefly, what have been the consequences of the internalist metanarrative for the evolution/creationism controversy of the 1920s. As we have seen, errors in historical judgment have arisen from the attempt to force diverse cultural contexts into one historiographical scheme, a scheme that imposes a pernicious context-transcendent principle on past events. The source of the errors is not simply incomplete historical scholarship (for example, the historian did not look at enough sources or at primary sources); it is also the presupposition of rationality in the course of history. The modern, internalist metanarrative removes the central historical interpretation from its cultural context and plants it in the world of disembodied knowledge (i.e., to be rational it *must have happened* this way). This may explain why Nelkin presupposed Northerners had reconciled evolution to their faith and why Numbers presupposed evolution was in 1925 a long-established component of high school biology textbooks.

### I. *Scientific Creationism*

A plethora of historical (and philosophical) accounts has appeared on the resurgence in the 1970s and 1980s of the legal battles over the teaching of evolution in public schools. These efforts trace some aspect of the story that originates roughly with Henry M. Morris' and John C. Whitcomb's publication of *The Genesis Flood* in 1961, to Morris' (1918-2006) role in forming the Creation Research Society in 1963 and then founding the Institute for Creation Research in 1970, and then to the legal and legislative battles over the teaching of evolution over the course of the next two decades.<sup>49</sup> Most if not all of these accounts fit

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<sup>49</sup> Henry Morris (1918-2006), who earned a Ph.D. in hydraulic engineering, joined the Virginia Tech faculty in 1957 and served as Head of the Civil Engineering Department from 1959 to 1970. In his obituary in *The New York Times*, Morris was hailed as the "father of modern creationism." His 1961 book *The Genesis Flood*, written with the theologian John C. Whitcomb, has sold over 250,000 copies in English and is in its forty-fourth printing; it is considered the "handbook of creationism." Morris' Institute for Creation Research, located in Santee, California, grants Masters degrees and is considered to be the leading research center on

squarely into the camp of internalist historiography.<sup>50</sup> Indeed, in one of these court cases in Arkansas in 1981, the historian and philosopher of science Michael Ruse and the sociologist of science Dorothy Nelkin were used by the American Civil Liberties Union as expert witnesses in arguing against giving creationism equal time in the classroom (Ruse 1984b, pp. 311-42). One may ask, given the obvious sympathies of these two scholars, what are we to make of their historiographical stances? Ought we to trust narratives written by scholars who have such an obvious personal stake in one particular side of the debate? Should historiography be a function of taking one side or the other?

The emergence of *scientific* creationism in the 1970s can be distinguished from the 1920s controversy in that the creationists' strategy changed. Instead of arguing that evolutionary theory is not scientific, they began to follow the lead of Henry Morris and argue that creationism *is* scientific, and therefore a legitimate scientific theory to be taught alongside evolution in public schools (cf. Numbers 1986, pp. 411-12). This tactic, and particularly the 1981 legislative decisions in Arkansas and Louisiana to sanction the teaching of creationism alongside evolution, put scientists on the defensive (and then offensive) in upholding evolutionary theory and discrediting creationism as a science. Around this time, a number of accounts by scientists appeared, aimed at convincing the public of their stand on

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scientific creationism. Morris died on 25 February 2006 at age 87 (Jodi Rudoren, "Henry M. Morris, 87, a Theorist of Creationism, Dies," *The New York Times*, 4 March 2006, p. A13; quotations from *ibid*).

<sup>50</sup> See, for example, Grabiner and Miller (1974); Philip Kitcher (1982), *Abusing Science: The Case Against Creationism*, Cambridge: The MIT Press; Nelkin (1982); Norman D. Newell (1982), *Creation and Evolution: Myth or Reality?* New York: Columbia University Press; Michael Ruse (1982), *Darwinism Defended: A Guide to the Evolution Controversies*, London: Addison-Wesley; Ronald Mushat Frye (1983), *Is God a Creationist? The Religious Case Against Creation-Science*, New York: Charles Scribner's Sons; Marcel C. LaFollette (1983), *Creationism, Science, and the Law: The Arkansas Case*, Cambridge: The MIT Press; J. Peter Zetterberg, ed. (1983), *Evolution Versus Creationism: The Public Education Controversy*, Phoenix: Oryx Press; David B. Wilson, ed. (1983), *Did the Devil Make Darwin Do It? Modern Perspectives on the Creation-Evolution Controversy*, Ames: Iowa State University Press; Ashley Montagu, ed. (1984), *Science and Creationism*, Oxford: Oxford University Press; Numbers (1986); Janelle Rohr, ed. (1988), *Science and Religion: Opposing Viewpoints*, St. Paul, MN: Greenhaven Press; Michael Ruse (1988), *The Darwinian Paradigm: Essays on its History, Philosophy, and Religious Implications*, New York: Routledge; and Tim M. Berra (1990), *Evolution and the Myth of Creationism: A Basic Guide to the Facts in the Evolution Debate*, Stanford: Stanford University Press. In his laudatory review of Kitcher's (1982) book, Ruse (1984a) characterizes scientific creationism as follows: "[W]hat a hollow sham modern day Creationism really is: crude, dogmatic, Biblical literalism, masquerading as genuine science, in order to avoid the strictures of the first amendment to the U. S. Constitution, which bars the teaching of religion in schools" (p. 348).

the issue.<sup>51</sup> While it would probably be unfair to expect unbiased historical scholarship in these accounts, their rhetorical effect on public opinion is potentially as great (or even greater) than accounts written by professional historians. Moreover, the tactics used are not much different from those used by some historians and philosophers of science. What is significant, however, is that the rhetoric used fails to meet the standards of contemporary philosophy of science. This is not to suggest that philosophy of science should have the last word on the nature of science. We should expect, though, that historically discredited metatheories not be resurrected in the defense of an untenable view of science.

For example, in his testimony in the 1981 Arkansas case, the historian and philosopher of science Michael Ruse (1984b) invoked Popperian falsification as a tenet of scientific methodology that evolution met while creationism failed (pp. 311-42). In the decision in favor of the plaintiffs, Judge William R. Overton cited falsificationism as one of the “essential characteristics of science” (quoted in Wilson 1983, p. 213; for the entire decision, see Overton 1982). These characteristics, derived from decades-old philosophy of science, were enumerated as follows:

- (1) It is guided by natural law;
- (2) It has to be explanatory by reference to natural law;
- (3) It is testable against the empirical world;
- (4) Its conclusions are tentative, i.e., are not necessarily the final word; and
- (5) It is falsifiable. (quoted in Wilson 1983, p. 213)

One point the expert witnesses drove home in their testimony was that the creationists used scientific evidence and statements by scientists selectively in order to support the scientific status of the theory of scientific creationism, or to weaken the status of evolutionary theory (Ruse 1984b, pp. 331-6). Michael Ruse recalled that he “wanted to prove not merely that Creation-science is not science, but that it is a dishonest and thoroughly corrupt enterprise,

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<sup>51</sup> See, for example, Niles Eldredge (1982), *The Monkey Business: A Scientist Looks at Creationism*, New York: Washington Square Press; Roger Lewin (1982), *Thread of Life: The Smithsonian Looks at Evolution*, Washington, D.C.: Smithsonian Books, New York: W. W. Norton; Douglas J. Futuyma (1983), *Science on Trial: The Case for Evolution*, New York: Pantheon Books; Laurie R. Godfrey, ed. (1983), *Scientists Confront Creationism*, New York: W. W. Norton; Christopher McGowan (1984), *In the Beginning—A Scientist Shows Why Creationists are Wrong*, Buffalo, NY: Prometheus Books; National Academy of Sciences (1984), *Science and Creationism: A View from the National Academy of Sciences*, Washington, D.C.: National Academy Press; and Kenneth R. Walker, ed. (1984), *The Evolution-Creation Controversy: Perspectives on Religion, Philosophy, Science and Education*, The Paleontological Society, Special Publication No. 1.

violating every standard of intellectual integrity” (p. 331). Indeed, Ruse claims that he “was able to show that statements made by eminent evolutionists were lifted by creation-scientists and quoted out of context. Evolutionists [were] made to say the very opposite of what they intended” (pp. 331-2).<sup>52</sup> Judge Overton agreed (Wilson 1983, pp. 214-8).

While Ruse is clearly correct in his assessment of the tactics of the scientific creationists, what are we to make of *his* tactics and those of his fellow expert witnesses? Does their use of an outdated and philosophically suspect account of scientific change amount to intellectual honesty?

The tactics of the Arkansas case plaintiffs were repeated in other historical, philosophical, and scientific accounts of this controversy. In a publication by the Paleontological Society (Schwimmer 1984), the following criteria are enumerated which scientists must follow in order for their activity to be deemed scientific: Presentation of Multiple Hypotheses; Objectivity; Positivism; Falsifiability; Occam’s Razor; and Logical Validity (p. 7). In contrast, the “logical fallacies” committed by the creationists, which make their work unscientific, are: factual error; complex questions and spurious correlations; false assumptions; and anachronisms (pp. 7-8). Note the use of lower case letters for these “fallacies,” while the supposed tenets of the “scientific method” are capitalized.

Concerning these “fallacies,” we have seen each one of them committed (not necessarily intentionally) by professional scholars in evaluating the evolution/creationism debate historically, philosophically, or sociologically. I argue that a major cause of these problems is internalist historiography, a component of the metanarrative of modernity. This metatheory of science, which guides narratives with a transcendental principle of rationality (embodied in such notions as *the* scientific method, scientific progress, or social explanation), must be rejected as a methodology for historians, philosophers, sociologists, and all those who study science. However, if internalism is to be rejected, with what do we

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<sup>52</sup> This charge is undoubtedly largely accurate. A distributed publication by the Jehovah’s Witnesses, which purports to be a “thoroughly researched examination of how life got here—and what this means for the future,” repeatedly takes quotations out of context and misrepresents the scientific evidence. See *Life—How Did it Get Here? By Evolution or by Creation?* (1985), Brooklyn, NY: Watchtower Bible and tract Society of New York, Inc. and International Bible Students Association, quotation from inside title page. For an analysis of the defects of the evidence of scientific creationism, see Robert T. Pennock (1999), *Tower of Babel: The Evidence against the New Creationism*, Cambridge: The MIT Press.

replace it? My answer is postmodern historiography.

## II. *Revisiting the Evolution/Creationism Debates*

It is instructive to outline briefly how historians have pursued and might continue to pursue a postmodern historiography in order to reevaluate the evolution/creationism debates. For the 1925 Scopes trial and debate, the historian Philip Pauly (1991) has investigated the origins of high school biology. Contrary to the assumptions of the internalists, Pauly shows that evolution was not an established component of biology teaching before 1925.

Furthermore, it was not even on the New York State biology syllabus—and New York City educators had a major influence on national high school biology curricula (pp. 685-6).

According to Pauly, “[t]his seeming inconsistency becomes comprehensible when one sees evolution as an issue comparable to field study, vivisection, or sex,” sticky issues for which educators thought the teaching of biology could provide a forum to better inform high school students of life lessons (p. 686). As for the “content” of contemporary evolutionary theory,

instruction on evolutionary mechanics seemed impossible in the 1910s and 1920s given the degree of professional uncertainty about the mutation, selection, and inheritance of acquired characters. What was important was to convey an evolutionary perspective, but this task was largely independent of specific instruction about species change. (p. 687)

It should be no surprise, then, that no furor erupted among biology teachers because of the conviction of Scopes. Pauly (1991) states that “New York educators periodically and routinely noted that teaching evolution could disturb the classroom or the community, and they saw little value in antagonizing potentially vocal elements of the public.” What was more important to the educators was their view of the kind of person they aimed, through biology teaching, to produce—liberal, secular, and humanist (p. 686). Given that evolution played a minor role in this effort, and given that they were successful in their efforts, the reception of Scopes’ conviction is no longer an historical mystery. The power of the creationists in changing course content is also put into proper perspective:

Biology educators were more cunning than timid, maneuvering within limits set by their perception of the state of scientific knowledge, their experience in urban classrooms, and their assessment of individual and collective power. They prevailed in the struggle that mattered to them most. High school biology became part of

public education throughout the country, and its fundamental themes and images became part of middle-class culture. Their view of life was coherently masculine, urban, and liberal. (p. 687)

Pauly's (1991) historiography clearly rejects the assumptions of modernity. Internalism is abandoned, and the historical mysteries generated by Nelkin (1982) and Numbers (1986) in their rationalist accounts, become comprehensible from the postmodernist standpoint. By removing the metanarrative of modernity and rejecting epistemic sovereignty as a presumption for historical analysis, a much different account of the creationism debates results. It is not necessary to presume that Northerners had assimilated evolution into their religious belief systems, nor is it necessary to assume that evolution was established in Northern textbooks. While he did not discuss Lyotard or postmodernism, Pauly (1991) adopted a postmodern stance, one that is "incredulous toward metanarratives," and resisted deploying a rationalist or internalist strategy in order to tell the story of creationism. One could conclude that the interpretation (History-3) fits the evidence (History-1) better, as the story Pauly tells seems more coherent, involves more relevant evidence, and seems to be told from a more "detached" standpoint, one that aims for historical truth. However, I tentatively resist this interpretation and conclude that the story is *different*. It is different because its History-4 is different, and I suspect, so is Pauly's History-5. In the next chapter, I explore whether it can be argued that this sort of interpretation is "better" in some alternative, or perhaps, deeper sense.

As for the 1970s and 1980s version of the creationism debate, the implications of postmodernist historiography should be clear. While no adequate historical account, to my knowledge, has been written, we can point to some guidelines that should be followed. First, the metanarrative of each side should be made clear. As the evolutionists point to the intellectual dishonesty of the scientific creationists, so too must the postmodern historian bring up front the scientific rationality and internalism of the evolutionists and "pull the carpet out from under the feet of science and modern[ity]" (Ankersmit 1989, p. 142). Again, this does not amount to accepting creationism as truth, nor does it entail bashing science as a cultural practice. It means, in this case, recognizing that education (and of course, religion) is a key underlying issue. Just as the creationists worried about the effects the teaching of

evolution could have on future generations of children, so too were the evolutionists worried about the effects of teaching creationism. Each side wanted to propagate its own (partly religious) worldview. The evolutionists mustered whatever arguments they could to defeat the creationists, even if this involved historically and philosophically questionable, if not discredited, philosophical dogma. This story is waiting to be written. Again, it should not be an anti-science story. It should be, in the spirit of Pauly, an analysis that remains reflectively and reflexively engaged in how the levels of history are related. As Ankersmit<sup>53</sup> rightly states:

Postmodernism does not reject scientific historiography, but only draws our attention to the modernists' vicious circle which would have us believe that nothing exists outside it. However, outside it is the whole domain of historical purpose and meaning. (p. 153)

### **Martin Harwit and the *Enola Gay* Exhibit**

The polemical debate over the planned 1995 *Enola Gay* exhibit at the Smithsonian Institution's National Air and Space Museum provides another example of how adopting a postmodern historiography can result in the interpretation of an historical episode that differs markedly from an interpretation dependent on a modernist metanarrative. In particular, this episode exemplifies a crucial problem for the historian of recent history: that participants' histories (or oral histories) are not always the most reliable or balanced interpretations of past

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<sup>53</sup> Ankersmit's (1989) article on postmodern historiography was sharply criticized by Zagorin (1990), who remarked: "What stands out in Ankersmit's postmodernist concept of historiography is its superficiality and remoteness from historical practice and the way historians usually think about their work. It trivializes history and renders it void of any intellectual responsibility" (p. 266). Ankersmit (1990) defended his article, maintaining that "at the level of the historical text and of historical interpretation, we cannot appropriately use the words truth and falsity. For we can say a lot of things about proposals, for example, that they are fruitful, well-considered, intelligent, to the point (or not), and so on, but not that they are true or false. . . . [T]he fact that proposals cannot be either true or false does not imply that no good reasons can be given for or against a certain proposal. The mere fact that we cannot label narrative interpretations or narrative substances as either true or false does not in the least leave us empty-handed in historical debate. It is a fallacy as silly as it is dangerous to believe that we can or ought to restrict historical interpretation and historical argument to what can truthfully be said about the past on the basis of available evidence" (p. 282). One component of my argument in this dissertation is that Ankersmit's view of history and historiography is *more* intellectually responsible than what Zagorin advocates.



events, in part because the participants often have a personal, political, and/or moral stake in interpreting the episode one way rather than another. However, postmodern historiography is suspicious of any position that presupposes a simple enumeration of “reliable” or “balanced” facts can be given that is in some sense objective, or that all participants can agree upon. Indeed, this ambition is misguided, as were many of the critics—including veterans and members of Congress—of the Air and Space Museum’s then director, Martin Harwit. Harwit wanted to present an exhibit on the *Enola Gay*—the plane that dropped the atomic bomb on Hiroshima—that illustrated the many sides of the contentious debates surrounding the dropping of the atomic bombs in 1945 on Hiroshima and Nagasaki. Harwit’s critics, however, wanted a pro-American exhibit that declared the necessity of the atomic bombings in ending World War II and saving millions of American and Japanese lives by preventing an American invasion of Japan (cf. Zolberg 1996, Harwit 1996). Ultimately, the problem inherent in this debate centers on our inability to access directly historical reality, and the inevitability of distortions and hindsight entering into any assessment of past events—in short, it rests on how to handle the philosophy of history.

Critics of Harwit, including Air Force veterans, members of Congress, and the media, and *The Washington Post* editors,<sup>54</sup> accused him of various historical and historiographical evils, including not presenting “the full story,” engaging in bad “historical revisionism,” and in believing that his historiographical constraints were “universal, ‘objective’ assumptions that all thinking people must share.” At stake was the ideologically charged decision that had to be made concerning what historical interpretation to give the atomic bombings of Hiroshima and Nagasaki in the *Enola Gay* exhibit. The choices were characterized by some in stark black and white terms: either present an exhibit that proclaims, “Thank God for the atomic bomb,” or present one that fosters the view that “Anti-Asian racism, long a factor in American life,” was behind the atomic bombings.<sup>55</sup>

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<sup>54</sup> “The Mission That Ended the War,” *The Washington Post*, 14 August 1994, p. C9; “Context and the Enola Gay,” *The Washington Post*, 14 August 1994, p. C8.

<sup>55</sup> Harwit (1996) himself provides a restrained account of the *Enola Gay* fiasco, and shows how pressure from Congress (including threats to cut federal funding to the Air and Space Museum), the media, the Air Force Association (AFA), and other veterans’ groups, caused the implosion of the exhibit, an exhibit and interpretation that was supported by many academic historians and other scholars, and by a resolution passed by

What the critics of Harwit shared in terms of their historiographical stances were adherence to the transcendental notion that a definitive history can be written.<sup>56</sup> This position is characteristic of the perspective of modernity, guided by its metanarrative that instructs historical interpretation. This metanarrative amounts to the view that we can have direct access to reality (including historical reality), unfiltered by gaps in crucial evidence and unencumbered by cultural, political, or ideological trappings. It is this modern perspective to which Harwit refers when he characterizes his opposing position as “more analytical, critical in its acceptance of facts and concerned with historical context.”<sup>57</sup> Yes, the museum should endeavor to tell “the full story” of the bombings, and it should not be simply an “opinion piece.” It should aim at the presentation of an exhibit that offers “the basic information that visitors will need to draw their own conclusions.” Yet all should recognize that unless the exhibit presents only uncaptioned pictures and artifacts with perhaps names and dates, the exhibit—like all historical narratives that attempt to tell a story and to teach—will be an *interpretation* of past events.

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the Organization of American Historians (Lifton and Mitchell 1995, p. 291). Harwit (1996) defended the exhibit to the end and offered this conclusion: “In one way or another, each of the [exhibit’s] opponents was attacking the basic charter of the Smithsonian Institution. Once a Congressman Johnson tells the National Air and Space Museum that it has no business teaching history, or orders one of its exhibitions to be shut down, or bans the publication of this exhibition’s catalogue, it becomes difficult to see where his concern for patriotism and national self-image will stop. It becomes a dangerous game.” (p. 429) Other works that offer similar assessments of the *Enola Gay* affair include Lifton and Mitchell (1995), pp. 276-97; Nobile (1995), which includes a chapter by Barton Bernstein; and Bird and Lifschultz (1998), part IV, which includes chapters by Mike Wallace, John Dower, Martin Sherwin, Stanley Goldberg, Tony Capaccio, and Uday Mohan. It also includes reprints of many (but not all) of the editorials and opinion pieces written on the episode, including those by John Dower, Martin Harwit, Kai Bird, Charles Krauthammer, Gar Alperovitz, Stanley Goldberg, *The Washington Post*, and *The New York Times*. The editorial board of *The New York Times* was the only board of a major newspaper that supported Harwit and resisted efforts to stifle the historical truth (contentious, contested, and complex) of the episode, regardless of pressure from veterans and veterans’ groups, the Republican-dominated mid-term House of Representatives elected in 1994, or other political or interest groups.

<sup>56</sup> See, for example, O’Reilly and Rooney (2005). These authors, who are not historians, buy into the “historical revisionism” argument and believe that the historians of the Air and Space Museum “ignored evidence and presented speculation as fact” (p. 167). Indeed, the authors claim they were “interested in documenting where the [Air and Space Museum’s] polemic fell afoul of *history as it is*, not as how the curators sought to corrupt it” (p. 168, emphasis added). Believing they have access to “indisputable facts about what happened as the war was ending in the summer of 1945” (p. 2), O’Reilly and Rooney “see the controversy as one in which revisionist ideology was going to trump the factual record and ought to be challenged” (*ibid.*). Astonishingly, they make the following claim: “[J]ust as war can be too important to be left to the generals, history sometimes is too important to be left to historians” (*ibid.*).

<sup>57</sup> Martin Harwit, “The Enola Gay: A Nation’s, and a Museum’s Dilemma,” *The Washington Post*, 7 August 1994, p. C9.

The controversy over the *Enola Gay* exhibit now reduces, in part, to a debate over historiography. The choice is between the metanarrative of modernity—with its transcendental link to “actual” and “definitive” accounts of the past—on the one hand, and the postmodernist’s rejection of the “truth” of the totalizing metanarrative, on the other hand. The latter view is the more coherent historiography, for reasons that amount more to common sense than to the alleged “intellectual sophistication” of elitist academics (cf. Forman 1991). Because our knowledge of the past—whether it be in the form of written evidence, films, artifacts, recollections, or the thought processes in President Truman’s head—is inevitably incomplete, historians must eschew any hope of writing definitive histories. History is written anew each generation, and there are practical, real-world issues and actions *at stake* in interpreting the past one way or another.<sup>58</sup>

The historiographical lesson in this case is that many of the critics of Harwit defeated their own purposes of having their viewpoints included in the *Enola Gay* exhibit by adhering to historiographical transcendence. By clinging to an objectivist metanarrative and arguing that their interpretation guarantees historical “truth,” once and for all, these critics ironically betrayed the very contentiousness of historical interpretation that they took great pains to point out when criticizing Harwit. The major historical questions generated by the exhibit were those that were most subject to tendentiousness—for example, whether the atomic bomb was necessary to end the war with Japan, whether an invasion would have been necessary without it, whether Truman dropped the bomb to impress Stalin, whether it was morally wrong and inhumane to use atomic weapons when conventional weapons might have forced Japan’s surrender, and so forth.<sup>59</sup>

Hence, it was unfair of critics (including the *Post* editors) to suggest that Harwit was unable to “perceive that political opinions are embedded in the exhibit. . . .” There could be

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<sup>58</sup> C. Vann Woodward (1908-1999), historian of the American South who was Sterling Professor of History at Yale University, wrote in defense of both the duty and importance of history as a discipline that ought to engage the issues that are at stake in the cultural milieu of the historian, *and* as a discipline that ought to “reinterpret” and revise history in the light of what is at stake in the present, with an eye toward influencing the future. See *The Future of the Past* (1989), New York: Oxford University Press, esp. ch. 1 (“The Future of the Past”), from his presidential address to the American Historical Association in 1969, and ch. 4 (“The Age of Reinterpretation”), from 1960; both published in the *American Historical Review*.

<sup>59</sup> Howard Zinn (1997) provides a first-person historical account of the struggle for the historical interpretation that suggests that the atomic bombings of Hiroshima and Nagasaki were wrong .

no exhibit without them; as a professional historian, Harwit was clearly aware of this. To suggest, as did one veteran, that the Smithsonian's National Air and Space Museum "was not established to be a center for political, philosophical, sociological or ethnic discourse," is tantamount to claiming that a definitive and objective (in the transcendental sense) presentation is possible, sanitized and free of interpretation. The problem with this view is that the exhibit that resulted was shorn of all aspects of its historical context—something the critics of Harwit claimed they wanted *more* of, or a better version of. The result was a B-29 aircraft and other artifacts and pictures placed in a room, with little or no accompanying contextual information to help *interpret* the past. Clearly, this was not *history*, as contentious, contested, and controversial as history can be. It was not a "factual and honest explanation" of the *Enola Gay* and the atomic bombings of Hiroshima and Nagasaki.<sup>60</sup>

In addition, the exhibit and the processes that led to its creation (and subsequent destruction) propagate and reinforce the metanarrative of modernity. The costs of this cultural phenomenon can be high. If contextual, evidence-based interpretation is how we want future generations to learn about history, the *Enola Gay* exhibit did not accomplish this. If sanitized, internalist, and propagandistic history (perhaps one could use the term "politically correct") is what we want to teach our children; if reified cultural myths are the goal, then the exhibit succeeded. As the historian Kai Bird noted:

Nothing is more debilitating to our national discourse than the notion that history is a known commodity, frozen in time, to be handed untouched from generation to generation. History is a living thing, and it is the wholly legitimate task of historians to be constantly modifying and rewriting it based on new evidence, obtained in large measure from the archives.

Unfortunately, too many Americans want their history simple and unadorned by archival evidence, served up by court historians and written with one goal in mind:

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<sup>60</sup> In January 1995, the Smithsonian's National Air and Space Museum canceled the *Enola Gay* exhibit, and in early May its Director, Martin Harwit, resigned. The fuselage of the *Enola Gay* was scheduled to go on exhibit that summer, but without any significant interpretation (cf. Eugene L. Meyer, "Smithsonian Sifts Debris of Enola Gay Plan," *The Washington Post*, 20 April 1995, p. D1). The editors of the *Post* concluded the following: "Exhibits are not the place to dictate the appropriate or acceptable view of a contested historical episode" ("Smithsonian: After the Shouting," *The Washington Post*, 7 May 1995, p. C6). I visited the Air and Space Museum's *Enola Gay* exhibit that summer, and I saw an "exhibit" stripped of any competent historical interpretation. This kind of exhibit fosters the notion that national museums ought to be forums for patriotic propaganda, rather than for serious historical interpretation and public education. One lesson from this chapter is that this view is dangerous and irresponsible, as the victim is historical "truth," however we may want to define it.

the propagation of patriotically correct mythology.

This is particularly true of those defenders of the conventional wisdom on Truman's decision to use the atomic bomb without warning on the largely civilian population of Hiroshima.

I could refute each of the points made by [those defenders] with archival documents, diary quotes and statements from Truman and his closest military advisers.

But that would be tedious; instead I refer readers to a large body of scholarly articles and books written by Barton J. Bernstein, Martin J. Sherwin, Gar Alperovitz, . . . Stanley Goldberg, Gregg Herken, Herbert Feis, McGeorge Bundy, [and so on].<sup>61</sup> One cannot read these historians without concluding that the decision-making behind the Hiroshima bombing is a still-unsolved—and fascinating—mystery story.<sup>62</sup>

### **Historiography and Philosophy of History**

The discussion of postmodern historiography in this chapter points to some philosophical issues that lurk beneath any developed discussion of history, historiography, and narrativity. Some of these issues I have considered briefly in this and the last chapter; these issues are *the real*; *time* or *temporality*; and *causality*. While I discuss these issues in more depth in the next chapter, I raise them here to push farther where the postmodern worldview might take them. To understand the technological infrastructure construct as a tool for historical, philosophical, and/or social/cultural research and understanding, it is necessary to address these foundational issues. Indeed, how one addresses these issues has bearing on how research, analysis, and ultimately, cultural understanding, will proceed.<sup>63</sup>

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<sup>61</sup> See, for example: Barton Bernstein (1976), *The Atomic Bomb: The Critical Issues*, Boston: Little, Brown; Martin J. Sherwin (1975), *A World Destroyed: The Atomic Bomb and the Grand Alliance*, New York: Knopf; Gar Alperovitz (1995), *The Decision to Use the Atomic Bomb and the Architecture of an American Myth*, New York: A. A. Knopf; Gregg Herken (1980), *The Winning Weapon: The Atomic Bomb in the Cold War, 1945-1950*, New York: Knopf; Herbert Feis (1961), *Japan Subdued: The Atomic Bomb and the End of the War in the Pacific*, Princeton: Princeton University Press; McGeorge Bundy (1988); Robert Jay Lifton and Greg Mitchell (1995); and Kai Bird and Lawrence Lifschultz (1998).

<sup>62</sup> Kai Bird, *The Washington Post*, Book World, 12 February 1995, p. 14.

<sup>63</sup> Currie (1998) provides an excellent guide to what is at stake professionally and intellectually in adhering to postmodern narrative theory. Olábarri (1995) gives an excellent overview of historiographies from the 1920s to the 1970s that claimed to be “new,” including the *Annales* school, Marxist historiography, American social historians, and the “Bielefeld school,” or *Gesellschaftsgeschichte* (see also Rösen 2005, ch. 6); he compares these developments to subsequent developments in “postmodern historiography” in the 1970s, 1980s and beyond, including works by Frank Ankersmit, Paul Ricoeur, Richard Rorty, and Jörn Rösen. Novick

In the discussions of narrativity and its significance for research in the history of science and related fields, I indicated in Chapter I that I agreed with Rouse (1990) that we should not look at a narrative as a completed story, with a beginning, middle, and end. Rather, we should consider a narrative to be a context, perhaps one of many, in which we *live* and take action that has meaning. We may speak of a “narrative field” in which we belong; any action we take will have meaning because of our participation in that field of potentially intersecting narratives. In any event, we desire that the narrative context be an *ongoing* process. Just because the stories we tell must come to an end—this should not mean that the narrative context disappears. However, if narrativity is to have the cognitive significance historians believe it does; if “narratives” as “stories” are indispensable constructs used in history and in philosophy in order to construct arguments that are compelling; and if it makes no sense to consider oneself “outside” of a narrative context or narrative field—then it seems “the narrative” should itself be deconstructed.<sup>64</sup>

In the writing of fictional novels, the traditional view of the narrative is that it is an indispensable form for the telling of a meaningful story (e.g., White 1981a, b, 1987). A

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(1988) provides an extended study of historiography in the American historical profession, including developments leading up to a postmodern version; he credits Hayden White as the main proponent (see ch. 16, esp. pp. 599-607).

<sup>64</sup> According to strategists involved in the 2004 presidential campaign in the United States, “narrative” was a key political issue that had ramifications for whether a candidate could properly deliver his/her message to the people and/or have that message heard. One Democratic pollster said, “A *narrative* is the key to everything” (William Safire, “Narrative: The New Story of Story,” *The New York Times Magazine*, 5 December 2004, p. 34, quotation from *ibid.*). The Democratic strategist James Carville said, “They [the Republicans] produce a *narrative*, we produce a litany.” That is, continued Carville, “They say, ‘I’m going to protect you from the terrorists in Tehran and the homos in Hollywood.’ We say, ‘We’re for clean air, better schools, more health care.’ And so there’s a Republican *narrative*, a story, and there’s a Democratic litany” (quoted in *ibid.*). The view that narrative and a coherent view of one’s past are important is echoed by Peter Beinart of *The New Republic*; he argues that “Democrats have no shortage of talented foreign-policy practitioners. Indeed, they have no shortage of worthwhile foreign-policy proposals. Even so, they cannot tell a coherent story about the post-9/11 world. And they cannot do so, in large part, because *they have not found their usable past*. Such stories, after all, are not born in focus groups; they are less invented than inherited. Before Democrats can conquer their ideological weakness, they must first conquer their ideological amnesia” (Peter Beinart, “The Rehabilitation of the Cold-War Liberal,” *The New York Times Magazine*, 30 April 2006, pp. 40-45; quotation from p. 41, emphasis added; see also Beinart’s (2006) *The Good Fight: Why Liberals—and Only Liberals—Can Win the War on Terror and Make America Great Again*, New York: HarperCollins). The distinction here is between a History-3 and a History-2; the Republicans offered a story with connections to the past, the Democrats a list or a chronology of events to be completed without a connected story. This suggests that narrativity is not as far removed from everyday life as it might seem at first glance. In this dissertation, I argue that narrativity is crucial to understanding the significance of the sciences and technologies, and that this significance can be extended to politics, policy, and other realms of culture.

literary narrative, then, is a story with a beginning, middle, and an end. An *end* to the story is necessary, as Kermode (1967) argues, because we humans “need in the moment of existence to belong, to be related to a beginning and to an end” (p. 4). For us humans, we seem to find ourselves always *in medias res*, or as Kermode put it, “in the midst.” In order to make sense of our lives, to give them meaning, we tell stories in which we “project ourselves . . . past the End, so as to see the structure [as a] whole, a thing we cannot do from our spot of time in the middle” (p. 8). In other words, the novelist will impose on a series of events a plot, which is, according to White (1981a), “a structure of relationships by which the events in the account are endowed with a meaning by being identified as parts of an integrated whole” (p. 9). Furthermore, the meaning that the plot of a narrative imposes on the events of the story is generated “by revealing at the end a structure that was immanent in the events *all along*” (p. 19). Closure, a coherent summing-up, an organized whole with moralistic meaning, a story with an *end* that was immanent in its plot structure from the *beginning*—these are some of the main characteristics of a literary narrative.

Literary narratives are successful in telling compelling stories when the reader’s expectations for a satisfying end are met. This success depends on there being an end to the story. As Kermode (1967) put it, humans

in the midst make considerable imaginative investments in coherent patterns which, by the provision of an end, make possible a satisfying consonance with the origins and with the middle. (p. 17)

Great works of fiction, for Kermode, vary the established conventions of literary form of the time, whereas the “popular story” will adhere more closely to them (pp. 17-18). A successful story, nevertheless, will have an end that does not *fully* meet our perhaps naïve predictions. The end must, to make a great story with meaning that captures something of the *reality* of the reader’s expectations, upset “the ordinary balance of our naïve expectations” (p. 18). Hence, there is, at the end, a “disconfirmation” of our expectations, but this is a requirement for a good story. We want our naïve expectations defied, according to Kermode, because in this disconfirmation we find a “consonance” at the end of the story. That is, by having our expectations denied in a way that resonates with our *Denkmittel*, with our sense of “reality,” we are led to something new and rewarding in the meaning of the story; this is “related to our

wish to reach [this] discovery or recognition by an unexpected and instructive route” (p. 18).

### I. *Reality*

But what is the relationship between narrative and “reality”? For Kermode (1967) and White (1981a, 1987), who are the classic figures of contemporary historiography,<sup>65</sup> there is no escaping the conclusion that the plot structure of narratives is a “regulative fiction” that is *imposed* on reality.<sup>66</sup> There is a distinction, then, between *real* events and the *stories* or *narratives* we tell about them. Alternatively, to put it another way, we attempt to *represent* events that happen in the real world in the form of narratives, but these attempts at representation should not be taken as perfect reflections or mirrorings of the true nature of reality. After Nietzsche and the transition from nineteenth century positivist historiography—with its rigid narrative structures, predictable endings, disembodied narrators, and ethical absoluteness—to modernist fiction, fiction writers began to resist the traditional closure of the novel—with the ubiquitous death of the hero and ostensibly timeless moral authority—and instead to problematize their own position as the ostensibly disembodied author (Erdinast-Vulcan 1994). This involved a conscious understanding that the use of narrative in storytelling also involved the imposition of fictions on a reality that resisted this imposition. Indeed, to take these “regulative fictions” *too* seriously, as did some nineteenth century authors, was to reify them, or as Kermode (1967) put it, to make them into “myths” (p. 39). To treat fictional literary tools as myths is to treat them as metanarratives. By not consciously realizing that narratives are fictions, an author conflates fiction with reality; the fictions “degenerate into myths” (p. 39), where myth

presupposes total and adequate explanations of things as they are and were; it is a sequence of radically unchangeable gestures. Fictions are for finding things out, and they change as the needs of sense-making change. Myths are the agents of stability,

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<sup>65</sup> Cf. Erdinast-Vulcan’s (1994) excellent analysis, p. 146, fn. 2.

<sup>66</sup> Fine’s (1993) discussion of “fictionalism” and his analysis of the “as-if” philosophy of Hans Vaihinger (1852-1933), a contemporary of the logical positivists, shows that the idea of regulative fictions had some import in the philosophy of science. However, as with Neurath and his more naturalistic and particularist ideas, Vaihinger was marginalized and has been largely ignored. Fine’s analysis shows, however, that Vaihinger held a position that comes close to Fine’s (1984, 1986) “Natural Ontological Attitude” and to Rouse’s (2002a) radical philosophical naturalism (see Ch. III below). Vaihinger held that fictions are important in the *practice* of science; his view was critical, tolerant, and was widely known in its time.



fictions the agents of change. Myths call for absolute, fictions for conditional assent. Myths make sense of a lost order of time . . . ; fictions, if successful, make sense of the here and now, *hoc tempus*. (p. 39)

The cost of treating such fictions as real is to adopt them, as Lyotard (1984) argued, as totalizing metanarratives—the results of which are often catastrophic. Anti-Semitism, the Third Reich, the Final Solution are examples of the results of treating various cultural fictions as real, as myths to be reified.

Kermode (1967) and White (1981a) further argue that this scheme concerning narrative fictions can be applied to the writing of *history*. That is, even in the writing of an historical narrative, fictions are imposed on the events of the story—in this case the ostensibly “real” events of the past—in order to create the “formal coherency that stories possess” (White 1981a, p. 20). It is evident from the outset that there is an irony here. There is a tension between historical *reality* (History-0) and the *stories* that we write about it (History-3). On the one hand, we have faith that there is an historical reality “out there,” yet it seems we must conclude that we cannot have definitive or total knowledge about it. And on the other hand, it seems that in taking seriously postmodern historiography, that in pushing the distinction between the *real* and what we can *know* about it, we are led to a position in which the distinction seems to break down. Should we abandon the fiction/reality dichotomy altogether, and if so, on what grounds?

White (1981a) believes that professional historians, in their desire to make historiography into a science, have elevated the “narrativity of historical discourse” (p. 23) to the status of a metanarrative, in the sense used here (see also Rösen 2005, esp. chs. 1, 4). Again, “narrativity” here is, according to White, the notion that historical discourse *must* be in the form of a narrative—a coherent, “explanatory” story with a beginning, middle, and end—to be considered “proper history.” That is, for the professional historian,

it is not enough that a historical account deal in real, rather than merely imaginary events; and it is not enough that the account in its order of discourse represent events according to the chronological sequence in which they originally occurred. The events must be not only registered within the chronological framework of their original occurrence but narrated as well, that is to say, revealed as possessing a structure, an order of meaning, which they do *not* possess as mere sequence. (p. 5)

In other words, because a mere chronicle of events does not embody the meaning narrativity provides to it, and perhaps because we humans “can perceive a duration [of time] only when it is organized” (Kermode 1967, p. 45) into the coherent framework of a completed narrative—historians require that stories about the past be told in the form of a completed narrative (History-3) in which the evidence (History-1) seems to “speak for itself” (i.e., give us History-0). The coherence of the completed narrative, with its plot—revealed at the *end* to be immanent in the story from the *origin*—that imbues the events with meaning, must, then, be an “embarrassment” to the historian because it “has to be presented as ‘found’ in the events rather than put there by narrative techniques” (White 1981a, p. 20). A reason for this embarrassment is, in part, that all narratives, all stories we tell, must have an end. However, as White notes, “we cannot say, surely, that any sequence of real events actually comes to an end, that reality itself disappears, that events *of the order of the real* have ceased to happen” (p. 22).

Some postmodernists have been interpreted to say that we cannot extricate ourselves from narrativity. As the argument goes, since the distinction between fiction and reality (or nonfiction) has been problematized, then consequently everything is fiction.<sup>67</sup> Derrida ([1967] 1976) is by now famous (or infamous) for proclaiming that there is “nothing outside of the text” (p. 158). He can be read as saying that there is no world “out there.” However, he can also be interpreted as saying that whenever we attempt to refer to the real world, we are doing so in an inevitably *mediated* way, with texts referring to other texts, and so forth, such that we cannot completely extricate ourselves from our use of discourse. It does not follow from this that the world does not exist. In addition, Derrida’s ([1972] 1981, p. 41) program of “deconstruction” can be seen as a way of exposing the very narrative structure of

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<sup>67</sup> See, for example, Sokal and Bricmont (1998), p. 2. Do we conclude from this that there is, in fact, no reality, no real world; or, do we say that there are epistemological problems concerning what we might say about our right to proclaim what we *know* about the world out there? Postmodernism need not lead, as it perhaps does for Jean Baudrillard (1929-2007), to nihilism (*Simulacres et simulation*, Paris: Galilée, 1981). However, we should not, as Harvey (1989) urges us, fall into the trap of renouncing political action (ch. 6). But even Baudrillard’s (and Nietzsche’s) version of nihilism does not deny the real world, as Allison (1999) argues: “Baudrillard’s inquiry into the transfiguring and transforming play—the seductive game—of objective appearances would serve as a modest beginning to counter the totalizing systems of purposive interpretation, whose legitimate agency . . . may be largely nominal.” And it is this “seduction” that “would in large part constitute Baudrillard’s return to the actual, to the primacy of the objective domain in its sensible integrity, its objective necessity” (p. 184).

texts, literary or historical, that I have described above. Authors' "true" intentions, original meanings, and foundational (or fixed, objective) meanings are all revealed as rhetorical or fictional literary strategies, rather than epistemically privileged, or sovereign, modes of knowledge production.<sup>68</sup> However, it does not necessarily follow from such arguments, those that problematize the fiction/reality dichotomy, that the real world does not exist. That would amount to accepting the fiction pole of the dichotomy. Problematizing a dichotomy does not mean reducing one pole of the dichotomy to another. Postmodern thinking, as I (and others) envision it, means, among other things, problematizing the dichotomies of modernity and developing a position that does not swing to either pole.<sup>69</sup> There are, in many cases, real possibilities other than the binary oppositions of modernity. The notion of *reality* is discussed further in the next chapter.

## II. *Temporality*

In the discussion of narrativity above, another concept that begs problematization and

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<sup>68</sup> Derrida ([1972] 1981) described his "*general strategy of deconstruction*" as a means "to avoid both simply *neutralizing* the binary oppositions of metaphysics and simply *residing* within the closed field of oppositions, thereby confirming it" (p. 41). He believed that inherent in such dualisms is a "violent hierarchy." Hence, one pole of the opposition will dominate the other; "[t]o deconstruct the opposition, first of all, is to overturn the hierarchy at a given moment. To overlook this phase of overturning is to forget the conflictual and subordinating structure of opposition. Therefore one might proceed too quickly to a *neutralization* that in *practice* would leave the previous field untouched, leaving one no hold on the previous opposition, thereby preventing any means of *intervening* in the field effectively" (*ibid.*).

<sup>69</sup> White's (1972, 1981a, 1987, 1995) criticisms of historians for their "blindness to theory" (1995, p. 244) was challenged by Marwick (1995), who attacked "postmodernists" in general, and White in particular, for being too "metaphysical." According to Marwick (1995), "[w]hen the postmodernists talk of 'historicizing,' this means producing a very naïve, formulaic history, one which stifles genuine curiosity about the past, and is potentially harmful if students are not also introduced to the history of the historians" (p. 29). Marwick's (1995) attack, replete with character assassination and name-calling, amounts to a thorough misunderstanding of postmodernism and of White's metahistorical work, and is a defense of the turf of professional historians, which he believes is being invaded by ignorant nonprofessional philosophers and social and literary theorists, operating under the fraudulent veil of postmodernism and its direct connection to the (metaphysical and insufficiently empirical) historical materialism of outdated Marxism. In his response to Marwick, White (1995) observed that "Marwick's characterization of 'Postmodernism' is so bizarre and uninformed . . . that there would be little point in trying to discuss the issues he wishes to raise in terms of this concept" (p. 234). White (1995) reiterated his belief that "[m]ost historians are not only incapable of analysing the discursive dimensions of their writing, they positively repress the idea that there may be such a dimension. In the professional training of historians, there is much talk of the 'historical method' . . . , but not even talk of how to *write* a historical work, whether of a narrative or an argumentative kind" (p. 245).

deconstruction is *temporality*. In claiming that a narrative is a coherent and completed story, with a beginning, middle, and end, and that what makes a story compelling is a plot structure that allows us to see the end immanent in the beginning, one is speaking about *time* and how we humans experience it. Moreover, one is making a distinction (at least) between the time structure of a narrative and that of our experience in the world. If, as Paul Ricoeur (1981) has argued, narratives give an “illusion of sequence” to the events one is telling a story about (p. 165), then it seems we must conclude there are different “forms” or structures of time, and that one thing narratives do is to represent time as a “linear succession of instants” (p. 166). In addition, if there is a distinction between the fictionalization of reality in the narrative structure, on the one hand, and events in the world or the past *wie es eigentlich gewesen ist*, on the other hand, then the tension between the linear time of a narrative and the time structure of “reality” should be deconstructed.

Ricoeur (1981) sees the problem of narrative time to be related to another dichotomy of modernity, that between the “chronology of sequence” in narratives and the “a-chronology of models,” as in supposing the events of the past to be nomological (p. 165). He supposes, as do the classic historiographers White (1981a, 1987) and Kermode (1967), that there is an intimate relationship between narrativity and temporality—he believes “temporality to be that structure of existence that reaches language in narrativity and narrativity to be the language structure that has temporality as its ultimate referent” (p. 165). In this reciprocal relationship between narrativity and temporality, Ricoeur (1981) argues, drawing on Heidegger ([1957] 1962), that the linear temporality of narrative structure, with its “linear series of ‘nows’” (p. 166), masks a deeper representation of time. Ricoeur agrees with Heidegger that there are at least three levels of time. First, there is “within-time-ness,” or time “‘in’ which events take place” (p. 166). Second, there is what Heidegger ([1957] 1962) called “historicality” (*Geschichtlichkeit*),<sup>70</sup> with its emphasis on “the weight of the past,” our

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<sup>70</sup> Ricoeur (1981) translates *Geschichtlichkeit* as *historicality*, whereas Macquarrie and Robinson (Heidegger [1957] 1962) and Stambaugh (Heidegger [1953] 1996) translate *Geschichtlichkeit* as *historicity*. Stambaugh translates *Historizität* as *historicality*. While I do not hope to reconcile these differences, it seems that Ricoeur may be trying to avoid any English-speaking confusions of *historicity* with *historicism* (Stambaugh translates *Historismus* as *historicism*); therefore, he translates *Geschichtlichkeit* as *historicality* and not as *historicity*. For an excellent analysis of historicism, see D’Amico (1989).

“‘extension’ between birth and death,” and the notion of “repetition,” or the retrieval from the past of our “basic potentialities” we humans have inherited from that past (Ricoeur 1981, pp. 167, 176). Finally, there is deep “temporality,” or the “plural unity of future, past, and present” (p. 167). In the next chapter I take up these issues in some more detail; for now I focus on Ricoeur’s (1981) major claim regarding the connection between narrativity and temporality.

Ricoeur (1981) believes that any narrative combines two traits of time—one chronological and one nonchronological. These he terms the “episodic” and the “configurational” dimensions of the narrative, respectively (pp. 173-4). The episodic dimension of a narrative is that which displays the story as made out of a succession of events in chronological time order. The configurational dimension is that property of a narrative “according to which the plot construes significant wholes out of scattered events” (p. 174). In other words, in the telling of a story, as I have described above, a narrative with its plot structure provides a coherence, a moral significance, to the linear sequence of events being represented (retold). What Ricoeur (1981) argues is that a narrative functions to “provide a transition from within-time-ness to historicity” (p. 174). That is, the narrative allows us to apprehend the events of the past, and the humans and their actions—which happened “in” time (the episodic dimension)—and, in addition, the narrative “also brings us back from within-time-ness to historicity, from ‘reckoning with’ time to ‘recollecting’ it” (p. 174). Hence, it allows us to “apprehend a set of historical events under a common denominator” (p. 175). In doing so, the narrative displays its configurational dimension, and allows us, I argue, to answer “*why* did it happen?” and as such to do History-3. In addition, for Ricoeur (1981), narratives function to “establish human action at the level of genuine historicity, that is, of repetition” (p. 176). Narratives, then, enable us humans to apprehend the past as more than a sequence of events. They enable us to see the past as a story:

By reading the end into the beginning and the beginning into the end, we learn to read time backward, as the recapitulation of the initial conditions of a course of action in its terminal consequences. In this way, the plot does not merely establish human action “in” time, it also establishes it in memory. And memory in turn repeats—recollects—the course of events according to an order that is the counterpart of the stretching-along of time between a beginning and an end. (p. 179)

And further, we “retrieve” from the past a sense of our “personal fate” and “common destiny.” Therefore, by telling a story—a narrative—we humans gain a sense of our place, significance, and meaning to being in the world. We are thus able to do more than recite a chronology of events of the past. We can do History-3; we can *explain* those events; we can say how and why those events happened as they did. It seems, if Ricoeur is right, we *must* tell stories about history to make sense of the present, and those stories *must* be narratives; it could be no other way. Narrativity is, on this view, required for we humans to make sense of ourselves.<sup>71</sup> Nevertheless, what does seem clear is that a thoughtful consideration of *temporality* is useful in bringing to light issues regarding historical understanding and the foundations of history (i.e., History-4 and -5), issues that even historians themselves do not normally problematize. I analyze aspects of this deconstruction of time in the next chapter, and subsequently incorporate them into the technological infrastructure of science.<sup>72</sup>

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<sup>71</sup> This is not to suggest, however, that Ricoeur is a structuralist who argues against what David Carr ([1985] 1991) has called the “standard view” (p. 161), that is, the insistence by those such as Kermode (1967) that narrativizing the past (creating History-3s) adds a level of distortion to what actually happened in the past. Ricoeur’s ([1983-5] 1984-8) classic, *Time and Narrative*, offers as one of its central arguments (in vol. I) that history cannot be subsumed under a nomological model (see below and Taylor ([1985] 1991). Carr (1986), arguing against Kermode and the standard view, believes that historical and fictional narratives are “not distortions of, denials of, or escapes from reality, but extensions and configurations of its primary features” (p. 16). In Chapter IV below, I grapple with this problem in the context of scientific experimentation and argue that Rheinberger’s (1994, 1997) use of Derrida’s *historiality* shows that historical narratives (History-3s) have an element to them that seems inevitably to add a dimension of distortion to what happened in the past. The modernist historiographical worry that there is *only* a dichotomous choice between the *representation* of the past, on the one hand, and a vicious circularity or textuality, on the other hand, is—as Ricoeur ([1985] 1991) argues and I explore in Chs. III and IV below—a nonproblem when considered from an appropriately postmodern perspective (although many thorny issues remain).

<sup>72</sup> The implication here of problematizing the distinction between the past *wie es eigentlich gewesen ist*, on the one hand, and the past *as we can know it* from the perspective of people living in the present (or of the truth/fiction dichotomy), on the other hand, is not that the past did not happen, or that we cannot have “objective” knowledge of it. To suggest that reconstructing the past (which is always from the context of the present) adds an element of “distortion” or fiction to the past in attempting to tell *why* something happened (i.e., in trying to *explain* the events of the past, as opposed to giving a recitation of events), means that we add, consciously or not, elements to stories about the past, for which we have no definitive way (i.e., no nomological method or disembodied, privileged position) of verifying whether or not those elements are part of the *Wirklichkeit* of the past. This should not, however, amount to consciously adopting a presentist or Whiggish position that deliberately adds anachronisms to the past. The goal remains the empirical reconstruction of the past based on documentary sources.

### III. *Causality*

The final concept that begs problematization from the preceding discussion of narrativity is *causality*. In telling a story, including a story about the past, the author or historian is, in effect, imposing (a series of) causes on the contingencies of the events in the story. To make the story coherent, to make it feasible to apprehend it as a whole, the author must provide some series of causes in order to rank and link the events. However, in doing so, the author is providing an account of events for which there is more than one way to provide its coherence. Hence, White (1981a) believes that for an account of events to be considered “historical,” it

must be susceptible to at least two narrations of its occurrence. Unless at least two versions of the same set of events can be imagined, there is no reason for the historian to take upon himself the authority of giving the true account of what really happened. The authority of the historical narrative is the authority of reality itself; the historical account endows this reality with form and thereby makes it desirable, imposing upon its processes the formal coherence that only stories possess. (p. 19)

For the literary novel, Kermode (1967) puts it this way:

As soon as it speaks, begins to be a novel, it imposes causality and concordance, development, character, a past which matters and a future within certain broad limits determined by the project of the author rather than that of the characters. They have their choices, but the novel has its end. (p. 140)

And finally, Ricoeur (1981) states his “paradox of contingency” as follows:

There is no story if our attention is not moved along by a thousand contingencies. This is why a story has to be followed to its conclusion. So rather than being predictable, a conclusion must be acceptable. Looking back from the conclusion to the episodes leading up to it, we have to be able to say that this ending required these sorts of events and this chain of actions. (p. 170)

So, it seems, we must have causality with contingency. But how is this possible? What does it mean to suppose there are causes in the world, yet they are not determinate or predictable, perhaps even in principle? Do we look to physics for our answers? To social theory? To narrativity?<sup>73</sup>

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<sup>73</sup> We might look to a postmodern formulation of narrativity, as exemplified in *Finnegans Wake*, by James Joyce (1882-1941); Joyce finished the *Wake* in 1939. If one takes the next step in the postmodernization of narrativity and problematizes further the notion that distortion is inherent in narrative attempts to reconstruct

Causality itself appears now to require deconstruction. It seems that if a coherent notion of history is to be formulated, we need a coherent notion of causality and how it might operate in the world. We have come full circle to Hempel's ([1942] 1965b) problem of how to model causes operating in history (see Ch. I above). Another postmodern irony is here evident. On the one hand, it seems reasonable and, perhaps we can say, natural to suppose that there are causes operating in the world. While it may be difficult to model exactly what is happening when an event *causes* another event to happen, it seems unreasonable to conclude that anything at all could happen in a given physical or historical situation; scientists would surely disagree with this characterization. On the other hand, to reduce to causality the kind of *meaningful interpretation* the historian requires in an historical narrative seems equally absurd (cf. Danto 1956). However, Hempel's desire to consider how causes operate in history remains a laudable effort. We may reject his formal style of attacking the problem; we may also repudiate his method of attempting to reduce historical events to nomological explanations that leave no room for chance or contingency.<sup>74</sup> Nevertheless, Hempel was grappling with a problem that historians and others have been struggling with, at

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the past (that is, to tell a story, fiction or nonfiction), then the need to posit that the end of a story is inherent in its beginning *itself* begs deconstruction. Why not the beginning inherent in its end? If backward glances—recurrence—or story-telling inevitably distorts through hindsight, then it seems that the end of the story could influence how the reader views the beginning. In *Finnegans Wake*, as Eco ([1966] 1982) shows, “the causal relationship of events is entirely different [from the traditional, even modernist, novel, including Joyce's *Ulysses*]. The manner in which we understand a term totally changes the way in which we understand the term in the preceding pages, and the way in which we interpret an allusion deforms the very identity of its remote antecedents. It is not the case that the book finishes because it has begun in a certain way; rather, *Finnegans Wake* begins because it finishes in that way. . . . [The book] does not ‘speak’ about temporal subversion; it speaks *by means of* causal subversion at the level of the ‘telling process,’ not that of the ‘told content.’ In the *Wake*, the co-presence of diverse historical identities arises because there exist precise structural and semantic conditions that deny the causal order to which we are accustomed. Semantically ‘closed’ chains are established and, as a result, the total work permits the reader to freely construe the semantic universe according to the reversible order of causality” (pp. 75-76). The significance of this narrative strategy is taken up in Chapter IV below when Rheinberger's (1994, 1997) notion of “the spontaneous history of the scientist” is considered in the context of the technological infrastructure of science.

<sup>74</sup> Ricoeur (1981) would consider Hempel's ([1942] 1965b) effort to be one that imposes a nonchronological model on the necessarily chronological events of the past. Thus, Hempel does not problematize the chronological/nonchronological dichotomy. Traditional narratives, and presumably the events of history, have a time sequence that goes from beginning to end; they are directional. One could construct a nontraditional narrative, as in the *Wake*, to illustrate certain postmodern features of culture and narrativity. However, to presume that the actual events of History-0 are lawlike strips history of its contingency. It also strips time of its arrow into the future, and by making historical events determinate and predictable—the antithesis of Joyce's *Finnegans Wake*—the goal of the historian, to create narratives that give meaning to the events of the past and to specify how *and why* they happened, is obliterated.



least implicitly, for centuries. Moreover, he probed its limitations and revealed many of the problems associated with a nomological characterization of history.

Ricoeur (1981) would consider Hempel's effort to be one of imposing a nonchronological model on the inevitably chronological—that is, directed in time from a beginning to an end—stories that we must tell about the past. To say we can predict future events seems to defy our common humanity and understanding. But, alas, to say “anything goes” seems as equally hollow as historical determinism.<sup>75</sup> However, if we say causes operate in history, in the world, and if we are to retain our postmodern stance for developing an understanding of how history, science, technology, and so on, work—then we must develop a coherent account of how to include causality in narrativity. This I undertake in the next chapter. My goal is, again, to develop an account of the activities of those who do science and technology, one that is faithful to the postmodern worldview specified in this dissertation. To understand how scientific practice is “explained” by considering its technological infrastructure, I must show how that practice is to be understood historically—how else could we understand it? Furthermore, for it to be understood historically, I need coherent notions of *the real*, *temporality*, *causality*, and ultimately, of *social* and *cultural*. These I develop further in the next (and later) chapters, in which I consider what a postmodern philosophy of science (and technology) might be like.

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<sup>75</sup> Feyerabend's ([1975] 1978) “anything goes” (p. 28) was intended to show “that the idea of a fixed method, or of a fixed theory of rationality, rests on too naïve a view of man and his social surroundings” (p. 27). Feyerabend wanted his epistemological stance to be viewed as epistemological “anarchism,” yet he also saw himself “as a flippant Dadaist and *not* as a serious anarchist” (p. 21, fn. 12). To the extent that Feyerabend gestures to the radical contingency of history as a general methodological prescription, his views are consistent with this dissertation. In any given context, however, not all explanations or narrative reconstructions will go.

## CHAPTER III

### Toward a Postmodern Philosophy of Science

In the tale, in the telling, we are all one blood.  
Take the tale in your teeth, then, and bite till the blood  
runs, hoping it's not poison; and we will all come to  
the end together, and even to the beginning: living,  
as we do, in the middle.

—Ursula K. Le Guin<sup>76</sup>

In this chapter, I consider what a postmodern philosophy of science might look like. First, I focus on the rift between the worldviews of contemporary philosophers of science (and some scientists) and postmodernists, and argue that there is a significant barrier to mutual understanding. Next, I undertake an attempt at suggesting how the philosophy of science might proceed with a postmodern flavor. In doing this, I focus on the work of Joseph Rouse (esp. 1987, 2002a) and Hans-Jörg Rheinberger (esp. 1994, 1997), two philosophers of science who, I argue, come closest to embracing a postmodern perspective. I analyze important issues in their work, paying particular attention to how *narrativity* plays a role in their philosophies. In addition, I continue the discussion from the previous chapter on the role of *temporality*, *the real*, and *causality* in constructing an adequate philosophy of science. I pay particular attention to Rouse's (2002a) turn to "naturalism" and, it seems, away from narrativity, and to Rheinberger's epistemology of time as he employs it in his analyses of "experimental systems." I incorporate aspects of both of these philosophers (and others) in an argument for how to proceed to build a postmodern philosophy of science. In Chapter V, by way of example, I use the position I have constructed (or re-reconstructed) in order to deconstruct the arguments of Lucas and Hodgson (1990), according to which causality is basic to the Special Theory of Relativity and the reality of Minkowski spacetime is to be taken as an established fact of modern physics. This analysis of postmodern philosophy of science I use as the basis for my development of the *technological infrastructure of science* construct, which is designed to be not only a prescription for doing research involving

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<sup>76</sup> Le Guin (1981), p. 195.

science and technology as cultural practices, but also a means for understanding the significance of science and technology as human practices.

### **Philosophy of Science and Postmodernism**

Few philosophers of science today, it seems, would identify themselves in public as advocating a postmodern perspective.<sup>77</sup> The narratives of the discipline of philosophy of science are too immersed in the metanarratives of modernity—with their commitments to the transcendental themes of rationality, fixed objectivity, and progress toward truth—to take seriously the postmodern worldview (cf. Lelas 2000, part 1, esp. p. 4). Even though it can be argued that most scientists themselves—in their practice, in their work—need not and do not adopt such an extreme attitude, it seems that philosophers of science, who seek to justify the preeminence of science as the privileged engine of truth, have in their narrative embeddedness a deep-seated commitment to the worldview of modernity, with the Enlightenment as the critical turning-point in the history of knowledge (cf. Lyotard [1979] 1984, esp. pp. 23-53). In addition, when scientists turn from their own practice as scientists and enter the narrative fields of history, philosophy, or social criticism, they tend to adopt a modern position that reinforces the cultural norm that has science as the privileged knowledge engine (e.g., Gross and Levitt 1994; Marx 1994, Sokal and Bricmont 1998). In its extreme, this privilege, as Lelas (2000) argues, is grounded in a position between radical skepticism and absolutist dogmatism and requires, as much as possible, that these conditions be satisfied:

- (1) The human mind must be cleansed of everything human.

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<sup>77</sup> Laudan (1990) asserts that “strong forms of epistemic relativism derive scant support from a clearheaded understanding of the contemporary state of the art in philosophy of science. I am not alone in that conviction; most of my fellow philosophers of science would doubtless wholeheartedly concur” (p. viii). He states that “in this ‘post-positivist’ era, many scientists (especially social scientists), literati, and philosophers outside of philosophy of science proper have come to believe that the epistemic analysis of science since the 1960s provides potent ammunition for a general assault on the idea that science represents a reliable or superior form of knowing” (*ibid.*). Laudan’s (1990) book, written in the form of a dialogue, demonstrates how a philosopher of science, who holds the following position, can misunderstand scholars outside *his* field: “The displacement of the idea that facts and evidence matter by the idea that everything boils down to subjective interests and perspectives is—second only to American political campaigns—the most prominent and pernicious manifestation of anti-intellectualism in our time” (p. x).

- (2) During the cleansing a set of privileged representations must be identified in the human mind, and the role of being the foundation granted to it.
- (3) The divine gift, i.e. reason, operating on this set must be used according to the scientific method. (p. 16)

While Lelas (2000) intended this description to be relevant to those historical periods historians of science traditionally call the Scientific Revolution and the Enlightenment, we find remnants of it in the discipline of the philosophy of science today.<sup>78</sup> Sir Peter Medawar ([1963] 1990) criticized this Baconian inductivism in his attack on the typical scientific paper this way:<sup>79</sup>

Then comes a section called ‘results.’ The section called ‘results’ consists of a stream of factual information in which it is considered extremely bad form to discuss the significance of the results you are getting. You have to pretend that your mind is, so to speak, a virgin receptacle, an empty vessel, for information which floods into it from the external world for no reason which you yourself have revealed. You reserve all appraisal of the scientific evidence until the ‘discussion’ section, and in the discussion you adopt the ludicrous pretence of asking yourself if the information you have collected actually means anything; of asking yourself if any general truths are going to emerge from the contemplation of all the evidence you brandished in the section called ‘results.’ (p. 228-9)

However, we find the distinguished philosopher of biology Marjorie Grene (1985), in her plea for a new philosophy of science, proclaiming that while “science . . . always entails interpretation,” this claim “is a question, as people liked to say in the seventeenth century, of

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<sup>78</sup> Mumford’s (1961) view of “Science as Technology” is similar to Lelas’ (2000) critique of Enlightenment ideals and Baconian inductivism described above, yet it differs from Lelas’ (1993) view of “Science as Technology,” described in Ch. V below. Mumford’s (1961) view focused on “Bacon’s conception of the organization of science as a technology. . .” (p. 508). This organization, “to be fully effective, must enlist, not solitary and occasional minds, but a corps of well-organized workers, each exercising a specialized function and operating in restricted areas” (*ibid.*). Mumford’s view, however, was somewhat pessimistic: “Have we not already evidence to show that science as technology may, through its inordinate growth, become increasingly irrelevant to any human concerns whatever, except that of the technologist or the corporate enterprise; that, indeed, as in the form of nuclear or bacterial weapons, it may not be merely coldly indifferent but positively hostile to human welfare?” (pp. 510-11). For more on Mumford’s philosophy of technology, see Ch. IV below.

<sup>79</sup> Ironically, Medawar ([1963] 1990), the Nobel Prize-winning immunologist, presented his attack on inductivism in the context of his defense of Popperian testability and falsifiability, or conjectures and refutations, a method he refers to as “the hypothetical-deductive interpretation” that is embodied in Popper’s ([1934] 1959) *The Logic of Scientific Discovery* (New York: Basic Books). Medawar claims this method originated with the geologist William Whewell (p. 232). Brandon (1994) provides a useful discussion of inductivism and hypothetico-deductivism and their inadequacies for accounting for experiments in evolutionary biology. See also Burian’s (1994a) comment.

reading the text of the world” (p. 9). Nevertheless, Grene insists,

that is not to say that science is subjective either, a matter of fashion, convention, or sheer arbitrary choice. Objectivity, like judicial impartiality, is a standard that is genuinely authorized within certain societies. It is *disinterestedness*, which is itself an interest, the target of care. Like the stamina to run a marathon, or the persistence to get through medical school, or the ingenuity needed to devise experiments, it has to be cultivated and sustained by an arduous training program. (p. 10, emphasis added)

For Grene (1985), then, when scientists cleanse themselves of bias and operate properly as neutral investigators of nature, they “may affirm realism as a necessary, immediate, and primary ground of scientific inquiry and of scientific knowledge” (p. 6). Furthermore, the knowledge that results from this activity “designed to give us new access to some features of nature itself” (p. 6), if successful, succeeds because science is a privileged activity aimed at finding “the truth about some natural phenomenon, as distinct from the nature of human experience as such. . .” (p. 12). And finally, if a scientific activity “becomes so refined as to be dealing with artifact, it has failed as science” (p. 7).

One has only to compare this view of science with the concept of narrativity as I developed it in Chapter II to see the divergence of perspective between traditional philosophy of science and any formulation of a postmodern philosophy of science.<sup>80</sup> For now, I only point to the apparent circularity in the above view by noting that, if the standard for distinguishing failure from success in scientific activity consists in waiting until an experimental narrative has played itself out, and then seeing whether the scientific

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<sup>80</sup> West ([1981] 1999) refers to Quine, Goodman, Sellars, Kuhn, and Rorty as “postmodern American philosophers” who inherited Nietzsche’s “move toward antirealism or conventionalism in ontology;” his “move toward . . . antifoundationalism in epistemology;” and his “move toward detranscendentalization of the subject. . . .”; but that these philosophers did not offer a Nietzschean “countermovement” that could “overcome . . . nihilism and skepticism” (p. 189). Hence, West considers their efforts as having left “postmodern American philosophy hanging in limbo, as a philosophically critical yet culturally lifeless rhetoric mirroring a culture (or civilization) permeated by the scientific ethos, regulated by racist, patriarchal, capitalist norms and pervaded by debris of decay” (p. 210). West’s ([1981] 1999) views on politics, postmodernity, and Nietzsche are consistent with this dissertation’s views; however, there seems to be only a limited sense in which the philosophers he mentions are postmodern. In addition, West goes much farther in that he states, if not argues, what is wrong with much of the discourse of contemporary Western culture, including philosophical discourse. These views, and West’s (1999) general philosophical and political orientation are, in part, what this dissertation is attempting to legitimate as appropriate types of philosophical investigations from the perspective of the technological infrastructure of science. What are needed are detailed historical arguments using postmodern principles, such as those developed in this dissertation, that support these views, many of which West (1999) has supplied.

community has declared the investigation to be “artifact,” or “noise,” rather than “signal,” or “entity”—it does not follow that the “truth,” that is, the *end* of the narrative story about the scientific activity in question, is what has brought the narrative to its end. A scientific theory or experiment can be successful, as Hacking (1983, 1992) and Latour (1987), among others, have so clearly argued, without being “true,” that is, without being a truthful representation of what the *real* world is like.<sup>81</sup> The traditional naturalistic presupposition of being in the world, without adequate problematization, ignores narrativity and the epistemological and ontological problems that result.

Clearly, what is lurking behind Grene’s (1985) analysis is her commitment to a form of philosophical *naturalism*, according to which the obvious aim of the scientist “is knowledge of the real world because he or she has never left that world” (p. 6). That is, for Grene, “we are realists about science from the start” (*ibid.*). From this perspective, humans as scientists retain a privileged epistemological position because of their being in the world

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<sup>81</sup> Although we do not necessarily need the correspondence theory of truth (i.e., accurate theoretical representations of what fundamental reality is really “like”) to account for the success of science as a practice, it still may turn out that some formulation of “truth” may help us to account for why some scientists are successful, and others are not, at least in particular contingent historical examples. Hull (2001, ch. 8) distinguishes between two motivations of scientists, the search for truth and the desire for recognition, and he concludes that scientists who have *both* motivations are “likely to be more successful in discovering the truth than those who are motivated by the desire for acquiring credit or truth alone” (p. 183). What version of “truth” Hull is advocating beyond “successful knowledge,” “reliable knowledge,” or “accepted knowledge” is not clear, but it is not correspondence. Hull (1998), however, is another philosopher of science who criticizes science studies and its “relativist” tendencies. In his criticism of a Virginia Tech science studies research program of the 1980s designed to empirically study science “scientifically,” Hull (1998) provides a compelling critique of the efforts of Larry Laudan, Arthur Donovan, Rachel Laudan, Peter Barker, and others to study theories of scientific change empirically (Larry Laudan, Arthur Donovan, Rachel Laudan, Peter Barker, Harold Brown, Jarrett Leplin, Paul Thagard, and Steve Wykstra (1986), “Scientific Change: Philosophical Models and Historical Research,” *Synthese* 69: 141-223; see also Arthur Donovan, Larry Laudan, and Rachel Laudan, eds. (1988), *Scrutinizing Science: Empirical Studies of Scientific Change*, Dordrecht, The Netherlands: Kluwer Academic Publishers). Unfortunately, Hull (1998) appears to lump these researchers (including Larry Laudan!) in with the “[r]elativist students of science” (p. 225) and he suggests that “many of those who study science are more interested in debunking it than understanding it” (p. 225). In suggesting that “students of science have retraced all the familiar ground that generations of philosophers have trod before them,” (p. 226) Hull trivializes the efforts of many of those in science studies, suggests that philosophers of science have special access to truths about how science works, and implies that they are wasting their time. While there is undoubtedly some truth to these claims for particular cases, anyone who lived through the Laudan period of the 1980s at Virginia Tech or its aftermath in the early 1990s (as I did), knows that such easy generalizations of science studies efforts are patently inaccurate. More accurate is Hull’s (1998) claim that “those of us who study science [are] subdivided into numerous factions, each fighting to maintain its own turf. . .” (p. 225). Again, when the authority of science is attacked, or perceived to be attacked, labels such as “relativist,” “postmodern,” “naïve,” and others are so often invoked by philosophers of science (and scientists), yet they very often have not sufficiently studied the works of those they are so quickly dismissing.

and following the specialized practices of the sciences while investigating that world. But, by naturalizing humans as agents, and by placing humans themselves in the world and treating human knowledges also as natural phenomena, the naturalistic philosopher of science, as Lelas (2000) argues, has deemed that “humans have no privileged position whatsoever; they are endowed neither with a copy of [the] divine mind nor with [the] ability to speak [a] Cosmic Language” (p. 56).

What Grene (1985) seems to have not sufficiently problematized is the representationalist/performativity dichotomy (cf. Pickering 1994). We may agree that humans are beings in the natural world, but at the same time they, as scientists (at least) attempt to represent the world by interacting in and with it. In addition, if they exist, as beings in the world, and in narratives or narrative fields, then they rely on representations of the past—that is, they find meaning in what they do at least in part because of *Geschichtlichkeit*, because of their being in a narrative context with a history attached to it. The important point here is that the philosopher of science should take into account how we humans stand in relation to *the real*, the world in which we find ourselves, and to *history*, or the narrative context(s) in which we find ourselves, and how that relation is, or is not, *mediated*. If we are in *the real*, we have no privilege by virtue of being outside of it. Our disembodied, god-like position is denied and cannot grant us the authority to privilege our knowledge of the world. So what, then, grants us the privilege that Grene (1985) seems to suggest we still retain by adopting a naturalistic philosophy? It seems that a coherent notion of scientific activity as performed by scientists in the world requires that we break away from the representationalist view of science (cf. Rorty 1979). Yes, scientists attempt to represent the natural world by interacting with(in) it. But if that activity of representing is not from a god-like, disembodied perspective, then what is the relation between the representations of science and the natural world, and what is it that grants us the authority to say that those representations are compelling (or truthful)?

Asking such questions and problematizing the dichotomies of modernity, I argue, is part of what it means to be postmodern. Scientists and philosophers (of science) do not normally invite discussions of narrativity into their domain. Their view of postmodernists

and what is important to them is often distorted and grossly incomplete, at least in part because of the rift between the worldviews (see, e.g., Shackel 2005).<sup>82</sup> Conversely, postmodernists do not normally engage the scientific worldview on its own terms; their view of scientists and what is important to them is, similarly, often distorted and grossly incomplete. Is it possible to broach this bifurcation?

In her article on the possibility of a postmodern philosophy of science, Parusnikova (1992) denies the possibility by belittling and mischaracterizing the ideals of postmodernity, at least as she sees them, and by erecting a view of the philosophy of science that adheres to the metanarratives of modernity. She frames her argument around the assumption that the only proper function of the philosophy of science is to tell scientists what to do (pp. 21-2). But, she argues, because the “natural sciences remain, to a large extent, exclusively esoteric, and, in their ‘pure’ forms are simply too difficult to converse about,” the “philosopher-ironist who is a philosopher-outsider can have nothing to say concerning internal scientific practices” (p. 26). Her argument here seems to rest on the same kind of assumption Grene (1985) made regarding the “arduous training program” scientists go through so as to provide them with the special tools to do privileged work (p. 10). Because non-specialists cannot speak the language or understand the concepts, then it seems that no philosopher who takes postmodernism seriously could possibly do philosophy of science. However, this argument fails utterly, since there are already credible philosophers of science who take postmodernism seriously (Rouse, Rheinberger, and Pickering are examples).<sup>83</sup> In addition, what is to prevent a philosopher from being trained in the sciences (e.g., Rheinberger, Pickering, and Haraway) and then going into the field of philosophy of science and embracing postmodernism?

In addition to adhering to a narrow view of what philosophers of science might do as philosophers, Parusnikova (1992) also presents a narrow and distorted view of

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<sup>82</sup> Shackel’s (2005) stylistic polemic against postmodernist philosophy is another example of a philosophical work that contains many of the usual criticisms of postmodernism, yet reflects a serious misunderstanding of (or blindness to) the works of Foucault, Rorty, Derrida, and Lyotard.

<sup>83</sup> But this does not entail that traditional philosophers of science will embrace or understand the work of postmodern philosophers of science. See, for example, Grene’s (1997) comments on Rheinberger’s work; Grene thinks Rheinberger’s grounding in continental philosophy—for example, in the works of Nietzsche, Heidegger, and Derrida—only obscures his otherwise important work and is unnecessary (pp. 274-6, esp. p. 276).



postmodernists and what *they* do. She believes that postmodernism entails a “focus on imagination as opposed to discipline and the rejection of any higher authority for the legitimation of the rules and goals of scientific performance” (p. 22). While the de-legitimation of a “higher” authority for knowledge generated by scientists is indeed a result of most postmodern analyses, it does not follow that the route to this perspective was the result of lack of discipline. I fail to see why the work of Foucault, to take one example, should be seen as the result of anything less than the painstaking work, over many years, on problems and themes that have a distinctive postmodern flavor. This criticism seems more to be the result of invoking a modern dichotomy—discipline/ imagination—and supposing that because postmodernists do not agree with her viewpoint, then they must lack the discipline that scientists and philosophers of science utilize when they make legitimate claims about the world. For those familiar with the arguments of many of the classic postmodernist philosophers—Foucault, Derrida, Lyotard, Ricoeur, to name a few—it is difficult to see how their work can be taken as undisciplined. Moreover, this applies also to more recent philosophers of science—Rouse (2002a), Rheinberger (1997), Haraway (1997), and Pickering (1995), to name a few. While their work is sometimes difficult to approach (but remember this was Parusnikova’s argument regarding *scientists’* work), it is disciplined, well-argued, brilliant work. And it happens to be postmodern.

In addition, Parusnikova (1992) also presents postmodernism as advocating a view that moves “the problem of meaning from a universal to a local dimension” (p. 31). For postmodernists, this move results from the realization that we humans cannot remove ourselves from narratives or from the world; there is no objective, disembodied perspective that can decide the meaning of something, once and for all. However, it does not follow, as Parusnikova insists, that postmodernism entails “that it is *fundamentally impossible to make meaning present*, regardless of the scope within which meaning is situated and analysed” (p. 31). In the previous chapter, I showed that meaning was one of the central problems in historiography. In addition, I show below that *meaning* ought to be a central issue for a postmodern philosophy of science, and further that it does not follow that because one has rejected a definitive way to grasp meaning, that this entails that there is *no* meaning to be

had. Yes, we should problematize the representationalist view of determining meaning, and we should conclude that the generation of meaning is always incomplete, but it does not follow from this that “we cannot understand the world” (p. 32), as Parusnikova would have us believe.

Finally, Parusnikova (1992) arrives at the conclusion that because, for postmodernists, “meaning is never complete,” the result of this is that “[n]o position can be defended” (p. 34). Yes, Derrida ([1967] 1976) concludes that all texts can be deconstructed; meaning is elusive and there exists no objective standpoint from which one can pronounce the “truth” about the final meaning of a text. Nevertheless, it does not follow from this that no philosophical (or other) position can be defended.<sup>84</sup> Parusnikova is trapped in this position primarily because of her entrenchment in modernity and her commitment to epistemic sovereignty. If the only choices are the binary dichotomies of modernity—in this case sovereignty/anarchy—then her conclusion makes sense. If the cherished notion of privileged epistemic authority is rejected, then the only alternative is complete anarchy, with meaning never present and the total inability even to defend a coherent position. Postmodernists, however, want to problematize as many of the dichotomies of modernity as possible. There can be a position that does not swing to either pole of the dichotomy. That is, by analyzing presuppositions, deconstructing arguments, and loosening one’s disciplinary boundaries, one can reach a “middle ground,” which is not to say a neutral ground. Hence, one can conclude both that meaning is never finally realized *and* at the same time, that one can defend a position, a non-privileged, partial, and postmodern position.<sup>85</sup>

I contend that a coherent postmodern position is possible—the key is to specify those

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<sup>84</sup> Rorty (1998a) defends Derrida against the backlash against deconstruction, which he believes was perpetuated “for the most part, by people who have not read his books. . .” (p. 328). As he states: “One reason why professionalized philosophers dislike Derrida so much (and why professional Derridean deconstructors, praising one another for the rigor with which yet another excluded element has been exposed as the presupposition of yet another text, are so hilarious) is that some people would rather be pure than get a life” (p. 345, fn. 21). Richard Rorty (1931-2007) died on 8 June 2007.

<sup>85</sup> Rorty (1985) argues that “[t]he best argument we partisans of solidarity have against the realistic partisans of objectivity is Nietzsche’s argument that the traditional Western metaphysico-epistemological way of firming up our habits simply isn’t working anymore. It isn’t doing its job. It has become as transparent a device as the postulation of deities who turn out, by happy coincidence, to have chosen *us* as their people” (p. 15).

premises about certain philosophical principles we are to retain, and then to construct a coherent position while following through on those principles. For the moderns, whether scientists, historians, or philosophers, it often seems they will permit no loosening of the view of science as the privileged and sovereign knowledge generator. Moreover, they will often attack the postmodernists, even unfairly and viciously, to defend their cherished worldview, which is still the dominant worldview in the realms of science and philosophy of science (e.g., Gross and Levitt 1994, Marx 1994, Sokal and Bricmont 1998, Stove 2001), and perhaps in most twenty-first century Western cultures. These moderns will even indict entire fields of knowledge or entire spheres of inquiry as “irrational” to support their viewpoints; for example, Science and Technology Studies and even the “academic left” have been targets. The strategies of these attempts to deflate postmodern arguments proceed much the same way as Parusnikova’s (1992) argument above. They presume the epistemological superiority of science from the beginning, in addition to a series of philosophical and scientific presuppositions that are often only implicitly stated, if at all. Then they proceed, by erecting a distorted and often ignorant characterization of postmodernism, and by building a seemingly unbridgeable gap between modernity and postmodernity, to obliterate ostensibly the possibility of adopting any coherent postmodern system of knowledge.<sup>86</sup> Like Parusnikova’s (1992) attempt to deny the possibility of a postmodern philosophy of science,

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<sup>86</sup> Cartmill’s (1991) hostile review of Haraway’s (1989) *Primate Visions: Gender, Race, and Nature in the World of Modern Science* is an example of a scientist, thoroughly immersed in modernity and epistemic sovereignty, erecting a distorted view of postmodernism and of historiography and then using it to vilify an alternative interpretation of aspects of the history of the scientist’s own field. Cartmill (1991) accuses Haraway of being “unfriendly” (p. 69) to science and claims her book is “an expression of hostility and contempt, to the scientific enterprise in general and to primatologists in particular” (p. 73). He suggests that Haraway advocates the view that “scientists are nothing but politicians and shamans, and that objective knowledge is itself a myth cooked up by scientists to protect and enhance their power” (p. 68). Cartmill’s (1991) misunderstanding of postmodernism and historiography precludes a competent grasp of what Haraway tries to do in her work: “[T]he book’s virtues are outweighed by the faults that arise from Haraway’s postmodernist epistemology. The worst of these faults is her refusal *to deal with the past on its own terms*, to give an account of people’s actions in terms of their own ideas and intentions. Because she is not really interested in the thought of the past, but only in poking holes in it to reveal the scandalous Thing lurking within, she does not hesitate to caricature it into unintelligibility, leaving out vast sectors of the primatological tradition and distorting others to make them fit her picture. This approach may be appropriate for Haraway, who believes that reality is an artifact constructed for political ends, but it makes it hard to take her seriously as a historian of ideas” (pp. 71-2, emphasis added). Clearly, Cartmill (1991) not only misunderstands Haraway’s conception of truth, he presupposes one can have access to definitive, context-transcendent interpretations of the past and of scientific evidence through which we can generate fixed, objective statements of fact.

these works are aimed more at political and academic posturing, than at careful philosophical, historical, or social argumentation.<sup>87</sup> In the next section, I begin to construct a postmodern philosophy of science by analyzing the views of a philosopher of science, Joseph Rouse, who takes many aspects of the postmodern worldview seriously. Rouse's serious philosophical work, while at times dense and challenging reading, is nevertheless work within the postmodern worldview. I use it as a starting point for constructing a postmodern philosophy of science that takes narrativity to be a fundamental component of postmodern thinking.

## **Joseph Rouse, Narrativity, and Naturalism**

### *I. Temporality*

In much of his work in the philosophy of science, Joseph Rouse has used the concept

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<sup>87</sup> It is not my intention here to give a detailed deconstruction of the arguments of Holton (1993, 1996), Gross and Levitt (1994), Marx (1994), Windschuttle (1994), Gross, Levitt and Lewis (1996), Koertge (1998), Sokal and Bricmont (1998), Stove (2001), or other recent attempts to either de-legitimize Science and Technology Studies (STS) and other fields, such as Cultural Studies and even Philosophy of Science, as interdisciplinary fields that study what scientists do, or to paint whole groups of serious intellectual thinkers as foolish and/or dangerous. I do, however, offer some observations. The fact that Sokal was successful in his attempt to get a hoax published (the "Sokal affair") in the journal *Social Text* (see Sokal and Bricmont 1998) in and of itself does not de-legitimize postmodernism. It does show that, as in the sciences, there are in the humanities broad spectrums of methods, goals, and standards; as in the sciences—for example, with the peer review process—sometimes those standards fail. As philosophy, Sokal and Bricmont's (1998) arguments against the works of some postmodern thinkers, such as Laçan, Latour, Baudrillard, Deleuze, and Guattari, are weak. Sokal and Bricmont even claim that apart from these postmodernists' "abuses" of scientific metaphors in their work, that they "admit that we do not always understand the rest of these authors' work" (p. 9). What is clear, other than these scientists' and mathematicians' misunderstandings of the motivations of these French intellectuals, is that Sokal and Bricmont are deeply entrenched in modernity with its view of science as the privileged and sovereign knowledge generator. Pitt (1997a) provides an excellent analysis of the "Science Wars" and how they bear on STS and related fields.

The same criticism falls on Stove's (2001) arguments, according to which Popper, Feyerabend, Kuhn, and most, if not all, recent philosophers of science are "irrationalist" because their arguments are "derived from Hume, and . . . the key premise of Hume's irrationalist philosophy of science is deductivism" (p. 164). They are irrationalist because they believe "that there has been no accumulation of knowledge in the last four centuries" (p. 192). Apparently, convergent realism is what Stove is trying to maintain, although he does not develop this. He concentrates on belittling those involved in the post-empiricist debates on incommensurability (e.g., Kuhn, Popper, Lakatos, Laudan, and Feyerabend) for their irrationality in not believing in the accumulation of knowledge and for "conflat[ing] the history with the philosophy of science" (p. 24). Stove's (2001) formal logic-based argument is supposed to convince us, according to the foreword by Keith Windschuttle, that the field of Science and Technology Studies has degenerated into "political demagoguery, theoretical obfuscation and plain ignorance" (p. 14), and that its debates have "constituted a veritable monument to irrationalism" (p. 17), and are the "*Origins of a Postmodern Cult*."

of narrativity as a tool for helping to explain what scientists do and how science changes. Rouse's starting point is a form of philosophical naturalism, which he develops and refines over the course of his philosophical work. With this naturalism, Rouse (1987) agrees with Heidegger ([1957] 1962) that we must begin with a realization that "[w]e always find ourselves in a world whose sense is already laid out toward concrete possibilities" (Rouse 1987, p. 62). There is no need to conclude, as in one common misreading of postmodernism, that "there is no world out there." But the presupposition that we are always in the world does *not* mean that we can take "as already determined both the way the world is and our understanding of how our interpretations take it to be" (p. 154). Instead, we should view human existence, as did Heidegger, as hermeneutical:

Our way of being-in-the-world embodies an interpretation of the world and of ourselves, which can itself be elucidated by the interpretation of our everyday practices. Thus both the attempt to disclose the meaning of our practices and the practices themselves are hermeneutical. (Rouse 1987, p. 58)

As human agents in the world, we do make choices, but we cannot choose the "field of possibilities" (p. 62) for those choices. We do not choose them because we are already in the world, and this world is not of our making. That is, "the configuration of the world" is not a matter of presupposing a set of basic beliefs we have about the world. Hence, it is

not something we have chosen and not something we can articulate. It is therefore not something we could "stand back from" and accept or reject. It is what provides us with a hold on the world, allowing us to make sense of ourselves and to encounter significant things around us. To stand back from it would be to lose our grip rather than to make our interpretations of things clear. It is not a set of beliefs or assumptions we have, but a way into the world that "has" us. Thus it is not accidental or arbitrary, not one "conceptual scheme" among others. This configuration of things is the manifestation (to us) of what it is to be. Such a configuration may change over time, but not as the result of deliberate choice or action. (Rouse 1987, p. 64)

Rouse (1987) is here emphasizing the need for rejecting a representationalist view of the hermeneutics of existence and what that means for how to conceptualize our making interpretations. As we are already in the world, it is not a matter of getting right some representation of how the world is with us. Instead, we encounter the world "unmediated by theories or hypotheses" (p. 65). What we do presuppose is a "form of life" (*ibid.*).

Moreover, in that encounter with the world, we engage in interpretation. Following Heidegger, Rouse (1987) believes that interpretation is part of our existence, “the working out of what it is to be a person here and now” (p. 65). So understanding, then, is “not a conceptualization of the world, but a performative grasp of how to cope with it” (p. 63). It is “the way one’s actual situation hangs together and makes sense as a field of possibilities for interpretation” (*ibid.*).

When we humans engage in interpretation, we are already in a world that is not of our making. Rouse (1987) believes we should adopt Heidegger’s view that understanding is more “skillful knowing our way about in the world rather than theoretical knowledge of the world” (p. 66). This is significant, because it means that we cannot remove ourselves from the world and stand above it and make it as we choose. We do not have the *power* to do so. Any attempt we make to impose a belief or value on what we do “cannot be a belief or value whose authority depends on us” (p. 67). It cannot depend on us, because what we do involves something that matters in our encounter (being in) with the world, a world in which we already exist. Hence, “everything we do is interpretive,” and “what is disclosed in our interpretations and what is at stake in them are the same” (p. 67). Our interpretations are *in* the world, not imposed on it, and they reveal “what is at stake in what we do” (*ibid.*). They reveal, we might say, why they matter; they reveal the dynamics of power and resistance in the world in that particular local context.<sup>88</sup>

In this working out of his naturalistic philosophy, Rouse (1987) plants some of the seeds of his subsequent focus on narrativity. If our taking action in the world requires having a practical understanding of what it means to take meaningful action, then this action “cannot be reduced to theoretical representation,” (p. 40) because there is something *at stake* in our taking action at a particular time in a specific situation. In the case of scientific activity, what is at stake in choosing a research program or deciding on how to solve a scientific problem will involve a number of factors. These factors might include an assessment of problems with a currently accepted theory. Nevertheless, as Rouse (1987) argues, they normally include any number of other factors that reflect a practical involvement with scientific

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<sup>88</sup> Rouse (1987) here opens the door for Foucault and his power/knowledge construct. Rouse (1993a) goes on to show how to apply power/knowledge to the natural sciences.

research and experimentation. They might include

what results are (and are not) reliable bases for further work, what tools and techniques are sufficiently precise and illuminating, what prospective achievements would constitute a significant advance and what would not. (p. 88)

Such a focus on ongoing research traditions, coupled with Rouse's emphasis on practical, directed action that *matters*, suggests that such action should be seen in its specific, local context. In this local context, the action taken will depend on what the field of possibilities is in that context. Rouse (1990) later calls this local context, in which meaningful action can be taken, a *narrative* context.

In his discussion of scientific research activity, Rouse (1987) uses Heidegger's concept of *Umsicht*, or "circumspective concern," (p. 38) when describing how a scientist assesses the possibilities for research activity. Although Rouse only mentions *Umsicht* in two of his works, it turns out that this concept is crucial to his insistence that we should approach any interpretation of the practice of science from a hermeneutically naturalistic position. For Rouse (1987), any assessment of how and why scientists choose particular research programs should be one in which scientists are taken to be agents already situated in the world, in specific local contexts at particular times (ch. 6). This position is in explicit contradistinction to the view taken by most traditional philosophers of science (and others), according to which scientists adopt a sovereign viewpoint and philosophically probe the current state of theory in their field to make a determination of what are the salient problems or unknowns that cry out for experimental work. *Umsicht*, as Rouse (1987) applies it to scientific practice, is then *not* the adoption of an epistemologically sovereign viewpoint with which scientists can remove themselves from the local context (narrative context) in which they find themselves confronting the world, in order to solve the next obvious scientific problem in the straight and rational road toward truth. It is instead the context-bound activity scientists engage in when they formulate "a practical assessment of what it makes sense to do, given the resources available and the aims and standards that govern scientific practice within a field" (p. 88). These decisions involve an engagement with the realities of the local context and a directedness toward future results, even when they involve an assessment of previous scientific results. That is, when scientists evaluate previous results in their field,

they might accept or reject them, or interpret them in a novel way—in a way that could, for example, *change* what constitutes the “background knowledge” of the local context. In any event, Rouse’s (1987) main point is to emphasize his view of scientific activity as context-bound and dependent on “practical, engaged, and local” (p. 93) assessments, and not on contextless or (fixed and final) objective evaluations.

With Rouse’s (1987) utilization of Heidegger’s *Umsicht*, or circumspective concern, in evaluating how scientists make research decisions that are locally situated in specific contexts, we see the emergence of his subsequent focus on *narrativity*. If the circumspective concern of scientists must be conceptualized in a way that requires it to be irreducibly bound to a local context, and not to some sovereign, epistemically privileged and rational overlay, then we may see it as belonging to a *narrative context*. Moreover, treating science and the practices of scientists as belonging to narrative contexts that are fundamentally local and antithetical to epistemic sovereignty amounts to a significant shift from the historical trajectory of the philosophy of science. Rather than reason, rationality, realism, truth, objectivity, or a host of other constructs that might preserve the epistemic sovereignty of science, Rouse chose to reject those and instead focus on a hermeneutical naturalism that led him to consider how narrativity might play a role in the philosophy of science. What, then, is the need or the benefit of taking seriously the cognitive/cultural significance of narratives?

In reconceptualizing science as an activity that scientists do in the world, rather than a collection of representations that are open to logical, empirical, and/or problem-solving analyses from an epistemically sovereign perspective, Rouse (1987) places a primary emphasis on how to make sense of a scientist’s decision to adopt a particular research trajectory. Again, he uses Heidegger’s *Umsicht* to conceptualize how scientists make practical, engaged, and local decisions that are irreducibly bound to the time, place, and specific realities (material and discursive) of the moment (the ubiquitous *present*). In addition, he utilizes Foucault’s analyses of governmentality and power/knowledge to show how that specific *present* (in the sense of the here and now) can be conceptualized as a “field of action” that is “constituted both by material surroundings and technical capabilities and by the shared understanding of what it makes sense to do and to be in those surroundings” (p.



185). It is this “field of action” or “field of possibilities” (p. 62) for taking meaningful action in an already-configured world that led Rouse (1990) to view this “practical understanding” as having a narrative form (p. 182).

That is, to the extent that the circumspective concern of *Umsicht* embodies or constitutes the locus for identifying how and why scientific activity is significant, then such activity has the form of a narrative or a field (or set) of narratives. For Rouse (1990), scientists’ practical understandings of their field of possibilities for meaningful action constrain and help to constitute the very “intelligibility, significance, and justification of these activities and their products. . .” (p. 182). In this sense, then, this practical understanding has a narrative structure. It is a narrative structure, because the meaning assigned by the agent in the taking of the action will depend on the shared (yet normally contested) practical understanding of the research situation. In the sense that the shared understanding gives meaning to an event or a series of events, it serves a function at least analogous to literary or historical narratives. Literary and historical narratives, as I argued in Chapter II, function to give meaning to events by presenting them in a particular form. Again, a narrative is a meaning-generating device for making sense of a series of events and actions; without the *form*, the meaning is absent. So one benefit of viewing scientific activity as having a narrative form—the form of a story—is that the *meaning* of that activity in question becomes intelligible and coherent. However, as particular scientific events of the past are now to be considered historical narratives, there again arises the tension between viewing narratives as ongoing narratives *in medias res*, on the one hand, and narratives as structures *imposed* on events and actions of the past in order to give them meaning, on the other hand. Can this tension be resolved?

Rouse (1990) does not want us to view a narrative as “a scheme imposed from without on an unnarrativized sequence of happenings” (p. 181). We should, instead, view a narrative field as a set of ongoing stories in which we live. Our belonging to these ongoing stories is what provides “the intelligibility of action, and of the things we encounter or use in acting” (*ibid.*) and thus provides them with meaning. Rouse explicitly distances his conceptualization of narrative from the narrative of the historian of science. That is, his

narrative is *not*

quite the same as the story which might be later told by an historian of science. Scientists are situated differently from the historian because they see themselves as agents within an *unfolding* story, and consequently, that story is configured somewhat differently for them. (p. 191)

Scientists, or any actors in the present, on the one hand, are situated differently from the historian, precisely because they are “in the midst,” as Kermode (1967) put it. Historians, on the other hand, are trying to reconstruct particular episodes in the history of science. However, Rouse’s (1990) argument regarding narrative structure seems not to adequately problematize temporality. I am now in the *present*, in the here-and-now. But when I attempt to conceptualize, as does the historian, why a scientist pursued a particular research program (that is, a scientist in the *past*), I am still in the present, but the scientist and the events in question are separated from me temporally. Therefore, on what basis does Rouse (1990) claim to know anything about the ongoing narratives to which scientists of the past belonged? Is not the primary evidence for making the very claim that scientific activity is guided by the circumspective concern of *Umsicht* a thorough knowledge of the history of science? And if so, are we not led back to the problem of *how* to construct those historical narratives?

Rouse may claim that the basis for his naturalism is that all humans are already in the world with its already-determined field of possibilities. This premise is not the issue. However, if we are to say anything specific about particular historical episodes in the history of science, if we are to say anything beyond our own situation in the present, we need evidence—historical evidence. And while I, as an historian, am still context-bound in my own set of narratives, it still remains that if I am to say anything about another local, context-bound episode in the history of science, I must construct a narrative that amounts to taking a set of events from the past and imposing a narrative structure on them. Just as scientists attempt (among many other things) to represent the natural world as they believe it to *be*, historians attempt to represent the *past* as they believe it to *have been*. Whatever form of naturalism we want to preserve—whether it be Rouse’s hermeneutical naturalism, the pragmatic naturalism of historiographers such as Kermode (1967) or White (1981a, 1987), or

the phenomenological naturalism of Ricoeur (1981)—it remains inescapable that historians interpret (yes, from within their own field of narratives) the past, and they do this by constructing a representation of the past in the form of an historical narrative. Indeed, this activity is situated differently from what scientists do; it may even be the case that the historian, in accounting for why a scientist pursued a particular research program, will contradict the scientist's recollection of the events or even the scientist's reasons given at the time (as interpreted from the historical record). Nevertheless, there is still a sense in which scientists themselves must interpret historically in order to make sense of their *present*, which is at least in part the basis for any action that is an attempt to create a *future*. But in what sense?

The sense in which scientists, as part of their activity as scientists, must interpret historically involves Heidegger's *Umsicht* and *Geschichtlichkeit*, as interpreted by Rouse (1987) and Ricoeur (1981). To illustrate this, grant Rouse's naturalism and his view of scientific activity as belonging to a field of (or set of interrelated) narratives. Let us also maintain that the activity of a scientist is context-bound in the sense that the local and temporal particulars are what drive the narrative into the future, and not some overall global narrative structure or disembodied epistemic privilege of the scientific activity. Moreover, let us grant that what gives meaning to the actions taken by the scientist at the time in the particularly (and irreducibly) local situation is, as Rouse claims, the field of possibilities (i.e., the particular narrative field) as given by the world in which the scientist finds him/herself. Having granted all this, do not the constructs of *Umsicht* and *Geschichtlichkeit* involve, at least in part, an assessment, while taking place in the present, of the *past* and how it has gotten one to this point in the present? *Umsicht*, or circumspective concern, while it is not, for the scientist, a comprehensive theoretical understanding of the current state of knowledge in one's field, but instead a *practical* understanding of how to proceed, nevertheless involves *some* historical understanding of how one's practices got to where they are. Indeed, as Rouse (1990) indicates, scientific literature is rendered obsolete rather quickly (pp. 190-1); we might then view *Umsicht* as involving an assessment of the *recent* history of the narrative(s) in which the scientist finds him/herself.

For example, imagine a research scientist is working with a team of scientists at the university, who are trying to develop a vaccine for HIV. This scientist has some conception of why she is engaged in this work; she knows something about the history of the disease, why it is a problem, and what methods have been used to study it. She also knows something about at least the recent state of her own science, including what particular problems with retroviruses must be overcome in order to develop a successful vaccine. In addition, she has some knowledge of her own group of researchers, why they are attempting to tackle this set of problems at this time, what techniques will be used, what sources of funding they have, who is the head of the group and why, and so forth. This perspective contributes, in some way, to how she and her colleagues will approach the problem of HIV and to how they will conduct their experiments and conduct their clinical trials. This is the sense of *Umsicht*, borrowed from Heidegger, that Rouse employs.

Similarly, *Geschichtlichkeit* (historicality), or the “weight of the past” (Ricoeur 1981, p. 167), involves a more retrospective look at one’s past in the sense of a shared understanding, tradition, or common experience. For Ricoeur (1981), it is this temporal aspect of narrativity that not only establishes “humanity, along with human actions and passions, ‘in’ time;” it also “brings us back from within-time-ness to historicality, from ‘reckoning with’ time to ‘recollecting’ it” (p. 174). In other words, it allows us not only to see ourselves as living in more than just a chronological series of events; *Geschichtlichkeit* also allows us to transition from the here-and-now of the present (the fundamental presupposition of naturalism that we are already in the world) to the interpretive recollection of our shared past. As such, it amounts to an interpretive historical assessment of one’s “present,” one which allows one to say *why* one is performing, or about to perform, the practices in which s/he is engaged.

For example, our HIV researcher sees herself as belonging to a tradition of biological research that goes back to the critical efforts of scientists such as Jenner, Pasteur, Salk, Milstein, and Kohler. She views her life as having meaning that is partly generated by reference to these people, their past efforts, the institutions and practices subsequently built on their efforts, and the ongoing efforts of scientists in many allied fields who try to study the

biology of viruses and other organisms, and who devote their lives to eliminating the human suffering caused by infectious diseases. She may not have a detailed historical perspective of the intricacies of the history of biology; indeed, her main source of historical knowledge might be her education, textbooks, and interactions with her colleagues, in addition to what she learned growing up as a young girl and then as a student interested in science.

Nevertheless, her participation in her culture as a citizen who is informed, to at least some extent, provides some of her perspective on the past traditions, problems, and issues in which she is currently participating through her actions in continuing and reconfiguring those traditions. To the extent that this characteristic of narrativity—that is, one’s sense of belonging to a story that is rooted in the past, or one’s sense of personal fate and common destiny—is essential to one’s maintaining a hold on what it *means* to belong in a particular field of narratives at a particular time, then we can say that such a meaning-generating activity involves an historical assessment of what it means to live in the present. Hence, we can say that even a scientist bound to a local field of narrative contexts must interpret historically to make sense of her present situation.

Clearly, the scientist’s interpretation is itself embedded in a local context and is an interpretation for some purpose, for something else that *matters*. Nevertheless, it is an interpretation that involves, at least in part, an assessment of the *past*. Therefore, it amounts to telling or believing a story that has a narrative form, that is, a beginning, a middle, and an end. The scientist in the local context belongs to a field of narratives—that is, a set of interconnected stories that are still unfolding, that have not yet ended—but the action of interpreting the past nevertheless amounts to constructing a narrative (or narratives) in the sense of a completed story (yet one which will surely change in the future). The main distinction, then, between the scientist *in medias res* and the historian is *temporal separation* (access to historical evidence and professional training would be two others). The historian has access to the *end* of (at least one of) the narratives to which the scientist belonged. Hence, the scientist and the historian are primarily *temporally* separated. In addition, even if we want to reject aspects of Heidegger’s, Ricoeur’s, or anyone else’s existential claims regarding being or temporality, it still remains that much of the *evidence* needed to argue for

or against them will come from *history*. And to do history we must construct narratives (History-3s) that impose interpretations on the events of the past (History-2), and those interpretations will involve “regulative fictions” (Kermode 1967).

## II. *The Real*

Rouse’s view of reality flows from his views on naturalism, narrativity, and from the postmodern principles that embody them. To begin to unravel his position on the real, we may begin with his article on Fine’s “natural ontological attitude (NOA)” (e.g., 1984, 1986, 1996). In this article, Rouse (1991c) presents different interpretations of NOA in an attempt to distinguish Fine’s position from traditional philosophy of science and from social constructivism, and to show its affinity for certain strands of postmodern feminism. Rouse (1991c) suggests NOA should be seen as an “attitude toward science . . . which is supposed to remove any felt need for a unified philosophical interpretation of science” (p. 610). We should instead “take scientific claims on their own terms, with no felt need to provide any further interpretation” (p. 611). We should abandon any attempts to construct a global, context-independent narrative or epistemic structure for science, and view scientific practice as local and context-bound. Realists *and* social constructivists (and throw in pragmatists and empiricists), who are supposed to be enemies in the philosophical game of interpreting science, are for Fine and Rouse, playing a game with similar premises: science has a global philosophical structure, and it is the job of the philosopher of science (or other analyst) to find the correct interpretation of this structure. The realists want to reduce science to the successful correspondence of scientific theories to the truth of the way the world is; empiricists to rational empirical adequacy; pragmatists to some notion of successful methodology or practice; and constructivists to context-transcendent principles of social reality. Instead, the postmodern philosopher of science should, at least as a starting point, “take scientific claims on their own terms” (p. 611) and reject any ready-made interpretations of how science is supposed to work. But what does this say about *reality*?

Rouse’s (1991c) interpretation of Fine suggests that reality is what scientists say it is. However, any such determination of the structure of reality, whether by a scientist or by a

philosopher of science, must be considered to be anti-essentialist, revisable, and the locus of “interpretative dispute” (p. 624). Furthermore, any interpretation by a postmodern philosopher of science should be one made from a perspective that does not “[attach] science to a ready-made philosophical engine” (p. 625). Rouse believes this undercuts the realist/anti-realist debates by showing that either pole of this dichotomy will not do. That is, for example, both the realists’ correspondence to the “actual” structure of reality, on the one hand, and the constructivists’ assumption that scientific reality is reducible to sociological “interests” or politics, on the other hand, are both to be rejected, as they are perspectives that proceed from “ready-made philosophical engines.” The postmodern philosopher must instead accept “that the contingencies of history give us all the resources necessary to tell us what we need to know about science” (p. 624). The grounds for interpretation are then not correspondence, epistemic privilege, or any other pre-fabricated essentialist philosophical position. What they require is “the appeal to reasonable judgement in specific historical contexts. . .” (p. 625). While this appeal is “not entirely unconstrained” (*ibid.*) in that it must flow from an interpretation of the particulars of the specific local context under scrutiny, it is nevertheless not an appeal to necessity or any final, objective (definitively fixed) constructs.

Rouse’s (1991c) interpretation of Fine seems to accord well with Lyotard’s ([1979] 1984) “incredulity toward metanarratives,” according to which we should reject essentializing or totalizing metanarratives as constructs for guiding the history and philosophy of science. Rouse (1991a) believes that too many historians and philosophers have adhered (not necessarily consciously) to the strictures of modernity. Instead, they should start with a conscious resistance to imposing modern metanarratives onto history and look to the narrative contexts of history and their local, context-bound concerns. Moreover, this interpretation seems to suggest that the philosophy of science is dependent on the history of science in a significant way. That is, any interpretations of how science is (or was) and why particular developments happened must be described as contingent historical events. As such, they are then subject to the deconstruction of historiography I presented in Chapter II. Indeed, Rouse (1991c) interprets Fine as accepting that past developments in science are subject to “epistemic and political interpretation” (p. 625) that can “raise and begin to answer

critical questions about the cultural and political significance of the sciences” (p. 626).

Rouse’s (and Fine’s) view is not an anti-interpretive stance, devoid of any means for criticism, as is suggested by a common misreading of the postmodern position. It is instead a plea for the construction of particular types of historical narratives about the history of science, even if that focus is undeveloped in Rouse’s (1991c) interpretation of Fine and in Fine’s explications of his philosophy of science. What is needed that goes beyond Rouse and Fine is a specification for how to go about constructing narratives that moves beyond the rejection of totalizing metanarratives. In addition, what is needed is a view of reality that eschews the totalization of reality, but that at the same time remains faithful to the “reality” of the past local context—that is, the narrative context. In other words, what is needed is a way of constructing narratives that tells a story about the past that reflects the past *wie es eigentlich gewesen ist*, to the extent this is possible in practice. In Chapter II, I deconstructed the extent to which it is possible to present history “as it actually happened.” That amounted to a deconstruction of historiography and of narrativity. The tension between narratives as ongoing, unfolding real-world contexts, on the one hand, and narratives as interpretative structures imposed on past events, on the other hand, remains unresolved.

### III. *Causality*

Rouse’s view on causality does not fully take shape until his views on narrativity and naturalism take on a more systematic philosophical form. Rouse (1996b) extends his conception of narrativity and claims “that the significance of scientific work is situated within narratives that are not merely retrospective but are also constitutive of its sense” (p. 170). The significance of scientific work, then, “must be distinct from the work’s conformity to prevailing assumptions, standards, and values within any particular research community” (*ibid.*). This distinction must be made, because the view that scientific work is made significant by its consistency (in some way) to (relatively) fixed and shared community standards, cannot coherently handle scientific work “that transforms a community’s prior commitments or changes what counts as the relevant scientific community” (*ibid.*). We must instead see scientific work as situated within ongoing narrative fields that not only constitute



a “collective enterprise” of research activity—the narratives themselves also “constitute a developing field of knowledge” (*ibid.*) and further provide the significance of the scientific work. The significance is not meaningful independent from its belonging to the ongoing, changing narrative field. The significance, hence, does not consist in an epistemic appeal to background knowledge. In addition, it does not consist in any fixed, definitive relationship between the deployment of power relations and the background social context. It is narrative bound, and requires coherent views of the actions of agents, how and why they have meaning, and how to make sense of the relationships among the various agents. This suggests that some account of the patterns of actions of the agents should be generated. That is, some account of *causality* is needed to make sense of the patterns of agency within the various ongoing, and possibly intersecting, narrative fields.

Rouse (1987) believes causal explanations are important for understanding scientific activity because they provide “a basis for experimental (and of course technological) intervention” (p. 189). Without a coherent view of causality, how are we to make sense of scientific experimentation or its technological infrastructure? Rouse accepts the view that causality hinges on a counterfactual argument involving human agency. That is,

the significance of causal explanation instead of mere correlation derives from its relation to possible intervention by manipulating the relevant causes. [In addition], our conception of causal connection depends upon how we understand agency. The counterfactual import of causal claims is justified by the possibility of our intervening to make the cause of an event present where it was not before, or to remove it. (p. 189)

However, Rouse also believes that there are differing conceptions of causality and of agency.<sup>89</sup> Moreover, a scientist’s view of causality will have bearing on the type of

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<sup>89</sup> A traditional problem in the philosophy of science has been to specify an adequate conception of causality (see, for example, Woodward 2003b). Since the questioning of the deductive-nomological method, exemplified by Hempel (1965b), in adequately modeling scientific reasoning or activity, many philosophers have probed the limits of an inductive, or inductive-statistical, method in specifying adequate accounts of causality, explanation, and scientific law, and their possible relationships to agency and intentionality. See, for example, works by Goodman (1947, 1955), Hanson (1958, ch. III), Hempel (1965a), and more recently, van Fraassen (1989), Cartwright (1983, 1989), and Salmon (1998), to name a few. Clearly, there are a variety of positions held, ranging from nominalist to realist, empiricist to idealist. Regarding counterfactuals, philosophers again hold a plethora of positions and do not even agree on whether they are licensed at all, for a variety of reasons.

For example, Cartwright (1989), one of the “New Experimentalists,” argues for the reality of capacities

experimental research s/he will engage in (pp. 189-91). Hence, agency and causality should be seen as interdependent and inextricably bound up with issues of power and politics (chs. 6, 7). And Foucault's analyses of power/knowledge are a useful, although not necessary, way to for us to see "why we should describe laboratory practices and their extension as embodying *power* relations" (p. 244).

Rouse (1996b) believes that a "relationship between two agents is a power relationship only because and to the extent that other agents will normally respond to the two agents' actions in ways that are coherently aligned with the dominant agent's actions" (p. 182). One example Rouse uses involves what happens when teachers assign students grades. A teacher exercises power when *other* agents "align" their actions with respect to what

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and perhaps causes, and against laws; she believes that "[s]pecific combinations of causes do not stay fixed long enough to produce the data necessary for a good induction." The "conventional answer to this kind of problem," she continues, "is the resort to counterfactuals: the requisite regularities may not in fact hold, but they are true counterfactually . . . [;] they hold '*ceteribus paribus*'. That means they would hold *if* all disturbing causes were absent. But that will not do. . . . Even if these regularities did hold *ceteribus paribus*—or, other things being *equal*—that would have no bearing on the far more common case where other things are *not* equal" (p. 177). Contrast this with Woodward's (2003b) manipulability notion of causal explanation, which is based wholly on the concepts of intervening (at least in principle) in nature and counterfactual argument, yet also rejects laws: "[M]y idea is that one ought to be able to associate with any successful explanation a hypothetical or counterfactual experiment that shows us that and how manipulation of the factors mentioned in the explanation . . . would be a way of manipulating or altering the phenomenon explained" (p. 11). His notion of "intervention" he considers a "regulative ideal" that specifies "what must be true of the relationship between X and Y if X causes Y and in this way tell[s] us what we should aim at establishing, perhaps on the basis of nonexperimental evidence or on the basis of an imperfect or nonideal experiment, if we want to show that a causal claim is true" (p. 114).

Rouse's (2002a) position on causality, explained in more detail below, rejects both Cartwright's and Woodward's conceptions on the grounds that they are both *agency* accounts. Rouse (2002a) rejects attempts to ground "causal talk solely in capacities to bring about effects; such capacities are no more basic than our understanding of ourselves as causally affected, and hence as vulnerable to being marked by intra-action with other causal agencies. Agency accounts of causality hence do not take seriously causal *intra-action*" (p. 314). Rouse also rejects regulist notions of causality: "Regulists differ from Humeans [or regularists] only by substituting [for exhibited rules for regular occurrence] instead a *governing* rule or law, whose necessity is merely exemplified by any actual sequences" (pp. 314-15). Rouse (2002a) instead takes causal intra-action in the world to be primary: "I take the intra-active phenomena that constitute causal relations as the basis for understanding intentionality. As agents, we are already situated in the midst of ongoing patterns of causal intra-action with our surroundings, which provide the basis for explicating agency. . . . 'We' are not agents apart from the phenomena in which we participate . . . , any more than are the objects whose causal capacities or behavioral regularities are invoked by anormative naturalists" (p. 314). His account of causality is not reductionist or emergentist; it is expressivist: "[M]ine is emphatically not a reductionist strategy of showing how intentionality and the conceptual content of scientific talk can be reduced to or explained in terms of causal relations. . . . I show how correct application of the concept of causal [intra-]action already involves conceptual/discursive normativity all the way down. The result is not then to reduce causal relations to intentional ones, but to show the inseparability of material and discursive interactions with the world" (p. 270). Narrativity, however, is crucial to *carrying out* Rouse's plan.

grades the teacher has given. This political alignment is not, according to Rouse, “simply a causal consequence of a discrete action by the dominant agent, but instead makes up an ongoing relationship within which each agent’s actions are situated” (pp. 182-3). Again, we can see this ongoing relationship as belonging to a narrative context. Moreover, in accordance with his view of naturalism, Rouse does not view intentionality as primary. That is, while the teacher may have *intended* to prevent a student from getting a job by assigning a poor grade, other agents’ actions may turn out to be aligned in ways that defy or change the original intention of the dominant agent. What is primary is what the other agents *do* in response to the dominant agent.

For Rouse (1996b), then, intentionality is involved in power relationships but does not govern them; we need no reduction to the mental states of agents. In addition, as the future actions of agents are important in this analysis, the need for narrativity is again emphasized. As for the natural sciences, Rouse introduces the term “epistemic alignment” (p. 188) as an extension of political alignment. This concept is meant to illustrate how the natural world—“materials, things, processes, and practices” (p. 191)—can mediate power, and how there can be “resistance” to epistemic power in much the same way there is resistance to social power. That is, in the sense that things “break down, are unavailable when needed, convey confusing signals, and sometimes even get in the way” (p. 190), the natural world can be said to offer resistance to the epistemic power relationships they mediate. Hence, a view of how the natural world is intermingled with human actions and human agency in an ongoing narrative field (a set of interweaving narratives) will have to make sense of causality in a coherent fashion.

Rouse (2002a) presents a view of scientific practices that makes “causal intra-action in the world” the primary philosophical concept, rather than causal or nomological necessity (see esp. chs. 8-9). Here we have a view of causality that we may take to be postmodern. In his analysis, Rouse carefully shows the reader how his philosophical system differs from other attempts in the philosophy of science, and he provides reasons why he believes his system is superior. Rouse’s main goal is to provide an account of science and scientific knowledge as practices situated in the world. Scientific practices should be seen as

“themselves causal intra-actions that disclose nature as consisting of causally intra-active phenomena” (p. 311). Rather than representing the world from a disembodied viewpoint that stands outside of it, scientific practices are always already in the world, and they are governed by *stakes*— what matters in a given context at a given time—that are “normatively binding upon [those] practices” (p. 342). The normative force of scientific understanding (what scientific practices disclose to us) is expressed by the irreducible modality Rouse calls *real possibility*, and not by nomological necessity (p. 331). That is, in accounting for scientific practices and their temporal developments, we should see them as governed by possibilities that are real in the sense that they are embedded in the actual (present) context and serve to transform the practice(s) of that context (pp. 337-8). Hence, scientific practices are “constitutively normative” (p. 181), but those norms “cannot be bindingly authoritative” because “what is at stake in practices outruns any present articulation of those stakes” (p. 25). While there is *no* “final accounting of the source and ground of normative authority” (p. 351), its source and ground is nevertheless “*this* world, the open-ended and ongoing patterns of causal intra-action in which we always find ourselves, or . . . our *belonging* to the world intra-actively” (p. 355).

Rouse (2002a) gives priority to causal phenomena rather than to any regularities or laws by which those phenomena are ostensibly governed. So Rouse’s naturalism starts with causal phenomena in the world; then, scientific practices, the interpretation of meaning, and the assessment of truth are to be seen as themselves causal phenomena (p. 23). In addition, Rouse gives ontological priority to *phenomena* in the world, rather than to any individual objects. Objects must be seen as “embedded in patterns of intra-action” (p. 314). There is, for Rouse, no *a priori nature* of nature, and objects should be seen “only as practically constituted components of repeatable phenomena” (p. 313). Scientific practices, then, are “constitutively normative,” because “what science or knowledge *is* is not already determined, but is instead at issue in scientists’ and others’ practical responsiveness to their circumstances” (p. 181). Hence, causality and normativity are “intra-twined” in a way that undermines the nature/normativity dualism that has been so pervasive in the philosophy of science: “Causality must be understood as always already normative, and normativity as

always already causally efficacious” (p. 183). That is, the material and discursive “worlds” are not separate worlds with, for example, human norms imposed on an anormative material world. So, causality and normativity are not “inter-twined,” as that implies an interrelation but also separability. They are instead “inter-twined;” one cannot have or understand one without the other.

In his philosophical system, Rouse (2002a) presents a dynamic view of knowledge without reifying it. Rather than fixating on “background knowledge” and its role in making connections between hypotheses and evidence, Rouse develops a dynamic, not static, view of knowing. This view rejects the notion that there is such a thing as scientific knowledge as “theoretically coherent and surveyable as a whole” (p. 179). That is, knowledge and scientific knowledge do not constitute a “theoretically coherent kind” (*ibid.*). Again, this conception of knowledge and of science departs from traditional ways of capturing the significance of science, and its rejection of epistemic sovereignty and dissolution of several dualisms (representationalism/performativity, nature/normativity), among other things, makes it postmodern. As Rouse notes,

participation in the wholesale legitimation or critique of scientific claims to knowledge that has characterized much of late twentieth-century philosophy and sociology of science almost invariably proceeds from a conception of scientific knowledge as theoretically coherent and surveyable as a whole. It only makes sense to claim that scientific knowledge as a whole is approximately true, rationally arrived at, socially constructed, or interest-relative if there is such a (kind of) thing. (p. 179)

Knowing, then, is not merely a matter of comparing knowledge claims to some static view of accepted background knowledge. Such a static view of science cannot adequately account for developments that transform what is (was) currently (previously) accepted. What is *at stake* must be made sense of, and to do this we must view knowing as mediated by practices, and we must view nature (in the sense of causally intra-active phenomena) as irreducibly normative. Scientific practices are normative; such normativity is located not outside the world but *in medias res* (pp. 72-3). As Rouse put it:

Knowing is mediated not just by a background of assertional commitments, but also by models, skills, instruments, standardized materials and phenomena, and situated interactions among knowers, in short, by practices. Moreover, a dynamic account of language as discursive practices obliterates any clear distinction between the

representational content of knowledge and its material or social construction or implementation. (p. 178-9)

In this account of causality, Rouse (2002a) develops a rather comprehensive philosophical system to account for how to conceptualize scientific knowledge and its development over time. Again, this system rejects many of the dualisms of traditional philosophy of science, and in this sense, it is postmodern. Moreover, Rouse also situates his view as distinct in important ways from more contemporary views in science studies, particularly the sociology of scientific knowledge (pp. 174-183). What remains to be analyzed is how far Rouse has departed from his earlier views on narrativity, and to what degree can Rouse's system be seen as compatible with the postmodern historiography I developed in Chapter II. In the next section, I analyze Rouse's postmodern philosophy of science and argue that it does not fully consider or adequately problematize some of the themes of postmodern historiography.

### **Joseph Rouse, Narrativity, and Historiography**

Rouse's (2002a) argument for his philosophical naturalism, culminating in his explication of the primacy of normative causal intra-action in the world—with *real possibility* as the irreducible modality that expresses the authority of the normative force of understanding, including scientific understanding—is dependent on some crucial assumptions. One such assumption involves the “problem of manifest necessity” (ch. 1). In his discussion of the differences between how Heidegger and Neurath took up the problem, compared to how Husserl and Carnap grappled with it, Rouse argues that the former two philosophers took the turn to naturalism, while the latter retained positions with necessary structures in the world. The problem, as Rouse sees it, involves “whether the manifestation of necessary structures within the material-historical world is itself a necessary structure or a material-historical contingency” (p. 72). Rouse adopts the latter viewpoint, as do Heidegger and Neurath, for which the “locus of the normativity of . . . concrete, historical-material activity . . . shifted from the necessity displayed by their atemporal structure to their

contingently situated futural temporality” (*ibid.*). We must conceive of science not from a disembodied viewpoint (narrative in the traditional sense with a disembodied narrator who retains or imposes normative authority), but instead as a set of practices situated *in medias res* (narrative in the modernist literary sense, with normativity situated in the world). This brings us back to a discussion of historiography.

We see here a significant parallel to the efforts of the classic historiographers, Kermode (1967) and White (1981a, 1987), that I considered in Chapter II. What Rouse and the historiographers have in common is the desire to tell a compelling *story* that can make sense of the tension between narratives as atemporal structures with a beginning, middle, and end, on the one hand, and narratives as ongoing fields of possibilities that are oriented toward the future, on the other hand. What is interesting to note, however, is that Rouse (2002a) does not mention the word ‘narrative’ even once in his comprehensive presentation of his radical naturalism. Indeed, Rouse makes little, if any, effort to link his new naturalism to his earlier work on narrativity. The problem with this is that narrativity is not adequately deconstructed. How do we ultimately make sense of Rouse’s efforts to account for scientific practices and scientific understanding?

In particular, to the extent that humans (in social groups, organizations, etc. and as individuals) use and are confronted by forms of historical understanding and interpretation, these forms of understanding will be bound up with normative accountability. We can make *sense* of normative accountability in general from a realization that we are situated in the world (or, alternatively, narrative bound), but how do we specify *which* norms were operating in a given local situation at a particular time (in the past)? Can we *only* attempt to specify the normative accountability for our own situatedness, here in the present? If not, if we are permitted to account for the normative accountability of other situations (*not* in the here and now), we are doing, at least in part, what historians and historiographers call engaging in historical interpretation. Yes, any attempt to do so must *itself* be seen as narrative bound, or for Rouse (2002a), as another instance of causal intra-action in the world. And that attempt will itself be “answerable to norms of correctness or incorrectness” (p. 169), even if those norms “cannot be established with finality” (p. 26); cannot “be reduced to

underlying regularities of performance or belief” (*ibid.*); and “cannot be bindingly authoritative” (p. 25). However, any attempts at historical interpretation involve constructing narratives—telling stories. In addition, to the extent that those stories are narratives in the traditional sense, with a beginning, middle, and end—then those stories are subject to the deconstruction of historiography I presented in Chapter II. The *activity* of specifying instances of normative accountability in the past must itself be deconstructed and reflexively situated within a narrative context.

In his discussion of Heidegger’s and Neurath’s radical naturalism, Rouse (2002a) emphasizes how each, in his account of scientific understanding, rejected the need for positing necessary structures in the world. According to Rouse, they

abandoned the effort to discover a supposedly necessary structure governing the normativity of our scientific and practical engagement with the world. They instead sought to understand how one’s practical situatedness in the world, in all its contingency and concreteness, could nevertheless make intelligible a practical normative accountability to something at stake in that situation. (p. 75)

For Rouse, *real possibility* serves as the concept that captures how to make sense of the contingency of events (in the here and now), and we may infer, of the contingency of history (events in the past). The events of history, while not present to us in the same way that we are now situated in the present world, nevertheless are (were) contingent events subject to the naturalistic strictures of the here and now, if we are to take Rouse’s naturalistic system seriously. But how do we determine, how do we *know*, what the *real possibilities* were in the past?

Rouse (2002a) is not entirely clear on this point, although he does make it clear that he wants to avoid conceptualizing science “retrospectively” in the sense that science supposedly tells us “what is (already) known about what the world (already) is” (p. 26). Instead, science should be seen “first and foremost [as] research, an activity that transcends present understanding” (*ibid.*). The notion that scientific activity transcends the present is supposed to indicate how the ongoing narrative (narrative as present, ongoing, and in the world) cannot be fully articulated until the narrative has played out (to some degree) some time in the future. Rouse is clear in indicating that the normative authority in a particular



situation (i.e., narrative) need not be clearly identifiable to all or perhaps any of the people involved at the time, nor is any ultimate agreement required on what those norms are (were) (pp. 338-44). The norms involved may be explicable only in the future, at which time the norms may be reconstructed (narrative as completed story about the past). Indeed, Rouse seems to realize this tension between these two conceptions of narrative when he states that “to arrive at an assessment is to transform the situation, opening newly possible ways to respond to it, with something further at stake in those responses” (p. 354). Moreover, he seems to have a grasp on the temporal complexities involved in these historiographical conundrums. However, his analysis of these, which he derives at least in part from Rheinberger (1992, 1994, 1997), to the extent that it incorporates an historiographical perspective, is brief. Rouse (2002a) notes that “discursive articulations of a situation belong to the situation they characterize, and to the extent that such formulations matter (make a difference), they change the situation and its stakes” (p. 355). The rest is relegated to a footnote:

Such transformations are also retroactive, however, so that one cannot ascribe a determinate normative authority to past circumstances while denying that it is any longer applicable. A situation is temporally open-ended in both directions—it incorporates its own past and future (including how its future will retroactively incorporate it as past, and [how] its past prospectively but indefinitely incorporated it as future). The temporal interconnectedness of causal intra-action is crucial to understanding how a normative reach always exceeds its grasp, in the sense that the authority (the stakes) to which it is accountable always outruns present understanding (indeed, outruns the present *possibilities* for understanding). Present action is always accountable to a not-yet-determinate future. (p. 355, fn. 62)

In this passage, Rouse is articulating the tension between ongoing action in the present world, on the one hand, and an assessment of a past situation (which is itself an action), on the other hand. However, the ramifications of this tension Rouse does not further develop. For example, when Rouse (2002a) asks whether particular discursive articulations matter (in the present), he seems to not fully realize that when he says a situation “incorporates its own past and future” (*ibid.*), it follows that *how* that past is constructed *itself matters*. For the historian, it involves historiographical inquiry. For the scientist, it involves *Umsicht*, or a practical, circumspective concern for how to proceed in present research given

the available situation. For humans in general, it involves *Geschichtlichkeit*, (historicality: the weight of the past, fate, or common destiny), if we accept Ricoeur's (1981) Heideggerian analysis of the epistemology of time and narrativity. For Ricoeur (1981), the function of at least some narratives (stories) "is to establish human action at the level of authentic historicality, that is, of repetition" (pp. 179-80). What he is suggesting is that creating narratives (stories about the past with a plot, i.e., a narrative structure) is what establishes past events as memories, and having memories—telling or repeating stories—amounts to placing past events in the form of a narrative with a beginning and an end. According to Ricoeur (1981),

[b]y reading the end into the beginning and the beginning into the end, we learn to read time backward, as the recapitulation of the initial conditions of a course of action in its terminal consequences. In this way, the plot does not merely establish human action "in" time, it also establishes it in memory. And memory in turn repeats—recollects—the course of events according to an order that is the counterpart of the stretching-along of time between a beginning and an end. (p. 179)

The significance of this ostensibly reciprocal relationship between temporality and narrativity is that to the extent the past is discursively articulated in the present in an attempt to create a future, it *matters* how that past is created, especially if there are contested versions of that past. Rouse (2002a) does not "bring us back," as Ricoeur (1981) would put it, from "within-time-ness to historicality, from reckoning with time to recollecting it" (p. 174). At the very least, to make sense of scientific activity in the present, we (those who study science) need accounts of past scientific activity as examples we can use to argue our historical or philosophical principles regarding science. In addition, to the extent that scientists incorporate an historical understanding of their research situation in order to take action, they also use accounts of the past. *How* we construct those stories, those narratives, *does* matter. Rouse (2002a) does not adequately problematize the construction of these stories, nor does he fully consider how such stories might matter in the present.<sup>90</sup>

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<sup>90</sup> In *Finnegans Wake*, Joyce takes the radical postmodern move and, like Rouse, rejects any easy dichotomy of the linguistic and the material, while at the same time rejecting an analysis of the world through things in themselves. Yet Joyce goes beyond Rouse and proposes that the analysis of the world should be through language, through poetics, through narrativity. As Eco ([1966] 1982) shows, Joyce "offers us the entire wisdom of mankind, without determining whether or not it reflects a unique Eternal truth. He is only concerned

Moreover, Rouse (2002a) does not sufficiently problematize the distinction between being in the present and telling stories about the past. His naturalistic system specifies how and why things hang together *in medias res*, but in order to do that, more than the *present* is required. The *past* is also needed, not to mention the *future*. Moreover, just as normative configurations in the present are contested and are themselves at stake, so also are the configurations of the past contested in the sense that how and why they are constructed *matters*. Furthermore, Rouse believes that those normative configurations of the past have no normative authority on us now, in the present, if they are far-removed enough from our own situation. That is, they have no normative hold on us (pp. 344-5).<sup>91</sup> Rouse (2002a) puts it this way:

One can ask intelligibly whether the normative authority of other ways of thinking and acting is conceivable, given what is at stake on one's own practices. One cannot ask in the same way, however, what normative configurations would be intelligible if these other norms were in fact our own . . . This question is forlorn not because of a lack of imagination, a hidebound enclosure within one's own preconceptions, for one can imagine any number of more or less plausible answers to such questions. The difficulty is instead a lack of any serious constraint upon the answer. It is not clear what would make any particular answer to such questions correct or incorrect, disconnected as it would be from anything actually at stake in the question. (p. 345)

What Rouse has articulated here, insofar as we can take "other ways of thinking and acting" as belonging to events in the *past*, are fundamental historiographical problems: *how* do we reconstruct the past and *how* do we do it responsibly, given that we must reconstruct it from the situatedness of the present (i.e., there is something *at stake* in our reconstructing the past in a particular way, whether as scientist or historian). Moreover, given that we cannot

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with the cultural repertoire assembled by the whole of History." Indeed, for Joyce, "there exists only one possibility: to engage the whole of this wisdom and to impose upon it a new Order, that of Language. Joyce engages a reality composed of all that has been said of it and organizes this world according to rules which are derived, not from the things in themselves, but from words that express things. He proposes a form of the world in language, a hypothesis offered from within the linguistic format." That is, in *Finnegans Wake*, "Joyce establishes the possibility of defining our universe in the 'transcendental' form of language. He provides a laboratory in which to formulate a model of reality and, in so doing, withdraws from *things* to *language*. To understand the nature of reality itself, rather than the cultural models of reality, is a task that belongs neither to science nor literature but to metaphysics, and the crisis of metaphysics arises from its inadequacy to this task" (pp. 84-5).

<sup>91</sup> Rouse (2002a) also put it this way: "But of course nothing makes a real difference in a merely possible world, even on the counterfactual supposition that it is actual" (p. 346, fn. 50).

reconstruct past norms as though they were our own, it seems Rouse would agree that any interpretation of the past would amount to imposing on the past (*wie es eigentlich gewesen ist*) a structure that gives meaning (for us in the present) to those events, and that this structure must contain elements that are “regulative fictions” (Kermode 1967, p. 43). Again, this structure is the familiar *narrative* structure of story telling, with a beginning, a middle, and an end. Clearly, then, we can say that the past is *not* present to us in the same manner as is the present; we are *temporally mediated* from the past in a fundamental way:

Time present and time past  
Are both perhaps present in time future,  
And time future contained in time past.  
If all time is eternally present  
All time is unredeemable.  
What might have been is an abstraction  
Remaining a perpetual possibility  
Only in a world of speculation.  
What might have been and what has been  
Point to one end, which is always present.  
Footfalls echo in the memory  
Down the passage which we did not take  
Towards the door we never opened  
Into the rose-garden.<sup>92</sup>

### **Joseph Rouse and Temporal Mediation**

It is not immediately clear why Rouse (2002a) did not adequately connect his systematic attempt to elucidate a program of philosophical naturalism to his earlier work on narrativity, which was itself rooted in naturalism. Indeed, that accounts of scientific understanding are fundamentally stories about the past Rouse earlier seems to have been quite clear. In his paper on the narratives of modernity in the philosophy of science, Rouse (1991a) states this emphatically (see esp. pp. 161-2). Later, in a rewritten version of this paper, Rouse (1996b, ch. 1) again makes it clear that we should not only accept a form of naturalism when studying how science works, we should also maintain the position that

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<sup>92</sup> T. S. Eliot, “Burnt Norton,” (1935) lines 1-14, in *Four Quartets*, (©1943, 1971), San Diego: Harcourt, Inc., p. 13; reprinted by permission of Harcourt, Inc.

accounts that are *about* science and scientists are ultimately stories that must be (re-) constructed—that is, they are stories about the past. To this end, Rouse (1996b) asks us to

not think of knowers as featureless and abstract reasoners but as situated agents with an inescapably partial position. Instead of thinking of the sciences in terms of “representations” of the world, look at the actions they involve and the ways they transform the situation for further action. Insofar as reports of scientific results represent something, these representations are embodied, and they represent only by being *used* for some purposes. Where they appear, how they are constructed, who is to read them, and how they circulate are inextricable from what they say. . . . There is no occupation of a metastandpoint from which to interpret science which is not a move within the contested terrain where scientists themselves operate, a terrain in which “science” names both the outcome and the shape of the contested terrain. (pp. 66-7)

Rouse (1996b) makes it clear that he is talking about our understanding of science as narrative-bound when he asks:

Would such a program for thinking about science help take us beyond modernity, or even “modernity”? That remains to be seen, as does whether that aspiration is worthwhile. If it does take us beyond modernist narratives, it does not get beyond telling stories, in which “science” names a character(s). The hope is that, in contrast to the *Bildungsromane* of “modernity,” the stories we may thereby learn to tell still hold out some possibility of being believed and perhaps even of being true. (p. 67)

Why Rouse (2002a) later omits any talk of narrativity seems to stem from his not deconstructing historiography adequately; in addition, his work’s primary aim seems to be to convince the reader that his version of naturalism is superior to others. Nevertheless, how the past is mediated from our actions in the present remains a problem for Rouse’s radical philosophical naturalism.

The mediation of the past is a fundamental problem for historians and as such is a salient historiographical concern. While our being-in-the-world and our dealings with things in the world may not be mediated by a conceptual scheme (i.e., one that we impose on a reality independent of us) because there is no uninterpreted reality (Rouse 1987, pp. 161-3), our *knowing* something, according to Rouse (2002a), “is mediated not just by a background of assertional commitments, but also by models, skills, instruments, standardized materials and phenomena, and situated interactions among knowers, in short, by practices” (pp. 178-9). However, this consideration of mediation applies also to our *knowing* about the past; that is,

it applies to history. Our knowledge of the past is mediated temporally, as well as spatially, from our belonging to the here and now.

Although “the world already has a (normative) grip upon us” (Rouse 2002a, p. 26), this “grip” is in the ubiquitous present, the here and now, ever-changing into the future. As naturalists, this we do not doubt—we are practical about this naturalistic presumption so we can get on with the business of life, that is, acting to change the future. Yes, I may have some idea of the norms of the present game, whatever that game may be, for effecting the kind of change (future) I desire, and I agree with Rouse (2002a) that my intentionality makes no sense without the material world. Yes, they are intra-twined; each must have the other; my being intra-actively situated in the world allows me to engage in intentional interpretation.

As an historian, however, I am not intra-actively situated in the world of, say, 1945—before the discovery of ribosomes, or the explication of a coherent quantum explanation of superconductivity, or the technological/political development of a system of intercontinental ballistic missiles with enough nuclear warheads capable of destroying most, if not all, life on earth. The temporal discontinuity between me and the world of 1945 creates serious epistemological (and ontological) problems for any attempt to account for the conditions of the production of the “discovery,” “explanation,” or “systems development.” That is, when I recur and attempt to interpret actions in 1945, I jump across a temporal breach to a time before they were produced. Yet, as Rheinberger (1994) argues, I ironically need these (future) products in order to assess the conditions of their production. This is the futural sense of normative authority Rouse (2002a) was considering when he said that “our normative reach always exceeds our grasp” and that “what is at stake in practices outruns any present articulation of those stakes” (p. 26). But the problem with this is that before their production, they each were, as Lyotard ([1982] 1984) suggests, “something which does not allow itself to be present” (p. 80). My access to 1945, now, is mediated by my dependence on historical evidence; this dependence requires me to engage in historical interpretation (representation) in order to fill in the temporal breach. However, when I jump the temporal breach, I confront an epistemological/ontological discontinuity—I am no longer considering

my world, the here and now; I must reconstruct a past “world,” but it is not (was not) a world I am intra-actively situated in (even if I were alive then). This temporal/epistemological/ontological discontinuity poses problems for Rouse’s naturalism, in that *my* presupposing a world in which people *were* intra-actively situated—the world of 1945—is epistemologically contaminated by my being in the world here and now. So, I must be cognizant of this discontinuity, reflect carefully on which level I am operating, and reflexively realize that my connection to 1945 is primarily, if not completely, *textual*. It is textual, and not material, even if I agree with Rouse (2002a) that there is something at stake in the world *now* in my interpreting the world of 1945 in this way or that.

In what sense, we might ask, is historical interpretation situated intra-actively in the world? Historical interpretation is a discursive/material practice; according to Rouse (2002a), we must be situated intra-actively in the world to engage in intentional interpretation (p. 285). I, the historian, engage in practices I am obliged to follow; I may have some idea of the norms operating in my situation, although it is not necessary for me to be cognizant of the rules for those rules to actually be operating. I interact (discursively/materially) with other people, institutions, standards, and so forth, in order to construct a product, namely historical interpretation. Significantly, though, I interact with evidence in the form of *written* texts, although pictures, films, and material artifacts may be helpful. I need these texts to construct my argument about how and why something happened in the past. So, yes, I am situated intra-actively here in the present world, but my world may be very different from the world of 1945, when quite different discursive practices may have existed. If I am far-removed from that past time, however, I may be better able to resist presentist tendencies to use contemporary categories to evaluate those past practices. Nevertheless, I am constrained by my present, ongoing, causal intra-action in the present world.

As a good historian, I attempt to find out what *were* the categories, logics, fields of possibilities, that is, the discursive/material practices of the past, and then I use them to construct an account of *why* something happened. Historical interpretation goes beyond mere description of *what* happened, although that itself may turn out to be problematic. Getting straight *what* happened is not necessarily easy to reconstruct. Telling *why* something

happened is most often more problematic, since what one is doing is adding another layer of interpretation to the narrative. Hence, telling *why* amounts to more than a simple recitation of one actor's account of the past, and it may even differ from that past actor's account. Therefore, my thoroughly indeterminate account of the past, while it is constrained and made meaningful by my being situated in ongoing Rousean causal intra-action in the *present* world, is mediated temporally from the world of the past.

Yet my goal is to account for past actors' intra-actions in that past world. Specifically, in the case of science or technology, I want to account for scientists' past attempts to describe (disclose) the natural world and how and why it works as it does, which for Rouse (2002a), was meaningful because it was a discursive/material practice at that time. However, past scientific or technological practices, especially if far-removed temporally, may not be very meaningful to us in the present. So, we must recur and reconstruct. That reconstruction, again, is mediated by temporal distance.<sup>93</sup> Any attempt to make sense of what scientists did—with an eye toward understanding the present—must first deal with this temporal breach. The only way we can philosophize about science or technology is to do history (tell stories) first. Hence, we must deal with the temporal breach first and the mediation that constitutes it. This mediation and how we cope with it is guided (not necessarily intentionally) by whatever principles we might apply to it—and that is historiography. To consider what constitutes an adequate historiography, or meta-history (i.e., History-5, or what a History-4 should be like), is one of the major goals of this dissertation.

How to breach the temporal discontinuity? This problem seems to be prior to constructing any philosophical system for accounting for science and technology. Rouse's naturalism, as it flowed from a principled postmodernism, remains compelling. Nevertheless, it is at least conceptually incomplete, for it does not handle properly the

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<sup>93</sup> I discovered that my use of the term *temporal distance* is not original. Ricoeur (1984) uses it, in his analysis of the relation between historical representation and the historical past, to identify the trend in historical inquiry that emphasizes the “otherness” of the historical past: “The concern with recovering the sense of temporal *distance* stands in opposition to the ideal of re-enactment when the main emphasis is placed, in the idea of historical inquiry, on *taking a distance* with respect to the temptation of or the attempt at ‘empathy’” (p. 15).



epistemology of time. It does not reflexively include the *author* in the narrative, the story—and that author could be a philosopher, historian, or even a scientist. How *we* know about people in the past belonging to patterns of ongoing causal intra-action is through mediated historical inquiry. Yes, we may naturalistically presuppose that we always already belong to an ongoing narrative context, but to *know* significant information about it, we must first do research—historical research—which involves confronting the temporal breach. And, ironically, by engaging in such activity, we thereby change the narrative (if anyone reads and takes up that interpretation). This is so, because we were forced to use the product—the present narrative—to assess the conditions of its production (cf. Rheinberger 1994, 1997). But that could have been no other way.

The norms of the game of historical interpretation are part of what this dissertation is about. However, one might ask, am I bound by rules or norms while I am trying to formulate and/or change the rules of the game? I might, following Rouse (2002a), presuppose that they exist, but can I know them without experimenting, that is, conducting historical interpretation? In doing so, however, if I am successful, I will have changed the rules of the game, of the narrative. So, in a sense, as historians, philosophers, etc., we sometimes work, according to Lyotard ([1982] 1984), “without” rules “in order to formulate the rules of what will have been done” (p. 81). We need the outcome, the product—which is *now* still in the future—in order to assess what the rules “are” (were). But by that time, the time when the analysis is performed, the rules will be (will have been) in the *past*. And in order to assess the past, we must confront the temporal breach separating the past from the present, the here and now.

In the next chapter, I begin to develop the construct *the technological infrastructure of science* from the principles and arguments I have developed in the first three chapters. Building on the historiographical and philosophical deconstructions so far presented, I incorporate further perspectives into Pitt’s (2000) model for the technological infrastructure of science. In addition, I consider Rheinberger’s (1994, 1997) views on narrativity, historicity (derived, in part, from Derrida), and the epistemology of time, and how they improve and add to others’ works. I then use the technological infrastructure construct in

later chapters to analyze historical and philosophical case studies.

## CHAPTER IV

### Joseph Pitt and *The Technological Infrastructure of Science*

So the question is not whether we will be extremists,  
but what kind of extremists we will be.

—Martin Luther King, Jr.<sup>94</sup>

In this chapter, I analyze Joseph C. Pitt’s concept of the *technological infrastructure of science* as an historical, historiographical, and policy tool. In doing this, I aim, in this and the next chapter, to submit a manifesto for how to do, and how not to do, philosophical, historical, and other work involving how to conceptualize science and technology as human cultural activities. In addition, I explore, with examples given in later chapters, how to apply the technological infrastructure construct to the history of science and technology, as well as science and technology policy. Following Rouse, with epistemic matters no longer taken as separable from political matters, policy prescriptions and policy critiques can then be formulated from the basic principles of the technological infrastructure, drawing on the philosophical, historical, and historiographical principles of Chapters I, II, and III. Ultimately, the construct, as I (re)conceptualize it, is grounded in the concept of “reverence for truth.”<sup>95</sup>

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<sup>94</sup> “Letter from the Birmingham jail,” in *Why We Can’t Wait* (1964), New York: Harper and Row, pp. 77-100. Written on 16 April 1963 in the city jail in Birmingham, Alabama. First published as “Letter from Birmingham City Jail,” in *The New Leader*, 24 June 1963.

<sup>95</sup> The sense of ‘reverence’ I invoke here is the areligious sense of the term as developed by Paul Woodruff (2001) in his scholarly book on reverence as a forgotten virtue. The sense of ‘truth’ is that for which Foucault ([1968] 1995) asks: “Why, in fact, are we attached to the truth? Why the truth rather than lies? Why the truth rather than myth? Why the truth rather than illusion? And I think that, instead of trying to find out what truth, as opposed to error, is, it might be more interesting to take up the problem posed by Nietzsche: how is it that, in our societies, ‘the truth’ has been given this value, thus placing us absolutely under its thrall?” (p. 45). On the concept of “reverence for truth,” Woodruff (2001) states the following: “Reverence for truth leads to humility in the face of the awesome task of getting something right. But humility is not despair, and it is not skepticism. In communities that seek learning, it is expressed in the rituals of conferences and peer-reviewed publications. Arguments must be given, and counterarguments must be heeded, or else positions must be modified or abandoned. Reverence is not easy” (p. 155). We should have reverence for truth, precisely because it—the truth—is something that is ultimately beyond our reach. As Woodruff (2001) states, reverence

To present the concept of the technological infrastructure, I first present a brief overview of constructs similar to Pitt's (1993a, 1993b, 1995, 2000) notion of *the technological infrastructure of science*, and I show how Pitt's view is superior. Next, I analyze Pitt's formulation, with an eye toward making it compatible with the prescriptions developed in Chapters I, II, and III. This new formulation, in addition to drawing on the works of Pitt, Rouse, and Rheinberger, also finds itself in close alignment with Lelas's (1993) notion of "science as technology." I then show how this new formulation is compatible with Rheinberger's conceptualization of the "experimental system" as an historical and historiographical construct, which aims to specify how, when, and why science and technology are successful enterprises. Finally, I show that the primary starting point for locating in history a technological infrastructure, and for answering historiographical questions of particular episodes in history, is finding out how scientists and researchers attempt to separate signal from noise. With this prescription, the present analysis comes full circle to the initial problem posed in Chapter I above, namely how to conceptualize the central question of the philosophy of experiment: how do scientists separate signal from noise, or entity from artifact?

I argue that the technological infrastructure of science construct, in part a meld of many other researchers' constructs, will facilitate a better understanding of the practices of science and technology, where "better" is understood as that which will permit conceptions arising out of the postmodern naturalism I have presented in this dissertation. The arbiter of the success of these conceptions will be ongoing research, which will in part consist of historical, historiographical, and philosophical examples and arguments, that themselves must be constructed from principles derived from one perspective or another. My provisional argument is that those arguments developed from the postmodern perspective (as I have borrowed and developed it here) will be superior to others that do not incorporate the fundamental principles of postmodernity (e.g., rejection of epistemic sovereignty), *if* what

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"cherishes freedom of inquiry. Reverence sets a higher value on the truth than on any human product that is supposed to have captured the truth" (p. 39). Furthermore, having such a virtue—"a capacity for self-transcendence"—as Václav Havel ([1994] 1995, p. 238) put it, can lead us back to the "certainty that we are rooted in the Earth. . ." (p. 237). In other words, realizing that we can only strive for the truth, but cannot reach it, and further that the striving is a virtuous activity, reflects a naturalism that finds us always already in the world.

one is striving for is a principled commitment to, or reverence for, histori(c)al and historiographical truth (appropriately defined). The principles of postmodern naturalism, it must be stressed, are not necessary constructs, but are derived from lessons learned from being in the world. As such, they are historically contingent, fallible, and defeasible, yet designed to be inexpugnable components of historical, philosophical, scientific, and political research. Their efficacy is contingent on the future of this present, this here and now.

### **History and Philosophy of Technology: *Technological Infrastructure***

The concept of a technological infrastructure permeating and perhaps even determining human activity goes back at least as far as Jacques Ellul (“Technique” as the ensemble of human means),<sup>96</sup> and perhaps further to Lewis Mumford (“technics” or the “megamachine” controlling societies),<sup>97</sup> or even Karl Marx (the infrastructural mode of production determining superstructural social relationships).<sup>98</sup> The view that the human

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<sup>96</sup> On Jacques Ellul’s (1912-1994) “Technique,” see *The Technological Society* (1964), trans. John Wilkinson, New York: Knopf; first published in French in 1954 as *La Technique ou l’enjeu du seicle* (Paris: A. Colin). Ellul’s (1962) view was strongly deterministic: “Modern man in choosing is already incorporated within the technical process and modified in his nature by it. He is no longer in his traditional state of freedom with respect to judgment and choice” (p. 396).

<sup>97</sup> On Lewis Mumford’s (1895-1990) “technics” and “megamachine,” see *Technics and Civilization* (1934), New York: Harcourt, Brace; “The First Megamachine,” *Diogenes: An International Review of Philosophy and Humanistic Studies* (1966a); *The Myth of the Machine*, vol. 1, *Technics and Human Development* (1967), New York: Harcourt, Brace and World, and vol. 2, *The Pentagon of Power* (1970); and *The Lewis Mumford Reader*, ed. Donald L. Miller (1986), New York: Pantheon Books, esp. Part VI, “Technology and Culture.” Mumford was not a technological determinist (cf. Miller 1986, “Introduction” to Part VI, p. 300). He explicitly challenged those who suggested we are at the mercy of our machines: “I shall suggest that not only was Karl Marx in error in giving the instruments of production a central place and a directive function in human development, but even the seemingly benign interpretation of [Pierre] Teilhard de Chardin reads back into the whole story of man the narrow technological rationalism of our own age, and projects into the future a final state in which all the further possibilities of human development would come to an end. . .” (Mumford 1966b, p. 304).

<sup>98</sup> On Karl Marx’s (1818-1883) view that modes of production constrain or determine social conditions, see, for example, Karl Marx and Friedrich Engels, *The Communist Manifesto*, ed. and trans. L. M. Findlay, Toronto: Broadview Editions, 2004; first published in 1848 in London as *Manifest der Kommunistischen Partei*. According to Marx and Engels: “But don’t wrangle with us by judging our intended abolition of bourgeois property against your bourgeois notions of freedom, culture, law, &c. Your very ideas are born of the conditions of bourgeois production and property, just as your law is but the will of your class made into regulation for all, a will, whose content is determined by the material conditions of existence of your class” (p. 78). See also Marx’s *Capital: A Critical Analysis of Capitalist Production*, trans. from third German

activity we call science is constrained and enabled in important ways by technological infrastructures, appropriately defined, is the unfolding contribution to technology studies of Joseph Pitt (e.g., 1993a, 1993b, 1995, 1997b, 2000). Pitt (2000) defines a technological infrastructure as “an historically determined set of mutually supporting artifacts and structures that enable human activity and provide the means for its development” (p. 129). With this definition, echoing the views of Ellul, Mumford, and Marx, Pitt means to include more than merely machines, rejecting the traditional definition of technology. For Pitt (1997b), a technological infrastructure of science is “that assembly of different forms of work relations among people which makes the doing of science possible.” Underscoring how a technological infrastructure is needed for new, successful developments in science to be made, Pitt (2000) suggests that

*the mechanism that makes the discoveries of science possible and scientific change mandatory is the technological infrastructure within which that science operates, and that to understand why a science worked the way it did and why it works the way it does, you need to understand its context, which happens to include in important ways its technological infrastructure.* (p. 132)

This formulation makes the search for the technological infrastructure not only a philosophical problem, but also an historical and historiographical problem.

Clearly, with technological infrastructure, Pitt has in mind a broad and powerful research tool with which he aims to answer important questions regarding the sciences and their changes over time. When accounting for scientific or technological change, Pitt (2000) argues we should look for the technological infrastructure that is making possible new, successful developments. If we do this, we will be able to pose answers to historical questions concerning scientific change without recourse to certain modern dualisms, such as realism/antirealism and positivism/constructivism (ch. 8).

Pitt’s hybrid construct, a conglomeration of material and social cultures, we can compare to Ellul’s ([1954] 1964, 1962) technological determinism and its embodiment in the concept of *Technique*. For Ellul (1962), *Technique* was “the new and specific *milieu* in which man is required to exist. . .” (p. 394). And, as Merritt Roe Smith (1994) notes,

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edition by Samuel Moore and Edward Aveling, and ed. Friedrich Engels, London: Swan Sonnenschein & Co., Ltd., 1902, eighth edition; first published in 1867-94 (3 vols.) as *Das Kapital*.

Technique “refers to a good deal more than machines. . . . It also encompasses organizational methods, managerial practices, and . . . a manner of thinking that is inherently mechanistic” (p. 30). However, Ellul’s (1962) construct, insofar as he believed it was “a closed organization which permits it to be self-determinative independently of all human intervention” (p. 394), Pitt (2000) rejects as “reification” (pp. 87-8). Indeed, Pitt (2000) is no technological determinist (ch. 6); for Pitt, technology is “humanity at work” (p. 114). So, while Ellul’s Technique shares and perhaps anticipates some of the characteristics of Pitt’s technological infrastructure—it involves material and social constructs—Pitt parts company with Ellul on the issue of determinism. For Pitt (2000), people create and apply technologies in various ways; he rejects the notion that “technology is threatening our way of life” (chs. 5, 7).

Another scholar who developed a concept similar to Pitt’s technological infrastructure is the economic historian Robert Heilbroner. In his classic 1967 essay, “Do Machines Make History?,” Heilbroner ([1967] 1994) noted that it is difficult to determine “the degree to which the technological infrastructure is responsible for some of the sociological features of society” (p. 61). Here we have explicit use of the terminology of technological infrastructure, yet it was Heilbroner’s goal, as it was Ellul’s, to argue for a form of economic technological determinism. Indeed, Heilbroner believed that as far as a society’s process of production was concerned, “We can indeed state that the technology of a society imposes a determinate pattern of social relations on that society” (p. 59). Again, Pitt’s technological infrastructure is incompatible with such a notion, even if Heilbroner will permit “the machine [to] reflect, as much as mold, the social relationships of work” (p. 61). Pitt will not allow for the reification of economic forces Heilbroner takes as primary, whether it be modes of production, or his more recent focus on the economic “force field of maximizing” possibilities and their function as a “mediating mechanism by which changes in technology are brought to bear on the organization of the social order” (Heilbroner 1994, pp. 71-73). Pitt (2000) rejects all attempts to maintain a general rule or universal explanation of this sort, for change in science and presumably for historical change in general (ch. 4).

Langdon Winner is another scholar who has placed significance in a concept close to

Pitt's technological infrastructure. For Winner (1986), a major issue for those studying technologies is "evaluating the material and social infrastructures specific technologies create for our life's activity. We should try to imagine and seek to build technical regimes compatible with freedom, social justice, and other key political ends" (p. 55). Winner's view that technologies are inherently value-laden—that is, political—underscores the cultural component of technological infrastructures. This raises the question of agency. While Winner attributes some degree of agency to technologies in affecting sociocultural change, it is not clear that Pitt will follow Winner on this issue, even though Pitt (2000) insists that "the development of a technological infrastructure is essential if science is going to *continue* to provide us with new discoveries about how the universe works" (p. 130). Pitt's analysis suggests that technological infrastructures have the ability to make an historical difference, but his view of technology as "humanity at work" suggests that nonhuman entities (i.e., "technologies" in the usual sense of the term) are inert, do not have agency, and are value-neutral.<sup>99</sup> Below, I analyze Pitt's view on technological agency and suggest a possible way to reconcile these views. However, it is clear that Pitt will firmly stand his humanist ground and reject Winner's view of technology because it is too deterministic—it grants too much autonomy to technologies.

Other scholars have developed constructs similar to Pitt's technological infrastructure, but important differences remain that leave Pitt's construct fresh and innovative. Thomas Hughes's (1987) *technological system* construct, for example, includes "physical artifacts" as well as "organizations, such as manufacturing firms, utility companies, and investment banks, and [it] incorporate[s] components usually labeled scientific, such as books, articles, and university teaching and research programs" (p. 51). In addition,

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<sup>3</sup> In his criticism of Winner (1986) as an ideologue, Pitt (2000) argues that "tools and technical systems are *inherently ideologically neutral*. Individuals with particular axes to grind may employ a tool to achieve their ends, but this does not make the tool itself ideological" (p. 72). Later, Pitt (2000) claims that "even if a tool is used by individuals committed to a particular ideological stance, it is not the tool that is ideological. I conclude, therefore, that technology, tools, and systems of tools are ideologically neutral" (p. 82). This bifurcation of human and nonhuman, with apparently only humans having agency, is consistent with Pitt's humanism and empiricism. In order for his empiricist historiography and policy strategy to be successful—and for Pitt this means making rational judgments based on empirical facts—Pitt needs a traditional humanism in which to isolate offensive ideological value judgments. If things were to be seen as ideological, this would frustrate Pitt's attempts to separate out epistemic judgments from aesthetic value judgments, and subvert his underlying epistemic sovereignty.



Hughes's (1994) view, according to which mature technological systems gain a degree of "momentum" or autonomy from the surrounding cultural environment, seems to echo Pitt's view of the technological infrastructure of science. For Pitt (2000), "after slow and modest beginnings, a *developed* science *requires* this kind [i.e., infrastructural] of technological framework" (p. 130). Here it seems we have a good match, if we overlook the point that Hughes does not consider scientific change in his work.

Nevertheless, it is not clear that Pitt (2000) and Hughes (1994) have in mind the same kind of mechanism for embeddedness. To make the point clear, I turn to Pitt's and Hughes' views about what accounts for the embeddedness of a science in its technological infrastructure and for the partial autonomy, or momentum, of a technological system from the surrounding culture. Do we invoke power or political mechanisms, economic forces, social forces, or epistemological criteria? Hughes (1994), on the one hand, is somewhat vague on what exactly momentum is and how it works, although he does say its "characteristics" include "acquired skill and knowledge, special-purpose machines and processes, enormous physical structures, and organizational bureaucracy" (p. 108). Pitt (2000), on the other hand, requires that the epistemic context of the change in scientific knowledge be given primary status in evaluating the role of the technological infrastructure, and that the explanation generated for the knowledge change be historically relevant and coherent (see esp. ch. 5). It seems both Pitt and Hughes want a variety of types of mechanisms to operate potentially in any specific historical context that is to be evaluated. What we seem to need, however, are further criteria that specify how knowledges, machines, and organizations are to be molded into a story of technological change, and how to apply criteria of epistemological and historical relevance to the telling of a compelling story of scientific change. In short, we need to know how *technological infrastructure* and *technological system* operate as historiographical tools, as well as historical tools.

## **A Critique of Joseph Pitt's *Technological Infrastructure* Concept**

Joseph Pitt's philosophical identity stems from American pragmatism, and in particular from Wilfrid Sellars, another pragmatist to whom he has repeatedly indicated his philosophical debt (e.g., Pitt 2000, pp. ix-xii; Pitt 1993a, pp. 3-7; see also Pitt 1981). Pitt likes to quote Sellars' definition of philosophy, a definition that is compatible with the postmodern perspective in this dissertation, even if Sellars and Pitt, because of their empiricist presuppositions, might not accept all components of this perspective. According to Sellars (1968):

The aim of philosophy is to understand how things in the broadest sense of the term hang together in the broadest sense of the term. Under 'things in the broadest possible sense' I include such radically different items as not only 'cabbages and kings,' but numbers and duties, possibilities and finger snaps, aesthetic experience and death. To achieve success in philosophy would be, to use a contemporary turn of phrase, to 'know one's way around' with respect to all these things, not in that unreflective way in which the centipede of the story knew its way around before it faced the question, 'how do I walk?' but in that reflective way which means that no intellectual holds are barred. (p. 1)

It is interesting to note how this definition of philosophical success seems to suggest a practical knowledge of being in the world, and as such seems compatible with the kind of naturalism Rouse (2002a) advocates. Pitt (1993a) interprets Sellars' definition as prescribing that a proper philosophical methodology "*assumes no privileged point of departure*" (p. 4). Pitt is adamant that philosophers reject "the assumption of an *a priori* privileged perspective in the doing of philosophy. . ." (pp. 4-5). He calls philosophy based on such a privileged perspective "fascist philosophy" (p. 5). Yes, we must start from some perspective or another, he suggests, but there are many perspectives that are legitimate, and not one that is privileged once and for all (pp. 4-5). However, as inclusive as this interpretation sounds, and as compatible as it seems with the perspectives explored above in Chapters I, II, and III, it is worth analyzing whether Pitt's own philosophy of technology has satisfied the conditions of his interpretation of Sellars' definition.

One of Pitt's main goals in his writings on the philosophy of technology has been to undermine the traditional trend in the field, a trend that attempts to argue that technologies are inherently dangerous, anti-democratic, or otherwise socially undesirable, largely because

of their autonomous characteristics (cf. Pitt 2000, pp. 8-12, 70-86, chs. 6-7).<sup>100</sup> One of the ways Pitt has reviewed this trend involves arguing that such traditionalists in the philosophy of technology have failed to separate epistemic judgments from aesthetic value judgments. For example, Pitt (1989) argues that “there is a major problem with using normative ethical categories to assess” technology (p. 173). Such analyses

fail to address the technology in terms of whether or not it will do the job for which it was introduced. Instead, they confront the technology with a set of ethical standards, formulated under different constraints than were employed in its creation. Thus, rather than asking if the technology works as it is supposed to, we compare the effects of the technology to an ideal picture derived from some conception of what ought to be the case. In other words, we often tend to confuse two dimensions of our analysis, criticizing what is the case by reference to what ought to be and rejecting appeals to what ought to be by reference to the facts. (*ibid.*)

For Pitt (1989), epistemic values should be given priority over aesthetic values, because epistemic values are objective in the sense that they can be “tested against the world” (p. 172). Aesthetic values, conversely, “lead us into an infinite regress, looking for the ultimate conditions of adequacy” (*ibid.*). For Pitt, philosophy of technology traditionalists, those who apply ethical and/or moral principles to analyze technological developments, have made the mistake of not getting their facts straight before making their aesthetic arguments. Clearly, the neo-positivist empiricism on which this perspective is based is diminished in Pitt’s later work on the philosophy of technology, yet his commitment to a form of epistemic

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<sup>100</sup> See, for example, Mitcham and Mackey (1972) as an example of the traditional trend in philosophy of technology that Pitt criticizes; the contributions to this volume concentrate on ethical, political, and religious critiques of technology, including many that adhere to technological determinism. Pitt (1990) argues that “philosophy and philosophers . . . must talk to the world in which we live. That means our account of technology must be sensitive to . . . history; otherwise how can we claim to know what it is or how it came to be that way? Our critical views on technology must be informed by as complete a knowledge of history as possible including its impact on the various aspects of human society. We must come to technology through history and only then can we ask the important philosophical questions concerning the nature of technological knowledge and worry about the proper means of assessing technological projects. An historical approach to understanding the roles technology has played in human evolution will force us to abandon talk about Technology as a single thing and will expose the bankruptcy of such notions as technological determinism. Only then will we see that just as the criteria for scientific knowledge have changed over time, so too has our understanding of what constitutes technological knowledge. We will see that attempts to box technology into a single kind of thing fail as new social technologies emerge with their own set of problems and concerns. To be of philosophical value we must abandon apriori metaphysical characterizations of technology and anything else, stop relying on superstitious religious values, and get to work learning about the thing we wish to understand” (pp. 14-15).

sovereignty seems to linger. Pitt wants epistemic judgments to be given their due when new technologies are assessed; he believes that even philosophers of technology have repeatedly failed to adequately employ such an epistemic policy prescription.

To rectify this problem, Pitt (1989) proposes a three-fold strategy for technology assessment. The three stages are *epistemic*, *aesthetic*, and *rational* (p. 177). First, get the facts straight, then make moral or ethical value judgments, and then make a decision among aesthetic judgments “*on rational grounds*” (*ibid.*). Pitt’s method for deciding among competing value judgments he calls CPR,<sup>101</sup> or the “Common Sense Principle of Rationality” (*ibid.*). According to CPR, we must “learn from experience” so that “when there is no obviously superior option,” we should “consider what we have already learned, that is, consider other similar situations” (p. 178). By using this rational, empiricist-based methodology, Pitt believes we can make technological decisions that are not based on *a priori* value judgments that confuse the situation by not fully considering the relevant epistemic context. As Pitt demands: “What should not be allowed is to have aesthetic considerations intrude into what is primarily an epistemic context” (p. 176).

Another way Pitt (2000) strives to undermine traditionalists’ attempts in the philosophy of technology to maintain the discipline’s focus on ideological critiques of technologies, has been to argue that manipulating the world is “epistemically prior” to theorizing about the world (p. 104). At first glance, this strategy seems to echo the anti-representationalist focus of such authors as Rouse, Rheinberger, Pickering, and other posthumanists who want our being in the world, rather than our theorizing about it, to take priority in how we conceptualize scientific activity. However, Pitt’s dichotomy of knowing/acting (or representationalist/performativity) serves mainly to bolster his pragmatist-based notion of making rational decisions based on past actions. As Pitt (2000)

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<sup>101</sup> Pitt’s “Common Sense Principle of Rationality” (CPR) dates back at least as far as 1988, when it appeared in Pitt’s (1988c) article entitled “Style and Technology”; it also appeared that year in “Simplicity and the Aesthetics of Explanation,” (1988b). Pitt (2000, p. 22, fn. 5) indicates that he first introduced CPR in his 1991 book, *Galileo, Human Knowledge and The Book of Nature: Method Replaces Metaphysics*. For more on Pitt’s earlier views on rationality and explanation in science, see “Galileo and Rationality: The Case of the Tides,” ([1985] 1987); and “Galileo, Rationality and Explanation,” in which Pitt (1988a) argues that Galileo was not a realist in the contemporary sense of the term; he suggests that the 1980s debate over scientific realism was unresolvable (pp. 101-2).

states,

*technology is epistemically prior to science because when technology is conceived as humanity at work, action precedes theorizing.* Theorizing grows out of action by way of reflecting on the success or failure of the action. As human beings, our major concern is to act, changing our environment to meet our goals and needs. We act first, learn from our actions, and then try again . . . . It is not just that we need to know before we can act. *The need to know derives from the need to act better.* Knowledge, including science, is secondary to acting. In other words, knowledge, i.e., science, is at most another tool. (p. 104)

Clearly, Pitt needs to separate acting from knowing, not out of concern for countering the representationalist trend in the philosophy of science and history of science that ensures epistemic sovereignty is maintained, but out of concern for supporting his own rationalist, empiricist methodology for making technological decisions. As I argue below, underlying Pitt's methodology is a form of epistemic sovereignty—it is built into his methodology for technology assessment and derives from his presupposition that science is the privileged “epistemological engine” (Pitt 1983).<sup>102</sup>

In developing his methodology for technology assessment, Pitt presupposes several modern dichotomies that serve to underpin his method with logical empiricist foundations and ultimately epistemic sovereignty. These dichotomies are human/nonhuman, epistemic/political, rational/irrational, and representational/performative. Pitt generally privileges the left side of each of these dichotomies, with the exception of the last, for which he privileges the right side, and epistemic/political, for which he seems to privilege the left side, then

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<sup>102</sup> In this article, Pitt (1983) expresses his commitment to “the priority of science as our epistemological engine” (p. 77), but the main focus of the article is on how the discipline of the philosophy of science, and its obsession with scientific theories and their logical status (and hence the notion of the progress of that knowledge toward truth, i.e., convergent realism), has missed the point about why science is successful. For Pitt, what philosophers of science have neglected is technology as “the support system of science” (p. 82). Hence, Pitt seems to be arguing that *technology* is actually the epistemological engine. Here we have an early version of the technological infrastructure concept; Pitt cites (p. 82) Emmanuel Mesthene's (1970, p. 25) definition of technology as an influence: “the organization of knowledge for the achievement of practical purposes.” Pitt (1983) argues that “we must begin first with an understanding of technology if we are to finally give science its appropriate analysis” (p. 92). Such an analysis should “begin with current theory *and* the current reservoir of tools, institutions, goals, values, etc. and see how they affect one another at a given time.” It should also prescribe “that there is no one factor which dictates how things change, but many factors in constantly changing configurations” (p. 91). This model for scientific and technological change, if stripped of its logical empiricist foundations, becomes compatible with the postmodern perspective developed in this dissertation.

suggests the right side has priority. We can view the left side of each of these dichotomies as representing the traditional view of science we have inherited from the logical positivist and logical empiricist movements in the philosophy of science. This traditional, or modern view sees science as conducted by special humans with a privileged method, one that is logical and rational, and as embodied in theories whose appropriate logical status is all that is required to ensure the objectivity of scientific knowledge. Indeed, a main principle of the traditional view that Pitt rejects is the focus on representing, that is, philosophy of science's obsession with scientific theories, yet he does so by privileging the opposite pole of the dichotomy, performativity, or as Pitt puts it, acting as opposed to knowing. While Pitt does problematize the realism/antirealism and positivism/constructivism dichotomies, as noted above, the question of whether or not Pitt has followed Sellars' advice and not assumed a privileged point of departure, remains an open question.<sup>103</sup>

In extending Pitt's concept of technological infrastructure, I propose resisting the remaining dualisms that underlie Pitt's methodology. First, following Rouse (2002a) and Pickering (1994, 1995a, b), a thorough posthumanism is needed that does not privilege agency as belonging only to humans. Tools or technologies should not be seen as value-neutral, as Pitt believes, because they cannot be removed from the historical or narrative contexts in which they belong. If something is *at stake*, as Rouse (2002a) would say, whether it is a proposed technological development, a knowledge claim, or whatever, there will be resistance (by at least some) in the sense that Foucault means there is resistance to power (power *not* sited in an individual or class of individuals, but power, we might say, that

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<sup>103</sup> Pitt (2000) undermines the realism/antirealism dichotomy by proposing "Sicilian Realism," or "realism with a vengeance" (p. 135). Sicilian Realism, so named to "reflect the varied cultural history of Sicily" (p. 135, fn. 11), deems all the entities of science as "real" in a pragmatic sense, without any appeal to a reduction of one entity to another (pp. 135-6). Even if Pitt is proposing a form of entity realism, it is a pragmatic realism that incorporates aspects of both poles of the dichotomy without privileging one over the other. Pitt (2000) dissolves the positivism/constructivism dichotomy by arguing that both the neo-positivist (or empiricist) trend in the 1980s and 1990s toward scientific realism by philosophers of science that developed in response to Kuhn's ([1962] 1970), among others, questioning of the traditional (or received) view of science, *and* the social constructivist trend that developed at about the same time among mainly sociologists, both make appeal to a realism that we must reject (pp. 127-8). The scientific realists want the correspondence of scientific theories to mind-independent physical reality, while constructivists want the correspondence of scientific theories to be empty, but explained instead by objective sociological interests. To explain the success of science, the former advocates scientific realism and the latter social realism.

in part constitutes a culture or subculture). Knowledge claims, when considered in their relevant historical or narrative contexts, presuppose power relationships; when there is something *at stake* there is, or should be, concern for the *real possibility* of how that claim might be realized. In addition, since Pitt wants us to view technology as more than just machines or tools—we should also include organizations, bureaucracies, and other groups of people with power resources—it seems an incoherent strategy to assume a privilege for humans when it comes to agency. What *a priori* reason do we have for denying agency to the natural world? Furthermore, assuming a bifurcation between humans and nonhumans results in violating the naturalistic presupposition that we are always already in the world by objectifying the “world out there.” Pitt wants us to do history carefully, but if in doing that history we remove tools or technologies from their appropriate historical or narrative contexts and claim to evaluate them “objectively,” that is, from a privileged, neutral and/or contextless vantage point, we have then violated the historiographical principles that require good historians to consider events, things, and people in their actual contexts (although getting that context right is *also* at stake, as we have seen).

Second, Pitt’s (1989) bifurcating of epistemic matters and aesthetic or value judgments should be rejected. This strategy is related to the strategy of privileging human agency over nonhuman agency in that it seems to presuppose that epistemic judgments can be tested unproblematically against the world and are hence “objective” to some degree, i.e., in learning from our mistakes. However, that Pitt suggests epistemic values are one type of value among others implies that any privileging of the epistemic is not of the same sort advocated by logical empiricists; indeed, one could argue that Pitt privileges the value side of the dichotomy, in that values provide the normative basis for taking action. It remains, however, that Pitt separates out epistemic from value judgments, and considers epistemic judgments to be relatively non-normative, if not free of norms. This strategy involves objectifying the natural world, removing it from its appropriate historical or narrative context, in order initially to make epistemic judgments that are relatively free of values or politics. Instead of such objectification, we should instead view any judgments we may make—that is, *any* knowledge claims—as constitutively normative, as Rouse (2002a)

instructs. Hence, making epistemic judgments, whether that be doing science, philosophy of science, technology assessment, or whatever, is not separable from making value judgments; scientific practices (in addition to other practices) are constitutively normative. Playing the epistemic game *is* being political (in the Foucauldian sense of power/knowledge).

Third, we must also reject the rational/irrational dichotomy that is embodied in Pitt's Common Sense Principle of Rationality (CPR). To suggest we should learn from our mistakes is laudable and eminently pragmatic, but this prescription begs the question of when to deem a present historical or narrative context as sufficiently comparable to a past context to permit one to conclude that learning may occur. In addition, if by "learning" one means some increase in the content of knowledge, and hence an implicit form of convergent epistemological realism (even if convergence is within some limited knowledge context), then one is in danger of assuming a coherence and global structure to "science" or "technology" or whatever, that begs the question of why to presuppose such a structure. Pitt, however, does not take this step; while his earlier method of technology assessment incorporates a bifurcation of the epistemic and the normative, his later focus on technological infrastructure embraces a local view of the development of changes in science. However, to imply that different narrative contexts, separated temporally, can be compared so that we can unequivocally make disinterested decisions and "learn from our mistakes,"<sup>104</sup> begs the questions of how to construct those contexts in the first place (cf. Ch. II above) and how to proceed with comparing those events without removing (i.e., abstracting or objectifying) them from their appropriate context(s) (cf. Ch. III above). Indeed, this strategy is one step away from assuming knowledge (scientific or otherwise) has a global structure that would facilitate the comparison of contexts, that is, presuming some objectified construct (e.g., background knowledge) against which other contexts may be (perhaps unproblematically) compared.

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<sup>104</sup> Pitt does not believe that the activity of learning from our mistakes—or using past knowledge to make more rational decisions in the present, in order to create the future we want—rests on a naïvely representationalist view of comparing fixed representations. His Common Sense Principle of Rationality (CPR) and Sicilian Realism, with its complex and multifaceted world of observable and unobservable entities, preclude this. His strategy does, however, bifurcate the epistemic and the normative; in addition, CPR does seem to rest on a method that incorporates transhistorical or transcontextual principles that permit *rational* comparison.



Alternatively, to adopt the naturalistic presupposition that we are always already in the world, is to hold that no such neutral, objective, ahistorical, or contextless ground exists. We are still narrative bound, and the future is always open and new and unknown, no matter how much we think we have learned from the past. Therefore, if this strategy, influenced by philosophers such as Nietzsche<sup>105</sup> and Heidegger, and novelists such as Hermann Hesse (e.g., *Siddhartha*)<sup>106</sup> and James Joyce (e.g., *Finnegans Wake*),<sup>107</sup> has any merit when fused with a thorough rejection of epistemic sovereignty, then we are led to consider that even experimental techniques, experimental systems, and their products should be included within this strategy. I return in the next chapter to the theme of how to apply the technological infrastructure concept to experimental science, extending Rouse's (2002a) naturalistic plan for how to conceptualize scientific practice. For now, let us note that even poets have expressed ideas that have relevance for the postmodern, naturalistic realization that

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<sup>105</sup> Babich (1999a) says of Nietzsche's critique of science: "It is the very emphasis on values as such that leaves science, as the prime 'value-free' value of modernity, singularly unable to determine values in practice, however counterintuitive this claim may seem for a culture that esteems science above all (as its highest value) and has become accustomed to the proclamation of the importance of instilling and reinstilling values. . . . [P]recisely where science . . . may be regarded as the highest value in a nihilistic culture, the thought of value remains fundamentally, intrinsically and incorrigibly, foreign to science" (p. 13).

<sup>106</sup> Hesse (1877-1962), in *Siddhartha: An Indian Tale* ([1922] 1999), undermines the dualisms of sense and spirit, of inner and outward self, of action and thought, of *samsara* and *nirvana*. He pleads for a dialectic of thought and action, for a unity of thinking and acting, of theory and practice. Yet, ultimately, Hesse suggests that it is action—practice—that matters most, even if one cannot do without the other. As *Siddhartha* proclaims near the end of the novel: "And look, here we are in the midst of the thicket of opinions, in the fight over words. For I cannot deny that my words about love contradict, seem to contradict, Gautama's words. That is why I so greatly distrust words, for I know that this contradiction is an illusion. I know that I am one with Gautama. How could he then not also know love? He, who recognized all humaneness in its ephemerality, in its vanity, and yet loved human beings so much that he devoted a long and arduous life purely to helping them, to teaching them! Even with him, even with this great teacher, the things are dearer to me than words, his life and deeds more important than his speaking, the gestures of his hands more important than his opinions. I see his greatness not in speaking, not in thinking, but only in doing, in living" (pp. 128-9).

<sup>107</sup> Eco ([1966] 1982) says of *Finnegans Wake*: "Joyce assimilates nature to culture and identifies what exists with what is said, the given of nature with the product of culture (the *verum* with the *factum*), and thereby conceives of the world as a dialectic of tropes" (pp. 64-65). In this novel, "everything moves in a primordial and disordered flow; everything is its own opposite; everything can coll[i]gate itself to all the others. No event is new for something similar has already happened; a *ricorso*, a connection is always possible. If history is a continuous cycle of alternations and recurrences, then it does not have the characteristic of irreversibility that we are accustomed to confer on it today. Rather, each event is simultaneous; past, present and future coincide. But since each thing exists to the extent that it is named, this whole movement, this game of continuous metamorphoses can only happen in words, and the pun . . . is the mainspring of this process" (p. 65). Indeed, "*Finnegans Wake* rebels against[t] the narrow-mindedness of modern methodologies which permit us to define only partial aspects of reality, thus eliminating the possibility of an ultimate and total definition" (p. 83).

objectifying historical or narrative contexts has its dangers. In this dissertation, I want to apply to experimental practice what T. S. Eliot here expresses regarding time:

. . . There is, it seems to us,  
At best, only a limited value  
In the knowledge derived from experience.  
The knowledge imposes a pattern, and falsifies,  
For the pattern is new in every moment  
And every moment is a new and shocking  
Valuation of all we have been. We are only undeceived  
Of that which, deceiving, could no longer harm.  
In the middle, not only in the middle of the way  
But all the way, in a dark wood, in a bramble,  
On the edge of a grimpen, where is no secure foothold,  
And menaced by monsters, fancy lights,  
Risking enchantment. Do not let me hear  
Of the wisdom of old men, but rather of their folly,  
Their fear of fear and frenzy, their fear of possession,  
Of belonging to another, or to others, or to God.  
The only wisdom we can hope to acquire  
Is the wisdom of humility: humility is endless.<sup>108</sup>

Finally, we should reject Pitt's privileging of the performative idiom over representationalism. Pitt's (2000) notion that technology is before science, because acting is prior to knowing, suggests a curious dichotomy: "We act first, learn from our actions, then try again" (p. 104). His belief that "[t]he need to know derives from the need to act better" (*ibid.*) seems to suggest a deep-seated human need to rank actions on a higher biological or evolutionary scale than the desire for knowledge. Whatever the scientific merits of this claim, Pitt's desire to place discussions and analyses of technology at the forefront of discussions of science does not require this claim. The "Turn to Practice," "The New Experimentalism," and other developments by philosophers of science (including Pitt) and others to reevaluate the significance of technology for science, has started a trend in

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<sup>108</sup> "East Coker," (1940) lines 81-98, in *Four Quartets* (©1943, 1971), San Diego: Harcourt, Inc., pp. 26-27; reprinted by permission of Harcourt, Inc. I do not suggest that Eliot should be seen as thoroughly postmodern, although in his introduction to *The Waste Land and Other Poems*, Kermode (1998) seems to suggest that Eliot was a modernist poet who was struggling to break out of modernity: ". . . Eliot's poems were unusual and new; they had no very unequivocal ancestry, and lacked any conventional consistency of style or setting" (p. xvi). In addition, the relationships among past, present, and future are a common theme in Eliot's poetry, and his writing often breaks out of the modernist mold. One could also make the argument that Pitt is a modernist struggling to break out of modernity.

scholarship that has attracted many adherents (cf. Ch. I above). Moreover, as an historiographical prescription, privileging acting over knowing rests upon the presupposition that knowing as an activity humans do (or perform) is somehow essentially distinct from other activities. Hacking (1983), as a notable example, promoted this notion in *Representing and Intervening* and it seems to have gained wide acceptance. However, it also implicitly suggests, as do traditional philosophers of science, that “science” is constituted fully by its body of theories. In his rejection of the representationalist idiom and its resultant view that activities other than theorizing must be classified as acting (or as part of the domain of technique or technology), Pitt and others implicitly uphold the notion that “science” ought to be equated with “theorizing.”

However, for those postmodernists who aim to resist such dichotomies, this view is unacceptable. For Rheinberger (1992b), “representing *is* intervening” (p. 394, fn. 15). For Rouse (2002a), causality and normativity are “intratwined” in a way that undermines the nature/normativity dualism: “Causality must be understood as always already normative, and normativity as always already causally efficacious” (p. 183). While both Rouse and Rheinberger, in addition to Pitt, welcome the transition in the philosophy of science from an obsession with the logical evaluation of scientific theories to a focus on practice, they also reject an extreme swing to the opposite pole of the representationalist/performativity dichotomy. Blurring the boundary between acting and knowing, as an historiographical prescription, seems to be required when we carefully consider the representationalist/performativity dichotomy, especially if we are to follow the Sellarsian dictum of not privileging *a priori* any particular philosophical perspective.

### ***Technological Infrastructure as Historical and Historiographical Construct***

We are now in a position to specify how technological infrastructure acts as both an historical and historiographical construct. In this section, I first explore how the concept may be used to locate in history particular technological infrastructures that played roles in fostering scientific change. Then I consider how to deploy the concept as an

historiographical construct, that is, as a construct researchers may use to help guide the construction of narratives (i.e., of interpretations or stories about the past) involving scientific practice. In doing so, I focus on how the technological infrastructure concept incorporates and yet differs from other significant attempts to specify how science changes over time. I stress how these similarities and differences reflect the lessons explored in Chapters I, II, and III, and how this bears on postmodernism and philosophical naturalism. Finally, in the next chapter, I present a brief philosophical/ historical example of how to deploy technological infrastructure in an historiographical manner by examining a text on the history and philosophy of Einstein's Special Theory of Relativity (Lucas and Hodgson 1990). I argue that this text is an example of how *not* to think and write about science, as it violates numerous principles developed in this dissertation, and that the outcome is a distortion of historical *truth* (appropriately defined).

Finding a technological infrastructure of science in history should begin with adopting the position of *postmodern naturalism* I have developed in this dissertation. However, as that amounts to constructing an historiographical perspective, I will *use* such a perspective to specify how to do History-3, or historical interpretation, realizing that I am blurring the boundary between History-3 and History-4 (or between historical interpretation and historiography).<sup>109</sup> What is required, then, is a strategy for doing historical research that incorporates not only the postmodern principles developed in this dissertation, but also the principles of Pitt's technological infrastructure (de-modernized) that we can maintain for finding those constructs that make developments in science possible. Where to focus, or to start, historians normally consider to be a matter of convention, and dependent upon a number of possible factors, including the particulars of the available historical evidence

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<sup>109</sup> A History-3, as I specified in Chapter II, can be seen as an interpretation of some past episode in history (i.e., an historical text). The act of constructing an interpretation *presupposes* a History-4, an historiography, or a set of guidelines for constructing interpretations. Additionally, a critique of historiography, or the critique of the historiographical principles guiding an historical text (or historiographical rules in general) can be seen as a History-5, a meta-historiography. These principles are all blurred, but then they are only labels. The blurring comes in because we can consider each of them alternatively as a thing, i.e., a representation of something (an historical text, historiographical critique, etc.) *and* as a practice, i.e., the *activity* of constructing an historical text using explicit or implicit historiographical guidelines. Since we have dissolved the representational (or constative)/performative dichotomy, we also should expect that the various levels of history will break down and resist being forced into such dichotomies.

(History-1) and the chronology of events (History-2) that the historian is trying to interpret, or “explain.” I start by following Rheinberger’s (esp. 1994, 1997) prescription, based on a problematization of the global/local dichotomy, that asks us to look at the “microdynamics” of scientific experimental activity. Then, using Lelas’s (1993) notion of “science as technology” and his argument that “knowing-that” and “knowing-how” are interdependent, I argue that looking at the context of the experimental strategy for separating signal from noise, or fact from artifact, will facilitate an understanding of how to begin to construct a *technological infrastructure* for a particular episode in the history of science.

### I. Hans-Jörg Rheinberger and Experimental Systems

In a series of articles in English-language journals, Rheinberger (1992a, b, 1994, 1995a, b) presented a strikingly original interpretation of scientific research he had begun to develop in German (e.g., Rheinberger 1992) and which resulted in a book, part of the Stanford University Press “Writing Science” series, entitled *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (1997). In particular, Rheinberger (1994) presented his notion of “experimental systems,” which are systems of material entities and the actions involving such entities, and which he takes to be the “basic, functional units of scientific activity” (p. 67), each with their “own intrinsic, or *internal time*” (p. 68). As Rheinberger (1994) explains, this internal time structure

is not simply a dimension of [the experimental system’s] existence in space and time. It characterizes a sequence of states of the system insofar as they can be considered to undergo a continuing cycle of nonidentical reproduction. Research systems . . . are characterized by a kind of differential reproduction by which the generation of the unknown becomes the reproductive driving force of the whole machinery. As long as this works, the system so to speak remains “young.” “Being young,” then, is not here a result of being near zero on the time scale; it is a function of the functioning of the system. The age of such a system is measured by its capacity to produce differences that count as unprecedented events and keep the machinery going. (p. 68)

Therefore, for Rheinberger (1994), when dealing with “the movement of material systems, systems of things, or systems of actions, time as an operator is not simply an axis of extension but a structural, local characteristic of any system maintaining itself far from

equilibrium” (p. 68). Hence, when describing research programs, Rheinberger (1994) wants us to view them as “network[s] of experimental systems” each possessing its own internal time as long as it can continue to produce unprecedented events (p. 69). These systems “get linked . . . not by stable connections but rather by possibilities of contacts generated by the differential reproduction of the systems and the constellation of their ages” (*ibid.*).

Moreover, this “constellation of differently aged systems constitutes a particular field of the possible. In this field, attractors constantly shift; there is no longer a fixed center” (*ibid.*).

Several points are immediately evident. First, Rheinberger’s (1994) focus on science as a “future-generating device” (p. 70) with networks of experimental systems functioning as “fields of the possible” anticipates Rouse’s (2002a) notion of *real possibility* with its futural configuration. Both authors embrace the contingency of history and the futural trajectory of taking action in the world, although Rouse (2002a) emphasizes causality and the normativity of practices, while Rheinberger (1994) wants to model how scientists (and historians) create *representations*, even if representation “may be taken to be equivalent to bringing epistemic things into existence” (Rheinberger 1997, p. 107). In addition, Rheinberger (1994) focuses explicitly on narrativity (citing White, Derrida, and Goethe) and believes that “the recombination and reshuffling of and within experimental systems is a prerequisite for producing stories from other stories. . .” (p. 72). Indeed, we can see Rheinberger’s (1994) networks of experimental systems to be forms of “narrative fields,” the construct on which Rouse had long focused but then omitted in his system of philosophical naturalism (Rouse 2002a). For the technological infrastructure concept, what is important is that the prescription of looking at experimental systems specifies that such systems or fields of systems make science possible by “differential reproduction,” that these systems have their own internal time structures, and that they are narrative in nature. That is, experimental systems, if successful, allow or produce new and productive research systems—*they make the doing of science possible*, while at the same time they constitute it at the level of experimental practice—and this is the significant feature of Pitt’s technological infrastructure of science.

In advocating Rheinberger’s focus on experimental systems as the starting point for

locating historically a technological infrastructure, I am also endorsing Pitt's prescription for initially placing priority on the epistemic context of scientific developments. However, I also endorse Rouse's view of "epistemic politics," that is, the notion that the epistemic and the political are intertwined, or that engaging in epistemic considerations *is* being political (see Ch. III above). The process by which an "epistemic thing" becomes an established "fact" is not wholly controlled by the "behavior" of the epistemic thing; it also involves the broader technological infrastructure, and such considerations may turn out to include research norms, laboratory politics, federal politics, business considerations, regulatory norms and politics, and so on. Nevertheless, since scientific research practices normally involve scientific experimentation—the quest for answers to questions about the natural world, which may generate new questions awaiting answers—looking at the microdynamics of experimentation and focusing on the practices of scientists is consistent with the postmodern perspective developed in this dissertation (cf. Rouse 1991c, Fine 1986). What is imperative, though, is that this focus on experimentation should be filtered properly through the lens of a postmodern narrativity.

Rheinberger (1994) calls his narrative perspective *historiality*, a term he borrows from Derrida ([1967] 1976). The main characteristics of Rheinberger's (1994) *historiality*, as an orientation for describing experimental systems, are

1. a commitment to the radical contingency of history,
2. an acceptance that the recurrence required when (re)considering past developments (including those involving the participants guiding experimental systems) involves an inevitable distortion of the "past,"
3. the realization that successful research programs involve (a series of) experimental systems that are able to generate new questions, answers, and even new systems, and
4. a recognition that these systems must be analyzed with a non-linear epistemology of time (see esp. pp. 69-72).

I consider each of these points in turn.

i. *Historiality and the Contingency of History*

Regarding the contingency of history, Rheinberger (1994) argues that “there can be no global foresight;” he quotes the biologist François Jacob,<sup>110</sup> who characterized the scientific research process this way:

What we can guess today will not be realized. Change is bound to occur anyway, but the future will be different from what we believe. This is especially true in science. The search for knowledge is an endless process and one can never tell how it is going to turn out. Unpredictability is in the nature of the scientific enterprise. If what is to be found is really new, then it is by definition unknown in advance. There is no way of telling where a particular line of research will lead. (quoted in Rheinberger 1994, p. 69)

This acceptance of the contingency of history meshes well with the view of narrativity developed above in Chapters II and III. The literary views of White (1981a) and Kermode (1967), as well as the futural orientation of Rouse’s (2002a) *real possibility*, all deny the predictability of the future and grant that the past “could have been otherwise.” What is significant here is that Rheinberger (1994) wants us to consider the “impossibility of any algorithm, of any logic of development that is ontologically or methodologically grounded” when considering the functioning of experimental systems in history (p. 70). For the technological infrastructure concept, this means that when we look to history for a technological infrastructure, we must reject the view that later developments are (were) the inexorable “result” of earlier events, or that prior events “caused,” in the sense of an in-principle predictability, the later developments. The future is open; we should accept that the “present as the future of the past is not a ‘result’—whatever that means—of the past; the past is the result of a future—its presence as a surrogate”<sup>111</sup> (*ibid.*). That is, we are continually (re)constructing (in the present) the past in order to create a possible future. Or, alternatively, what is now (only) a possible future will at some time (in the future) (re)create the past, which will take the place of what is now the present, and hence create a “distortion” through the use of “regulative fictions” that are needed to model or (re)present that slice of the past.

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<sup>110</sup> François Jacob, *The Possible and the Actual* ([1981] 1982), p. 67.

<sup>111</sup> See Chapter I above, for more discussion on this.



ii. *Recurrence and Retrospective “Distortion” of the Past*

Rheinberger’s (1994) notion that “recurrence”<sup>112</sup> is inherent in the research process, both at the level of scientific research and at the level of historical retrospection, again recalls the discussions of historiography in Chapters II and III. Rouse’s and Ricoeur’s explorations of Heidegger’s *Umsicht* and *Geschichtlichkeit* suggested that assessing one’s present circumstances and their relations to past developments is an activity that has a crucial bearing on how one acts to construct a future. Rheinberger (1994) aims to apply this kind of “nonlinear” thinking to experimental systems and argue that one is forced, when considering the origin or emergence of an epistemic thing, to use the outcome of the story (the ending) in that assessment of its “beginning.” That is, when one is undertaking *Umsicht* (the circumspective concern of the present situation) and/or *Geschichtlichkeit* (the weight of the past or common destiny) with respect to an epistemic thing, *recurrence* (a retrospective (re)search across a temporal breach) is required or inherent in that activity. Hence, one “requires the product in order to assess the conditions of its production” (p. 66). Moreover, regarding experimental systems, such *historial* thinking must “assume that recurrence works in the differential activity of the *system* that is *itself* at stake, and in its time structure” (*ibid.*). Consequently, the “recent, so to speak, is the result of something that did not happen. And the past is the trace of something that will not have occurred” (p. 67).

A significant point here is that in assessing a past development of the production of what turned out (i.e., in the future) to be a significant scientific moment (e.g., the discovery or invention of a constituent of nature), one is forced to use knowledge that was not known at the beginning of the story. One has used the end of the narrative, indeed requires the end, to tell the story of something *new* that was “nothing but an irritation at the point where it first

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<sup>112</sup> Rheinberger (1997) notes that in the context of art history, George Kubler has emphasized the notion of recurrence by stressing what André Malraux has called the “Eliot effect”—after T. S. Eliot—as a significant concept in evaluating works of art: “Every major work of art forces upon us a reassessment of all previous works” (quoted on p. 178). See George Kubler (1962), *The Shape of Time: Remarks on the History of Things*, p. 35. Interestingly, Umberto Eco ([1983] 1994) regards what he calls the “ironic rethinking” of the past to be one of the main reasons to call someone or some work postmodern: “The postmodern reply to the modern consists of recognizing that the past, since it cannot really be destroyed, because its destruction leads to silence, must be revisited: but with irony, not innocently” (p. 530). Perhaps this is another reason to see Eliot as postmodern, or at least struggling with modernity. Eliot’s poetry is marked by a reflexive recognition of how it uses and reacts to earlier works.

appears. . .” (p. 66). Therefore, to construct such a narrative, a distortion or “regulative fiction” is unavoidable in order to tell a story that has meaning. For the technological infrastructure, this means that one must be aware of the complexities of *temporal separation* (or temporal mediation) when considering how to locate a technological infrastructure in history and to tell its story, especially when one is describing it at the level of the experimental system.

There are varieties of theoretical perspectives regarding the problem of temporal separation. David Carr (2001), in his analysis of agents’ points of view in historical writings, rightly argues that historians should take seriously historical agents’ past perspectives of their own space and time (their spatial and temporal contexts) and that philosophers of history have not paid enough attention to these features. However, Carr (1986, 2001) advocates the view that historians do *not* distort the past by presenting it in a narrative form, as White (1981a) and Kermode (1967) have argued. Instead, Carr (2001) believes that the narrative structure of an agent’s actions is a “structural element of our relation to the world and our use of language to describe it” (p. 167). This distinction, between narrativity as a structural feature of our being in the world and narrativity as a form imposed on the (real) events of the past, was explored in Chapters II and III above in the contexts of representations of the past and of how humans make decisions, at least in part, based on assessments of the space and time considerations (i.e., *Umsicht* and *Geschichtlichkeit*) present in a given context. Ricoeur’s (1981) view on narrative, as I presented it in Chapter II, was a phenomenological view that comes close to Carr’s structuralist view but ultimately resists its extreme structuralism.<sup>113</sup> Indeed, Ricoeur (1984) suggests that White’s (esp. 1978) *tropological* analysis of the relation between history and the past stands as a “decisive contribution” to such analyses, but that “the recourse to the theory of tropes runs the risk of erasing the

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<sup>113</sup> Ricoeur (1984) states that “a certain tropological arbitrariness must not make us forget the kind of restraint that the past exerted on historical discourse through known documents, by demanding an endless *rectification* on its part. The relation between history and fiction is certainly more complex than we can ever say. Of course, we must combat the prejudice that the historian’s language could be made entirely transparent, so that the facts would speak for themselves; as if it were enough to get rid of the *ornaments of prose* in order to do away with the *figures of poetry*. But we would be unable to combat this first prejudice if we did not at the same time combat the second, according to which the literature of imagination, because it constantly makes use of fiction, can have no hold on reality. These two prejudices must be fought together” (pp. 34-35).

dividing line between *fiction* and *history*”<sup>114</sup> (p. 33). Moreover, as I argued in Chapter III above, Rouse’s (2002a) view on retrospective judgments suggested that *representations* of the past, insofar as they are performances undertaken with something at stake in the present (i.e., for the historian attempting to reconstruct the past), cannot be separated from the historian’s context in the present; as he argues:

One can ask intelligibly whether the normative authority of other ways of thinking and acting is conceivable, given what is at stake on one’s own practices. One cannot ask in the same way, however, what normative configurations would be intelligible if these other norms were in fact our own . . . (p. 345)

In other words, Rouse (2002a) seems to be advocating the notion that historians cannot, apparently even in principle, make meaningful judgments about the past if the normative authority operating then was different from the historian’s norms in the present. How do we come to grips with these seemingly divergent views, and how do they bear on technological infrastructure?

One way to look at the problem of historical representation is to look again at the representational/performativity and reality/fiction dichotomies. Of the theorists considered in this dissertation, White has perhaps best problematized these dichotomies and resisted a swing to either pole. However, it has been common to criticize White for overstating the literary dimensions of narrative history and for arguing that because the representation of the historical past involves the “regulative fictions” that Kermode (1967) so eloquently described, historical reality (History-0) is unattainable or at best ephemeral.<sup>115</sup> Ankersmit

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<sup>114</sup> Ricoeur (1984) notes that White “is not unaware of this danger.” He states that “White is not too far away from what we mean by the *interweaving reference* of fiction and of history. But, as he does not really show us what is realistic in all fiction, only the fictive side of the purportedly realistic representation of the world is stressed” (pp. 50-51, n. 37). For Ricoeur (1984), White should be seen as the theorist who has put us on the right track for dealing with the relationship between History-3 and History-0, between historical representations and actual history, and White’s elucidation of the tensions between these marks him as one of the classic historiographers (pp. 32-36).

<sup>115</sup> Carr (1986, pp. 8-9, 11-16) is one example; Marwick (1995) and Rouse (1990, p. 185; 1996b, p. 165) are others. Carr (1986) criticizes White (1981a) for asking what “The Value of Narrativity in the Representation of Reality” is; he believes White “concludes, in essence, that its value is nil” (p. 11). I argue that this is a gross misreading of White’s (1981a) essay; White does not argue that narratives cannot get at historical reality. White (1981a) *does* argue against the naïve view of historical representation, according to which historical reality speaks for itself in correctly constructed and objective narratives: “It is the historians themselves who have transformed narrativity from a manner of speaking into a paradigm of the form which reality itself displays to a ‘realistic’ consciousness. It is they who have made narrativity into a value, the

(1998) shows that such criticisms are misreadings of White's work, and he believes that White is actually an "unrepenting structuralist" (p. 185) who argues that historical reality "is only encountered in our attempts to define our relationship to our past, in our attempt to 'write ourselves' by writing history" (p. 193). Historians, for White, can say something about historical reality, but this reality "is not something that we stumble upon in the way that we may find out about the chairs and tables in a room that we have just entered—that is the misguided pseudo-positivist model . . ." (Ankersmit 1998, p. 193). What we must do, instead, is follow White's "exhortation . . . that we must not cowardly shun its boundaries, but always courageously probe and explore the area where the discipline [of history] begins to lose its grasp. For it is there that we encounter past reality. . ." (*ibid.*, p. 192).

In other words, by reflectively and reflexively realizing that probing the tension between the poles of the representational/performativity and reality/fiction dichotomies—that is by adopting a postmodern perspective—the historian can *represent* something of the reality of the historical past. This is not the naïve realist view of the correspondence of History-3 to History-0, but neither is it entirely unconstrained. The constraints are History-1 and History-2, the documentary evidence and the chronology of events of von Ranke, whether contained in written discourse, oral accounts or stories, or a group's *Geschichtlichkeit* or common view of its past. Returning now to Rheinberger's (1994) contention that a "distortion" of the past is inevitable when telling stories about past experimental systems, we find that Rheinberger probes the above dichotomies when showing how retrospective views of experimental systems seem to have such a distortion built into them. This "spontaneous history of the scientist" (p. 77) reflects the narrative nature of scientific research:

At a given moment and in a given research process, what, say, a microsome or a virus "represents"—in the sense of how it is "produced," how it is "brought forth"—is an articulation of graphemes traced and confined by the procedures of the research process. Thus what André Lwoff of the Pasteur Institute of Paris had to say about

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presence of which in a discourse having to do with real events signals at once its objectivity, its seriousness, and its realism" (p. 23). White, like T. S. Eliot, can be seen as a modernist struggling to break out of modernity. Ricoeur's (1984, pp. 27-36) presentation of White's work suggests this point. Moreover, if one defines postmodernity as does Spiegel (1992) and as I have done in this dissertation, then at least some of White's work can be seen as postmodern.

viruses in 1957 is not to be read as a tautological joke but precisely points at the argument I am trying to make: “Viruses should be considered as viruses because viruses are viruses.” Here the signified has been crossed out and the reference itself has become a signifier—which is the essence of narration. (p. 77)

Representation is what scientists try to *do*, whether by constructing a research system, constructing a theory, or by *Umsicht* (the appraisal of the current state of a research program or problem) or even *Geschichtlichkeit*. We must look at such representation for what it is: the research activity of the scientist in its appropriate narrative context(s). An anti-representational stance will not do. To suggest, as does Rouse (2002a), that one cannot meaningfully reconstruct a past account of normative authority, because those past norms are not our own (i.e., binding on us), is to suggest that the kind of historical investigation that *aims* at the reality of the historical past, which White and others advocate, is either impossible or entirely unconstrained (as Rouse argues)—that is, one would have to collapse the distinction between reality and fiction. Nevertheless, historians (and humans in general) *do* aim to represent the past, and in particular they *do* aim to reconstruct the normative authority operating in a given context. Yes, they cannot hope to do so in a final, definitive way (although the possibility of a definitive history is a common misconception among nonhistorians and many historians; cf. Ch. II above), but there are constraints upon the answer: the goal of reconstructing adequately the norms of the past *is* something *at stake* for the historian in the present, and our own similar (or different) experiences in the world, in conjunction with careful and skilled analysis of documentary evidence, are some of the tools historians (and scientists and humans in general) have potentially at their disposal (although this will not, as Rouse argues, result in making those past norms our own) in order to say something about historical “truth.”<sup>116</sup> To suggest that one cannot reconstruct past normative

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<sup>116</sup> This is the sense of truth indicated in footnote 2 above, where I suggested that “reverence for truth” (Woodruff 2001), as an activity guided by a Nietzschean or Foucauldian form of “truth,” is the virtue needed to construct good histories. On the traditional dualist, representationalist version of truth, Nietzsche ([1873] 2005) wrote the following: “What then is truth? A movable host of metaphors, metonymies, and anthropomorphisms: in short, a sum of human relations which have been poetically and rhetorically intensified, transferred, and embellished, and which, after long usage, seem to a people to be fixed, canonical, and binding. Truths are illusions which we have forgotten are illusions; they are metaphors that have become worn out and have been drained of sensuous force, coins which have lost their embossing and now are considered as metal and no longer as coin” (p. 17). We can, however, transform into a virtue the moral value of the search or aim for such

authority because those past norms are not our own begs the question of how we *know*, in the present (granting that we need not be aware of any norms for them to be *actually* operating, as Rouse suggests) here and now, what norms are operating, or how we might go about finding out what they are. Are we really to conclude that we can only know about normative authority when those norms are presently acting upon us? Can Rouse reflexively apply his own model of normative authority to his own construction of a narrative of normative authority?

How Rheinberger (1994) uses historicity to describe experimental systems and research activity in general, presupposes that one *can* compare retrospective accounts of past developments and then make judgments, based on the documentary evidence, on which accounts are better stories about what happened. This point is required when considering his “recurrence” and the distortions that ironically result when attempting to model the past. On the one hand, we should want to say that we can make meaningful judgments about past norms; but on the other hand, it seems that recurrence, since it *is* from a perspective (a future) of *different* information, discursive practices, material entities, and norms, will result in distortions of that model of the past.<sup>117</sup>

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a traditional view of truth by adopting a viewpoint that reflectively and reflexively incorporates Nietzsche’s deconstruction of that traditional version of truth. Medina and Wood’s (2005) volume provides an excellent collection of philosophical and other versions of the varieties and pluralities of the concept we call “truth,” including those of Foucault, Rorty, Derrida, and the American pragmatist, William James ([1907] 1981, lecture VI).

Regarding scientific research, the virtue of reverence for truth is maintained through many means, including, for example, controlled clinical trials, the peer review process, and expert-based government and independent regulatory and advisory bodies, such as the National Academy of Sciences of the United States (NAS), the U. S. Government Accountability Office (GAO), and the U. S. Food and Drug Administration (FDA). However, as recent events have shown, these institutions and processes are not infallible. The NAS has been overruled by the Department of Energy (and the White House) on the issue of the safe storage of nuclear waste, and the FDA has failed as an expert-based regulatory body to prevent dangerous medical drugs from entering the U. S. market. See, for example, Shankar Vedantam, “Storage of Nuclear Spent Fuel Criticized: Science Academy Study Points to Risk of Attack,” *The Washington Post*, 28 March 2005, p. A1; Marc Kaufman, “FDA is Criticized Over Drugs’ Safety Problems: Response to Approved Medications Cited,” *The Washington Post*, 24 April 2006, p. A5; U. S. Government Accountability Office, “Drug Safety: Improvement Needed in FDA’s Postmarket Decision-making and Oversight Process,” GAO-06-402, March 2006.

<sup>117</sup> Nietzsche ([1887] 1990) put it this way: “There is no set of maxims more important to the historian than this: that the actual causes of a thing’s origin and its eventual uses, the manner of its incorporation into a system of purposes, are worlds apart; that everything that exists, no matter what its origin, is periodically reinterpreted by those in power in terms of fresh intentions; that all processes in the organic world are processes of outstripping and overcoming, and that, in turn, all outstripping and overcoming means reinterpretation,

Why does this matter? One reason it matters is that it has bearing on *how* to construct appropriate interpretations of the past. That, in turn, is important because how we construct them will have bearing on the future, that is, on future practices (for example, on our cultural view of how science functions as a cultural practice). Moreover, if Rheinberger is right, how such narratives are constructed will also have bearing on the future of the work in the scientific research activities itself. That is, the narratives scientists believe (think again of *Umsicht* and *Geschichtlichkeit*) concerning their field, their research programs, their experimental techniques, and so on, will affect how the research programs are carried out, as Burian (e.g., 1996, 1997) has argued convincingly for molecular biology. Hence, *how* the past is reconstructed has bearing on issues involving *science policy*: accounting for and skillfully handling the “distortions” that inevitably result from retrospectively reconstructing the past can be seen to *constitute* a component of scientific activity itself, whether at the level of experimental practice (e.g., in the laboratory) or at the level of regulation and policy-making (e.g., government regulation and/or institutional or governmental policy). This feature of narrativity is explored below in Chapter V.

### iii. *Experimental Systems as Generators of the “New”*

Rheinberger’s (1994) view that experimental systems, in order to be successful, must generate the new, is fleshed out in connection with his appropriation of the Derridean notion of *différance*. For Rheinberger (1994), successful experimental systems can be characterized by *différance*, which Derrida describes as follows:<sup>118</sup>

Everything in the design of the *différance* is strategic and bold. Strategic, because no transcendental truth present outside the field of writing is able to dominate theologically the totality of the field. Bold, because this strategy is not a simple strategy in the sense in which one says that the strategy guides the tactics to a certain end, toward a telos or the motif of a domination, a dominance and a definite reappropriation of the movement of the field. A strategy, finally, without finality; one could call this *blind tactics, empirical roaming around*. (quoted in Rheinberger 1994, p. 71, his translation and emphasis)

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rearrangement, in the course of which the earlier meaning and purpose are necessarily either obscured or lost. . . I realize that this truth must distress the traditionalist. . .” (p. 209).

<sup>118</sup> Jacques Derrida, “La différence,” in *Marges de la philosophie* ([1968] 1972), quotation from p. 7.

The characteristics of experimental systems Rheinberger wants to emphasize are, ironically, “reproductive stability” and “sufficient sloppiness” (p. 71). Since what the researcher is trying to do “is to produce results that by definition cannot be produced in a goal-directed way” it follows that the “unknown is something that cannot be approached straightforwardly precisely because one does not know what is to be approached” (p. 70). In addition, Rheinberger grants agency to the natural world in the sense that the “new” is not simply the imagination or belief of the experimenter (thus underscoring his posthumanist view of the agency of nonhuman actors).

The significant point here for technological infrastructure is that the capacity of an experimental system to produce some *new* thing, entity, or concept seems to be a requirement for the system to be successful. That is, scientific change is constituted by experimental systems that have the capacity to continue to produce novel things. These things, which Rheinberger (1994) calls “material, graphic entities” (p. 71) when they are in the process of being elucidated by an experimental system,

contain the possibility of an *excess*. They contain more and other possibilities than those to which they are actually held to be bound. The excess embodies the historical movement of the trace: It is something that transgresses the boundaries within which the game appears to be confined. As an excess, it escapes any definition. On the other hand it brings the boundary into existence by cutting a breach into it. It defines what it escapes. The movement of the trace is recurrent. The present is the future of a past that never happened. (p. 71)

Rheinberger’s (1994) experimental systems are a first approximation at identifying the technological infrastructure of science—that which makes successful science possible. Interestingly, Rheinberger’s view sounds close to what Lakatos (1978a) had in mind with his requirement that “progressive” research programmes must produce novel facts. Notice, however, the difference in frameworks, presuppositions, and ultimately the means by which science changes, between what Rheinberger (1994) is advocating and Lakatos’s (1978) view of “research programmes” and his “rational reconstructions” of science; between the postmodern view developed in this dissertation and the modern, rationalist view developed by Lakatos and so many other philosophers and historians of science until fairly late in the



twentieth century.<sup>119</sup> The difference in frameworks *does* matter. It matters because how we think about science—and by “we” I mean not only historians or philosophers, but also scientists themselves and the general public—affects how we act concerning scientific issues. Professional and other cultural views of science, through the stories we tell ourselves about science, have immense importance for future developments, whether technical, educational, political, popular, or otherwise.

iv. *Experimental Systems and Nonlinear Epistemology of Time*

Regarding Rheinberger’s (1994) epistemology of time, it is evident that the modern, rationalist view of history that has identifiable, determinate causes operating will no longer do. A more complex view of time, one that takes into account the nonlinear aspects of the relationships among the past, present, and future, must be incorporated into any account of experimental systems that takes seriously both the postmodern worldview (as here developed) and the kind of radical philosophical naturalism advocated by Rouse (2002a). The presumption that our experience of time is best characterized as a linear continuum of events is a presumption in need of deconstruction; what is the foundation for assuming that time “flows” or “passes” equably? If our experience of the “passage” of time is related to or dependent upon narrative context, that is, the stories we tell ourselves, then it seems we should entertain the notion that different narrative contexts may have different temporal metrics associated with them. For Rheinberger (1994), this applies to experimental contexts as well—that is, to experimental systems.

As for the scientists who participated in these contexts, their retrospective accounts of

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<sup>119</sup> One could argue *ad infinitum* the when, where, who, how, and why of the origin of the questioning of what Kuhn ([1962] 1970) called the “received view” of science, the modern(ist) view of science infused with some form of epistemic sovereignty. Kuhn is often cited as an origin, as are Quine (1951), Goodman (1951, 1955), Sellars (1956), Hanson (1958), and Feyerabend (1962). One could even go farther back and invoke Hempel (1965b), whose work in the 1940s and 1950s dissected many of the problems with the received view. Clearly, one would also need to point out later developments, such as British Cultural Studies in the 1960s and 70s; American Cultural Studies in the 1980s; the Sociology of Scientific Knowledge (SSK); Science and Technology Studies (STS); Cultural Studies of Scientific Knowledge (CSSK) in the 1990s (e.g., Rouse 1993b, Haraway 1994, 1997); and anthropological studies of science and technology (e.g., Traweek 1988, 1992; Downey and Dumit 1997; Downey 1998). Rouse (2005) provides an insightful review of developments in science studies in his review of John H. Zammito’s (2004) *A Nice Derangement of Epistemes: Post-positivism in the Study of Science from Quine to Latour*.

dealing with experimental systems with their own internal times results in what Rheinberger (1994) calls the “spontaneous history of the scientist” (p. 77). In such a history, “the new becomes something already present, albeit hidden, as *the* research goal from the beginning: a vanishing point, a teleological focus” (*ibid.*). In addition to highlighting how such a view results in “distortions” of the past, the notion of the spontaneous history of the scientist also illustrates why a nonlinear view of time is necessary. How scientists make judgments about what systems are “new” is not always a matter of how recent the system is, in the sense of the linear passage of time. From the point of view of the scientist, it is, as Rheinberger (1994) describes, a function “of the functioning of the system” (p. 68). The system, as we have seen, is appropriately described in terms of narrativity. According to Rheinberger (1994),

the retrospective view of the scientist as a spontaneous historian reminds us of the following: An experimental system has *more stories* to tell than the experimenter at a given moment is trying to tell with it. It not only contains submerged narratives, the story of its repressions and displacements; as long as it remains a research system, it also has not played out its excess. Experimental systems contain remnants of older narratives as well as fragments of narratives that have not yet been told. Grasping at the unknown is a process of tinkering; it proceeds not so much by completely doing away with the old elements or introducing new ones but rather by *re-moving* them, by an unprecedented concatenation of the possible(s). It differs/defers. If in the spontaneous history of the scientist the latest story appears always as the one which has already been told, or that at least has been tried to be told, this is not a deliberate dissimulation; it reflects a process of *marginalization* that is born into the ongoing research movement itself. . . . In the spontaneous history of the scientist, the present appears as the straightforward result of the past pregnant with what is going to be. Strangely enough, in a kind of double reversion, it inevitably also presents the new as the result of something that never happened. The historical, without realizing it, obeys and discloses the figure and the signature of the historical. (pp. 77-8)

*Historiality*, then, is an historiographical stance that is to replace historicity, the traditional view of the contingency of history (D’Amico 1989). With historiality, linear time is deconstructed and a nonlinear time is embraced as a way to properly and honestly tell a story, here about scientific experimentation. Historiality is needed in order to properly locate and situate experimental systems, to account for their propensity to facilitate new developments in the research program, and hence to conceptualize technological infrastructures—that which is needed not only for scientific practice to occur, but also for it to be successful in the

future. To further explore how to locate technological infrastructures in history, I turn to where I began in Chapter I, the issue in philosophy of experiment of separating signal (or entity) from noise (or artifact)—that is, the research goal of the scientist (cf. Galison 1987, 1997).

## II. *Separating “Signal” from “Noise”*

Philosophers of scientific experimentation typically present the process by which scientists separate signal from noise as a salient feature of scientific experimentation. In a recent volume of essays entitled *The Philosophy of Scientific Experimentation* (Radder 2003),<sup>120</sup> the editor noted that several themes pervaded the chapters:

the material realization of experiments; experimentation and causality; the science-technology relationship; the role of theory in experimentation; modeling and (computer) experiments; and the scientific and philosophical significance of instrumentation. (Radder 2003, p. 3)

These themes reflect a desire “for a philosophy of experimentation as a subject in its own right” (p. 4) and show that this (sub)field has at least some adherents, even if it is “underdeveloped” (p. 2) within the broader field of the philosophy of science, despite the promising developments in the 1980s and 1990s with the “New Experimentalism” (cf. Ackermann 1989) and the “Turn to Practice” (cf. Pickering 1992). This list of themes, nevertheless, reflects a desire to account for what happens in (successful) scientific experiments that produce stable results: that is, what happens when scientists intervene in, and/or model, nature and produce (relatively) stable results that are invariably replicable. In other words, scientists conduct experiments and are able—as they would say—to separate real, stable entities (e.g., electron or ribosome) from experimental artifact, or else they are able to separate real, robust signals (e.g., the reality of the 3K homogenous, isotropic background radiation, or the statistical significance that suggests the pharmaceutical Vioxx reduces the pain and inflammation associated with arthritis without serious side effects) from

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<sup>120</sup> This volume (Radder 2003) developed out of a conference on the philosophy of scientific experimentation held at the Vrije Universiteit Amsterdam in June of 2000. The participants who contributed papers to the volume were Hans Radder, Rom Harré, Davis Baird, Peter Kroes, Jim Woodward, Rainer Lange, Michael Heidelberger, Giora Hon, Evelyn Fox Keller, Mary S. Morgan, Daniel Rothbart, and David Gooding. Joseph Rouse and Hans-Jörg Rheinberger were also participants, but did not contribute chapters (p. vii).

noise associated with the instrumentation, experimental set-up, or even statistical massaging of data. Hence, philosophers of experimentation want to answer questions involving how and why experiments *matter* in science: how and why do they result in successful scientific results?

Unfortunately, few of the chapters in the volume (Radder 2003) represent attempts at the study of experimentation without some form of epistemic sovereignty.<sup>121</sup> Moreover, only the chapter by Gooding enters waters that flow beyond traditional philosophy of science. Gooding (2003) argues that there are “modes of understanding that are inherently analogue rather than numerical or digital” and that they “are inherent in the practices even of the most technologically sophisticated experimentation” (p. 281). He suggests that qualitative methods and ways of knowing are needed in order to understand the process by which scientific knowledge is first abstracted, in how it is measured or tested using experimental apparatus, and then “expanded” when it is interpreted by humans after the experiment is performed. Gooding (2003) rejects any reductionism in accounting for the development of

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<sup>121</sup> That epistemic sovereignty is prevalent among philosophers of science who aim not only to study the philosophical significance of scientific experimentation but also call for a specialized (sub) discipline, may be attributable to the attempt to compartmentalize the study of experimental practice and isolate it from its cultural and historical contexts. Few of the chapters attempt to connect what happens in experimentation to broader cultural or historical concerns. For example, Radder focuses on “nonlocal,” or objective, knowledge produced by experiments (ch. 8); Hon believes focusing on “experimental error” will allow us to “uncover logical structures and characterize methodological principles that govern” experimentation (ch. 9, p. 176); Woodward argues that by analyzing causality and utilizing an objectivist version of manipulability theory, we can generate manipulability relationships that “will remain at least roughly the same across changes in fundamental ontology” (ch. 5, p. 113); Baird claims that because “scientific knowledge transcends the subjective beliefs and skills of any individual” we can have “public” (as opposed to community-dependent knowledge) and “objective epistemological objects”—for example “Faraday’s motor and contemporary reconstructions of it”—that scientific instrumentation and experimentation help to elucidate (ch. 3, pp. 62-3; see also Baird 2004); and Lange believes that successful scientific experimentation “transcends the local community of those who presently are members of that practice and ensures the transsubjective validity of its results.” (ch. 6, p. 136) The chapters by Kroes (ch. 4), Heidelberger (ch. 7), Fox Keller (ch. 10), Morgan (ch. 11), and Rothbart (ch. 12) are all apparently free of any explicit epistemic sovereignty, but they do not problematize narrativity or the use of history in making philosophical pronouncements, although Kroes does dissolve the natural/artificial dichotomy but without taking the next step to nature/culture; the chapter by Harré (ch. 2) is a confusing taxonomy of experimental types or configurations ostensibly aimed at countering constructivist interpretations of experimentation. Only Gooding (ch. 13) goes beyond traditional philosophy of science. He problematizes history and historiography (pp. 257-8), “essentialist views of science” (p. 261), scientific progress (pp. 262-6), and his chapter focuses on a questioning of the analogue/digital dichotomy and its implications for experimentation. Here and elsewhere, Gooding (1990; 1992, pp. 76-9) realizes that recurrence and reconstruction are involved even in experimentation: “Grasping the nettle of reconstruction means acknowledging that all accounts of experiments—even those made as experiments are done—involve reconstruction” (p. 76).

experimental scientific knowledge—for example, in contradistinction to the traditional trend in artificial intelligence, according to which “all important and interesting aspects of human thought are reducible to symbol manipulation” (p. 272). Moreover, he seems to reject the realism that was inherent in, but unnecessary in accounting for, his earlier view of the philosophical significance of scientific experimentation.<sup>122</sup> Nevertheless, the main thrust of the chapters in the Radder (2003) volume is to explain or account for the significance of scientific experimentation by invoking some sort of epistemic sovereignty.

Clearly, one must look beyond the discipline of the philosophy of science to find studies of scientific experimentation that go beyond epistemic sovereignty and entertain notions of narrativity. What is needed is a focus on what has been the traditional realm of the philosopher of science and experiment—that is, a focus on how scientists separate signal from noise<sup>123</sup>—combined with the postmodern naturalism developed in this dissertation. In this way, we can begin to locate a technological infrastructure of science by first locating and interpreting how scientists attempt to make some kind of “connection” between theories or hypotheses, on the one hand, and the natural world or models of the natural world (including the biological), on the other hand. Then, after focusing on the microdynamics of scientific practice, one can expand the search and look outside the laboratory to situate the research

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<sup>122</sup> Gooding (1990) provides a philosophy of experimentation in which he strives to steer a ground between social constructivism and reductionism, by focusing on the role of human agency in scientific experimentation. He seems to want to grant agency to the phenomena of nature (p. 217); he believes experimental practices are “particular and local” (p. 190); he rejects the notion that knowledge converges to truth or approximate truth, or that crucial experiments are possible (p. 181); yet he rejects the “purely sociological” (p. 211) view, for example that of Collins (1985), for which the consensus of the scientific community fully explains why experiments end. For Gooding (1990), the constructivist account “allows no room to explain the practical success of science” (p. 213). However, Gooding develops an account of “asymptotic realism” in which there is a convergence of practice—experimental manipulation and theoretical practice—to “reality,” but not a “given reality that is simply disclosed. . .” (p. 187). The “reality” is a reality that is “construed” by scientists from their manipulations of nature in experimentation. While Gooding (1990) calls this a “pragmatic realism” (p. xv), he primarily focuses on *human* agency and not on material agency. Ultimately, he is unclear on the relationship between the “construed reality” of the experimenter and the actual phenomena of nature; hence, he is neither clear nor specific on how his view goes beyond constructivist views by showing how the natural world plays a role in experimentation or in how scientific knowledge is in some way constrained by the agency of the natural world.

<sup>123</sup> Weber (2005) attempt to do this for experimental biology; he appropriates the work of Rheinberger and other Continental philosophers of science. However, he defends a position that locates epistemic sovereignty in scientific methodology, and then advocates a form of realism based on that position (see Ch. I above, p. 10, fn. 6).

activity in its appropriate historical and cultural context(s). Scientific practices do not reveal fixed, definitive time-transcendent principles, as many traditional philosophers of science who focus on theories and on the logical evaluation of the outcomes of experiments, would have us believe. They are sustained by components of their technological infrastructures that extend outside the laboratory; those infrastructures are themselves local, context-bound, contingent, and hence, narrative in nature. They involve stories waiting to be told.

Moreover, when research systems or experimental systems become “stabilized” or “black-boxed,” as Latour (1987) would say, and become extended in some way outside the laboratory—for example, a standardized lab technique, a commodified research instrument, a physical or biological entity, or a marketable consumer product—then one must look outside the laboratory and farther out into society for elements of the technological infrastructure making the development possible. Furthermore, this outward movement from the laboratory invites consideration of science and technology policy and even wider cultural and political considerations, since one will often have to consider cultural elements of the technological infrastructure that are more “social,” as with funding sources, government regulatory agencies, business interests, and federal oversight bodies, such as the Joint Committee on Atomic Energy of the 1950s and 60s, or the Senate Finance Committee of today. Ultimately, any dualistic distinction between the technological infrastructure, on the one hand, and the broader cultural context, on the other hand, breaks down. Scientific practices are, after all, practices performed by people who exist in particular contexts, and the products of those practices—whether they be experimental methods, biological entities, physical theories about the microstructure of spacetime, or knowledge claims about the effectiveness of prescription drugs, the safety of medical procedures, or the urgency of the need to take seriously scientific data on the global warming of the earth—exist within those contexts. And these products *matter to us*, to at least some of us. There is something *at stake* in how the entity or the knowledge claim is involved in the practices of many types of people, from scientists to government officials to private citizens.

## CHAPTER V

### Synthesizing *The Technological Infrastructure of Science*

Habe nun, ach! Philosophie,  
Juristerei und Medicin  
Und leider auch Theologie  
Durchaus studiert, mit heißem Bemühn.  
Da steh ich nun, ich armer Tor,  
Und bin so klug als wie zuvor!

—Faust<sup>124</sup>

In this chapter, I reconceptualize Joseph Pitt’s *technological infrastructure* concept, using the postmodern tools developed in Chapters II, III, and IV above. Pitt, in arguing that philosophers of technology and science ought to focus on specific technologies—with “technology” taken to mean the pluralistic variety of forms of the “support systems” of science—when attempting to philosophically and historically analyze scientific change over time, has supplied the core philosophical/historical ingredient for this dissertation’s focus on formulating a new strategy for analyzing scientific change based on narrativity. That is, the arrow of causality in traditional history and philosophy of technology, with theoretical developments in the sciences spawning new technological innovations, is for Pitt reversed, at least in many important cases. Much of recent history and philosophy of technology, not to mention related fields, such as Science and Technology Studies (STS) and Cultural Studies, suggests Pitt’s notion that developments in various technologies make the practice of science possible, is strongly convincing. On the one hand, new technological developments often are the catalyst for new scientific research programs, and on the other hand, to find out why and how scientific developments are successful, one must look to the support systems of science, without which research in the mature sciences is not possible. Technologies are the key to the development and maintenance of research programs; look to them to find *the prospects for future research*. Much of Pitt’s work in the philosophy of technology since at least the early 1980s has pursued this research trajectory; as such it is warranted that we historically

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<sup>124</sup> Johann Wolfgang von Goethe (1749-1832), *Faust: Eine Tragödie, I. Und II. Teil* ([1808, 1832] 1958), p. 25. Part I first published in 1808; Part II posthumously in 1832.

situate it as having taken place on a parallel course with the “New Experimentalism” of the 1980s and the “Turn to Practice” of the 1990s.

After first synthesizing the technological infrastructure construct from the arguments considered in previous chapters, I then move toward specifying how to deploy the construct in practice. By way of comparison, I consider the views of Srdjan Lelas, Andrew Pickering, and Peter Galison, scholars who each have a close affinity to the principles of the technological infrastructure. I show how their arguments and assumptions reinforce and differ from these principles. Next, I show why the views of Joseph Rouse and Hans-Jörg Rheinberger remain central to the construct as developed, and also how I have gone beyond their prescriptions. Finally, I use the principles of the technological infrastructure to deconstruct a text (Lucas and Hodgson 1990) on Einstein’s special theory of relativity, a text that offers an interpretation of physical reality based on a philosophical and historical interpretation of Einstein’s work. I show how the technological infrastructure undermines the arguments in the text, and I review how its principles make a difference in interpreting Einstein’s work and the special theory’s historical significance.

### **Finding and Constructing a Technological Infrastructure**

I turn now to specifying how to find and construct a technological infrastructure, and why its construction makes a difference. What ought a researcher do to tell a story about a scientific/technological development in history, and why? In Chapter IV, when specifying how to locate historically a particular technological infrastructure, I focused on Rheinberger’s experimental systems and his focus on the microdynamics of scientific practice. Therefore (I have changed narrative strategies and now offer prescriptions), look for where and how the researcher believes signal is being separated from noise; look for the glue that ostensibly binds together the discursive world of theory with the material world of the experiment. Hence, to locate the technological infrastructure of science—following Pitt, that which makes science possible—for a development in the history of science, first look for the “connection” between theories or hypotheses and the natural world or a model of it—that



is, the experimental system in the Rheinbergerian sense, with all its *narrative* properties. Then, utilize the philosophical naturalism of Rouse (2002a) to provide the framework for how to contextualize the scientific practices. View “experimental practices as *causal* interactions” but do not view “the boundaries of causally interacting systems (objects or events) to be already determinate, without asking how such determination occurs” (p. 270). In particular, do not try to “reduce causal relations to intentional ones, but [try] to show the *inseparability of material and discursive interactions with the world*” (*ibid.*, emphasis added). Therefore, characterize “material intra-actions,” including scientific experiments, thusly:

Just as an expression has meaning only through encounters with a “body” of interpretative capacities, so objects only display definite boundaries and properties through intra-action with a body of physical capacities. Their capacities and bounds are defined by the marks they can make upon another component of a phenomenon. Like a home language, however, a measuring system does not specify an absolute anchor, but only a location relative to other material systems. (p. 275)

Moreover, view these causal intra-actions, including scientific practices, as normative, with what is at stake given by the specific context. Hence, “normative significance . . . is sustained by its

*matter*ing to all parties to get it right about what is appropriately at stake here for all of them, even though they have not yet reached, and may never reach, agreement about what that is. Moreover, it matters that these stakes be binding on everyone involved. The intelligibility of anyone’s participation in a practice turns on there being something at stake for everyone in getting it right. That does not mean that the intelligibility of practices depends upon the possibility of ultimate agreement about and conformity to what those stakes are. Rather, it depends upon a recognition of and by those to whom the practice matters, such that they (ought to) hold themselves responsible for their different interpretations and [be] accountable to one another. (p. 342)

Indeed, take scientific experimentation to be another human cultural activity with no *a priori* privileged epistemic ground. The normative authority of the particular context, governed by causal intra-action in the world, will provide the appropriate epistemic authority to the practices.

However, go beyond Rouse and reject anti-representationalist modes of

characterizing human activity. When Rouse (2002a) states that the “present configuration of normative force (the stakes that can intelligibly bind current practice) governs what is even conceivable” (p. 345), he seems to suggest that doing meaningful history is not possible:

One can ask intelligibly whether the normative authority of other ways of thinking and acting is conceivable, given what is at stake in one’s own practices. One cannot ask in the same way, however, what normative configurations would be intelligible if these other norms were in fact our own. (*ibid.*)

Historians, as part of their attempts to grasp at the reality of the historical past, *do* attempt to make judgments by adopting past norms “as their own,” so to speak. The historian who tries to place herself in the normative authority of a past context is, to an extent, asking what it would be like if those norms were her own. Yes, the stakes in the present will guide and constrain her (re)construction of those past norms. What *should* be the proper norm for such a reconstruction, I have been arguing, is *getting it right*. That is, the stakes are (or should be) the *truthful representation* of what the past context’s normative authority was actually like.<sup>125</sup> Conversely, they should not be consciously held political or philosophical commitments that the historian is trying to advocate in interpreting history one way rather than another (as Pitt and Sellars demanded of us). The historian will, of course, have such commitments and they may play a role in her interpretive practice. Nevertheless, a principled commitment to the reconstruction of History-0, *wie es eigentlich gewesen ist*, should be the historian’s primary goal.

Furthermore, look beyond the experimental system or research program to find the

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<sup>125</sup> Again, in advocating “truthful representation,” I specifically do not mean to resurrect any dependence on nomological necessity for deciding how an historical interpretation (History-3) matches up with actual history (History-0). I do suggest that the norms (History-4) that should operate (i.e., the History-5 I am now suggesting) should be those that use History-1 and History-2 to construct History-3, and not some preconceived political, philosophical, or other construct that is to be forced upon the historical evidence. This is not to suggest that such “preconceptions” will not be part of the normative authority operating when the historian is constructing her model of the past; answers to *that* question, however, are themselves contestable, as one would have to reconstruct *those* norms as some future time. It does mean, however, that one must reflectively and reflexively resist the urge to force the evidence to fit an *a priori* construct. As White suggested, push the boundaries of the practice of history to the point where they begin to break down, and only then will you be in a position to grasp at History-0. Rouse (2002a) seems not to be able to handle the view from narrativity that suggests that narrative contexts must be (re)constructed; there are no ready-made narrative contexts, even if we humans are always already embedded in one (or more) of them. These narrative contexts, or stories we tell ourselves, in part constitute our human culture (along with our material world). These stories, nevertheless, are contested and have to be *told*. They do not exist in some disembodied repository waiting for historians to access them.

other cultural components of the technological infrastructure of science. Not all experimental systems will be of the type Rheinberger specifies. That is, not all experimental systems will harbor epistemic things, which turn out later to mark discoveries of enduring biological or physical entities, or “technological objects.” Some will involve specifications of possible causal relationships or statistical correlations, and as such may turn out to be influenced by and/or embedded in stronger political (in the juridical *and* Foucauldian senses) contexts. Indeed, the characterization of the type of “connection” between “theory and experiment” is often a clue to where to look for salient components of the technological infrastructure. It is instructive to look at how some philosophers of science have characterized this “connection.” We can view this connection as that which provides the stability or “glue” between the discursive and material worlds of scientific experimentation, while at the same time remembering our commitment to “the inseparability of material and discursive interactions with the world” (Rouse 2002a, p. 270).<sup>126</sup> Furthermore, this connection is the beginning point for conceptualizing what it is that makes the doing of science possible—and that is Pitt’s definition of the technological infrastructure of science.

Few philosophers of science have characterized the relationship between theory and experiment in a way that problematizes the material/discursive dichotomy. Srdjan Lelas (1993) is one (philosopher and physicist) who believes an appropriate conceptualization of experimentation will show that a “theory can be considered as a condensed set of instructions of how to build an experimental apparatus, or, better, how to guide the production of experimental artefacts. . .” (p. 442). According to Lelas (1993),

*alopoiesis*, or artefact production *always* intertwines two complementary, but in some sense inverse, processes of bringing-forth the natural possibilities unrealized but ‘hidden’ in nature, and of bringing-into nature forms invented in human minds. The bringing-forth that elicits from nature its ‘readiness’ to accommodate certain alien forms never before present in nature *happens only* through bringing-into, through a

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<sup>126</sup> This blurring of the material/discursive dichotomy is largely absent in philosophical studies of experimentation, yet it is essential for Rouse’s (2002a) radical philosophical naturalism. Viewing humans as always already in the world suggests we should see discursive articulations and material interactions as intertwined in a way that one does not make sense without the other. As Rouse (2002a) puts it, “the correct application of the concept of causal interaction already involves conceptual/discursive normativity all the way down” (p. 270). Lelas (1993) argues that “just as making is dependent upon theorizing, so theorizing is dependent upon making” (p. 442). Without this state of affairs, a research program would not be successful. It must contain *the prospects for future research*.

violent attempt to impose preconceived forms onto nature. The two processes can never be completely separated. (p. 435)

While this (Heideggerian again) conceptualization would need a dose of narrativity to be made compatible with the view presented in this dissertation, the notion that science and technology are intertwined is an important point. Lelas (1993) believes that “*science discovers because it invents*” (p. 440). The significance of this lies in the attempt to articulate a scheme for accounting for how theory and experiment mutually cohere in a way that produces relative stability. In addition, it begins to articulate a further component of Pitt’s technological infrastructure: the notion that for an experimental system or research program to be successful it must be able to generate the new, whether that be new epistemic things or (at least) new experiments. That is, there must be prospects for future research.

Lelas (1993) argues that for experimentation to be successful, one needs a “theory which, in some way, contains in itself *a code of practice*, that is of experimentation. . . . [O]nly the theory [that] follows, or at least is compatible with, the structure of experiment-making can be tested by experiments” (p. 432). He notes that the matching up of theory with experiment involves layers of interpretation that precludes a positivist view of experimentation, on the one hand, yet requires some kind of glue (but not super glue) that binds the two, on the other hand:

In real life we . . . face the following situation. On one side we have a sophisticated and sometimes very complex artificial apparatus, and on the other the equally sophisticated, abstract and often very elaborate, apparatus of a theory. They have somehow to match each other, and this matching is effected through a series of interpretations and translations [that] connects the concepts of high-level theory with the quantities and terms [that] experimentalists use in designing, implementing and interpreting their experiments. (p. 441)

What provides this “match”? What binds the material and discursive components so that stability results and further experimental practices are *real possibilities*?

Table 1 below indicates how various philosophers of science have characterized the theory-experiment relationship. These constructs are candidates for the technological infrastructure of science, and many elements of them are compatible with it, while others are not. They illustrate how the philosopher believes scientists separate signal from noise, or

how they view what binds the material and discursive worlds in successful experimentation. However, to the extent that the philosopher has embraced a form of epistemic sovereignty, the account proposed cannot be made compatible with the views in this dissertation. Moreover, how to think of the relationship between theory and experiment is one of the central issues at stake philosophically and historiographically in this dissertation. Ultimately, the answer rests mainly with a careful combination of Rouse (2002a) and Rheinberger (1994, 1997), of normative causal intra-action in the world and bifurcating experimental systems, of naturalistic philosophy and historical historiography, but always with Pitt's pragmatic insights in mind.

The views in Table 1 are a selection of ideas on separating signal from noise among philosophers who have considered the importance of scientific experimentation. There are elements of all of these conceptualizations that are compatible with the technological infrastructure of science, yet all have components that must either be rejected or expanded. For example, Pickering's (1995a) concept of *the mangle of practice* is a positive development towards situating scientific experimentation in a posthumanist and postmodern context. Pickering advocates a move away from the "representationalist idiom" to a "performative idiom" (pp. 5-9) when considering scientific practices, he willingly grants agency to the material world without imparting intentionality to it, and he rejects the constructivists' reduction of material agency to human interests (esp. chs. 1, 6, 7). For Pickering (1995a),

human intentions are bound up and intertwined (in many ways) with prior captures of material agency in the reciprocal tuning of machines and disciplined human performances. The world of intentionality is, then, constitutively engaged with the world of material agency, even if the one cannot be substituted for the other. And . . . tuning can also transform the goals of scientific practice. (p. 20)

"Tuning" is how Pickering describes "the dance of agency" (p. 21) involved when human agency and material agency are "mangled" in a scientific experiment and material agency is "captured" in a successful experiment. For Pickering (1995a), "Disciplined human agency and captured material agency are . . . constitutively intertwined; they are *interactively stabilized*" (p. 17). This conception of experimentation is compatible with the technological

infrastructure of science and echoes Rheinberger's (1994, 1997) conception of the experimental system, as both Pickering and Rheinberger are posthumanist. That is, in their narratives of science and technology, they allow nonliving things to have agency (i.e., they perform and cause other things to happen) in their interaction with other human and nonhuman entities.

However, Pickering's (1994, 1995a) historiographical views are problematic in light of the technological infrastructure of science. Pickering (1995a) wants science studies (Science and Technology Studies and allied fields) to move away from the representational idiom to a performative idiom, because he believes that what is needed is a "*real-time* understanding of scientific practice. . ." (p. 14). Indeed, Pickering states that material agency is "*temporally emergent* in practice" (*ibid.*). Yet beyond accepting a strong notion of the contingency of history (p. 24) and an "irremediable historicity of scientific knowledge" (p. 33), Pickering's historiography does not help to problematize the tension between his plea for real-time understanding of science, on the one hand, and the notion that one must (re)construct past accounts of "real-time practice" in order to study them, on the other hand. In his rejection of the representational idiom, Pickering even wants us to discount scientists' own views, rather than to accept, as a first approximation, that "science" is what scientists say it is (Fine 1986) or to sufficiently problematize what scientists say (Rheinberger 1994) in order to probe its tension with a nonlinear epistemology of time: "[I]t would make no sense to bow to the scientists and incorporate their retrospection as part of our explanation" (p. 15).

More importantly, Pickering (1995a) uncritically moves from level to level (e.g., from History-3 to History-4, and vice-versa) without considering all the implications of doing so for how he means for us to incorporate his historiographical and meta-historiographical prescriptions into the construction of a History-3, an interpretation. For example, Pickering wants us to view "performative historiography" as having a radically contingent and futural configuration:

[A]s historians our business might be to explore open-ended transformations of science and society in terms of the temporally emergent making and breaking of cultural alignments and associations with the worlds of production and consumption, transformations understood as having no determinate destination in advance of practice. (p. 232)

**TABLE 1**

**The Glue that Binds the Material and Discursive Worlds**

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Kuhn ([1962] 1970)	paradigm/normal science fit; disciplinary matrix
Derrida ([1967] 1976)	textuality; textual reconfigurations
Foucault ([1978] 1991)	governmentality; power/knowledge
Hacking (1983)	inductively justified entity realism
Ackermann (1985)	relationship between instruments and data domains
Franklin (1986, 1990)	Bayesian probability theory
Galison (1987)	short-, middle-, and long-term cultural constraints
Latour (1987)	actor/network relationship
Cartwright (1989)	reality of capacities revealed by crucial tests
Gooding (1990)	asymptotic convergence of models of reality with theory
Lucas & Hodgson (1990)	causal influenceability
Hacking (1992)	coherence of thought, action, materials, marks
Rheinberger (1994, 1997)	relationship between experimental systems and epistemic things
Pickering (1995a, b)	the mangling of human and material agency
Burian (1996)	interaction of mechanisms, structures, functions, at many levels
Galison (1997)	trading zones among local scientific cultures
Salmon (1998)	causal mechanical model of explanation
Rouse (2002a)	normative causal intra-action in the world
Woodward (2003a, b)	manipulability model of explanation with systematic patterns of counterfactual dependence
Baird (2004)	objectivity of thing knowledge

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However, Pickering seems not to realize that such explorations of past temporal emergence involve, as Rheinberger (1994) argued, recurrence and a hindsight the historian will find unavoidable. Moreover, Pickering even criticizes cultural studies (of science and technology) for being too “atemporal” (p. 219) in their accounts of past science, yet it is not clear how Pickering’s construction of a History-3 differs from those of cultural studies on this point. Pickering believes that cultural alignments are temporally emergent in practice, but other than specifying that cultural studies is not concerned with change in time, and instead offers relatively static mappings of ready-made culture (pp. 217-29), he does not problematize his own accounts of past developments beyond his commitment to the contingency of history and change in time.<sup>127</sup> Rheinberger’s (1994, 1997) version of nonlinear epistemology of time goes well beyond Pickering’s (1995a, b) plea for studying cultural change in real-time; furthermore, unlike Pickering’s (1995a, b), it probes the tension between real-time change and the realization that we must recur and (re)construct in order to know something, to interpret such real-time change. To this extent, Pickering’s historiography is not sufficiently reflexive, even if it aims for the type of futural orientation that both Rheinberger (1994) and Rouse (2002a) advocate.<sup>128</sup>

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<sup>127</sup> Pickering (1995a) seems to retain the sociologist’s tendency for not taking seriously the issue of reflexivity. Indeed, Pickering believes Woolgar’s (1988, 1992) and others’ concerns for reflexivity are “an intensification of the representational idiom in science studies” and that they are “symmetric about human and material agency, but in a negative rather than a positive way” (p. 11, fn. 17). That is, for Pickering (1995a), reflexivity merely adds a “deconstruction of SSK’s [the sociology of scientific knowledge] accounts of the human agency of scientists” to SSK’s own “deconstruction of scientists’ accounts of material agency. . .” (*ibid.*). It is not clear, however, why Pickering thinks this added layer of deconstruction is “negative,” especially in light of his own criticisms of SSK and especially of Collins (see esp. pp. 10-15, 25). Pickering (1995a, pp. 102-3, fn. 28) himself deconstructs Collins’ (1985) “consistent humanism” (p. 103, fn. 28), which Pickering believes causes Collins to ascribe mistakenly to human agency those happenings in science that should properly be ascribed to material agency, thereby supporting his own argument for posthumanism. For more on reflexivity in sociology, see Woolgar’s (1988) edited volume.

<sup>128</sup> Pickering (1995a) even criticizes Rouse’s (1993b) plan for cultural studies of scientific knowledge (CSSK), claiming that none of its themes “speaks directly to the real-time transformation of culture” (p. 219, fn. 7). Pickering misunderstands Rouse’s (1993b) concern for breaking the supposed boundaries between science and other aspects of culture as a call for the atemporal mapping of the “transverse connections linking science to the extrascientific world. . .” (*ibid.*). Rouse, as I argued in Chapter III above, has consistently advocated a temporal approach to understanding science, whether it be his earlier focus on narrativity or his recent focus on causal intra-action and real possibility. Rouse (1990) explicitly focused on the temporal situatedness of scientific knowledge, and this has remained a central theme throughout his career. It is interesting to note that Pickering (1995a) believes Rouse’s (1987) *Knowledge and Power: Toward a Political Philosophy of Science* is “an excellent philosophical discussion and extension of the actor-network approach” (p. 11, fn. 17). While



Galison (1987) is another scholar who has focused on scientific experimentation as the separation of signal from noise. Galison's aim is to provide an account of how experiments end, without offering "rational rules for discovery" or reducing "the arguments of physics to surface waves over the ocean of professional interests" (p. 277). Indeed, Galison rejects the social constructivist approach as well as the notion that experiments can eliminate all background and unequivocally settle questions, once and for all:

[T]he experimentalist can never, even in principle, exhaustively demonstrate that no disturbing effects are present. The world is far too complex to be parceled into a finite list of all possible backgrounds. Consequently there is no *strictly logical* termination point inherent in the experimental process. Nor, given the heterogeneous contexts of experimentation, does it seem productive to search after a universal formula for discovery, or an after-the-fact reconstruction based on an inductive logic. (p. 3)

Galison instead concentrates on the "*theoretical presuppositions*" and the "*experimental presuppositions*" (p. 4) of the scientists and their "construction of *arguments*" (p. 258) for how to eliminate background and produce convincing results, results that have a "solidity in the face of altering conditions that impresses the experimenters themselves—even when theorists dissent" (p. 259). According to Galison, physicists are convinced of their experimental results when those results exhibit an "increasing *directness* of measurement" and a "growing *stability*. . ." (*ibid.*). That is, "directness" refers to "all those laboratory moves that bring experimental reasoning another rung up the causal ladder" (*ibid.*), or it may refer to a more direct measurement of "the signal itself" (p. 260). As for "stability," Galison (1987) means

all those procedures that vary some feature of the experimental conditions: changes in the test substance, in the apparatus, in the arrangement, or in the data analysis that leave the results basically unchanged. . . . Each variation makes it harder to postulate an alternative causal story that will satisfy all the observations. (*ibid.*)

For Galison, "[p]rocedures, designs, interpretations, and data acceptance all fashion the end of an experiment" (p. 257).

As compatible as Galison's (1987) historical analyses may be with the technological

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Rouse (1987) did find parallels between his own hermeneutical approach and actor-network theory, this work is much more than a mere extension of the actor-network approach, and it problematizes temporality in ways that go beyond Pickering's (1995a, b) call for real-time analyses of science and temporal emergence.

infrastructure of science, the problem remains that Galison is vague on whether the material world should be seen as having agency, and if so, how such agency might interact or compare to human agency. Galison shows that the arguments physicists make are “subject to a network of constraints” (p. 277); he calls these “short-, middle-, and long-term constraints” (ch. 5). Examples of long-term theoretical constraints are “metaphysical commitments to methods” (p. 246), such as the conservation of energy, simplicity, or “unifying principles” (p. 247), while examples of long-term experimental constraints are familiarity with an apparatus or a tradition of using certain types of equipment (pp. 248-9). Middle-term constraints include “particular theoretical or experimental *programmatic goals*” that are “attached to specific institutions and people” (p. 249), such as specific experimental devices. Short-term constraints involve “particular theories and models” that can shape “the design and interpretation of experiments” (p. 252); experimental examples of this type of constraint are the individual runs of an experiment (p. 254). Historiographically, Galison provides compelling and detailed examples from the history of physics to support his generalizations regarding how physicists decide to end their experiments. Nevertheless, the question of material agency dangles precariously by the end of his well-researched book. How should we interpret these constraints on experimental argument?

On the one hand, Galison (1987) rejects the social constructivist notion that “interests,” professional or otherwise, can fully explain how and why scientists end their experiments. On the other hand, Galison rejects the idea that experimental results speak for themselves; he states that “procedures are *neither rule-governed nor arbitrary*” (p. 254). Arguments that physicists make, constrained by theoretical and material realities in the experimenter’s culture at the time, decide the matter. Nevertheless, is this constraining to be interpreted as merely the rhetorical force of argumentation, or do we grant agency to the material world?<sup>129</sup> This is not to say that Galison should have specified what the relationship

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<sup>129</sup> Galison’s (1997) more recent book presents a detailed cultural history of twentieth century high energy physics that is committed to problematizing dichotomies (p. xx); focuses on the infrastructure (detectors) of science (p. 3) and on experiment, rather than theory (pp. 8-9); probes the practices of physicists by analyzing how they interact across disciplinary boundaries (p. 9); sees experimental practices as distinct but linked to theoretical practices (pp. 13-14); shows how various subcultures in physics interact, while retaining their partial autonomy (p. 14); shows how the meaning of “experiment” changed over time and how that change is both

between theory and experiment is, once and for all. That would violate our commitments to postmodernism, Pitt, Sellars, and importantly, the radical philosophical naturalism of Rouse (2002a). What is needed is a specification of how to think about the relationship between theory and experiment that goes beyond stating that they are related or intertwined, one that rejects epistemic sovereignty, and embraces narrativity without losing sight of the kinds of practices scientists perform and of their own views of what those practices are.

Most of the philosophers listed in Table 1 above do not sufficiently problematize the relationship, or dichotomy, between theory and experiment. Cartwright (1983, 1989), Ackermann (1985), Franklin (1986, 1990), Lucas and Hodgson (1990), Gooding (1990), Salmon (1998), Woodward (2003a, b), and Baird (2004) advocate a form of epistemic sovereignty. Of those in Table 1 who are empiricists and epistemic sovereigns, Cartwright, Salmon, and Woodward focus on causal models of explanation, while Ackermann and Franklin focus more on error elimination methods. Lucas and Hodgson (1990), on whom I focus in some detail below, present a novel version of causality, which they call causal influenceability, yet they deploy it to advocate both ontological and epistemological forms of realism.

Of the philosophers in Table 1 who reject epistemic sovereignty, Kuhn ([1962] 1970), Derrida ([1967] 1976), Foucault ([1978] 1991), Galison (1987, 1997), Gooding (1992, 2003) and Hacking (1992) present analyses that reject the need for correspondence or forms of scientific realism, yet they are silent on material agency (and hence are not posthumanist) and most do not sufficiently problematize the material/discursive dichotomy. Latour (1987) grants agency to human and nonhuman actors in his actor-network theory, yet he presents this agency as symmetrical (i.e., interchangeable; cf. Pickering 1995a, pp. 10-15), thus

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“epistemic and moral” (p. 45); details how the concept of a “trading zone,” an “intermediate zone” between partially autonomous subcultures of experimental practice, can explain how experimentalists within those differing subcultures can “trade goods” (i.e., knowledge) without agreement on how to place “global” significance on that knowledge (pp. 46-51); rejects both attributing any global structure to science and “an idealized version of experimentation as a model of human decision making” (p. 59); and explicitly states that there are no “time-transcendent principles of laboratory action” (p. 62). While many of Galison’s (1997) principles are compatible with the orientation in this dissertation, his methodology shares a curious similarity with Latour’s (1987, 1992) in the sense that he proposes strongly contextual and historically detailed analyses, on the one hand, but is silent on the agency of the natural world and its ultimate role in constraining knowledge, on the other hand. Hence, he remains unclear on how his methodology is superior to the constructivist methodology he goes to great pains to discredit.

seemingly collapsing the human/nonhuman dichotomy, rather than probing its limits and elucidating the tensions among human intentions, material agency, experimental systems, and so on. Hacking (1983) and Burian (1996, 1997) reject epistemic sovereignty, imply material agency, and yet openly advocate a form of pragmatic ontological realism for some of the entities of science. Nevertheless, both are careful and reflective historians, and their pragmatic realisms without sovereignty are compatible with the postmodernism in this dissertation, especially if taken in conjunction with Fine's (1984, 1986) "natural ontological attitude" and Rheinberger's (1994, 1997) experimental systems and historicity.

Finally, Rouse (2002a) and Rheinberger (1994) remain. What sets them apart from the rest is a conscious blurring of the material/discursive, theory/experiment, nature/normativity and epistemic/political dichotomies, with the resultant problematization of many of the usual problems with realism, progress, agency, and normativity. Rheinberger (1992a) argues that in scientific experiments, "[t]echnological construction rests on identity in performance; scientific construction rests on difference" (p. 312). Nevertheless, as he argues, they are inextricably related:

A technological product basically answers the question [that] is implemented in its construction, if it is used under the proper boundary conditions. It is an answering machine. In contrast, a scientific object basically is a question-generating machine. Yet science and technology interact. In the process of making science, new possibilities of technological construction arise. Knowledge in the form of technological objects enters the social process of reproduction. They function as tools for production or serve as items of consumption. But they may also re-enter the research process, thus defining and refining the conditions for the formation of scientific objects. In this way technical tools define the 'system' of investigation—"any study thus begins with the choice of a "system"". They become part of the controlled boundary conditions of the experimental system. The character of fluctuation and oscillation of the scientific object within the experimental system, as a future-generating machine, is thus itself, in a way, the result of technological construction. Without a system of technically-granted identity conditions, the differential character of the scientific object remains meaningless: in other words, the particular piece of nature under inquiry does not exhibit the characteristics of a scientific object. (p. 312)

Indeed, Rheinberger (1992a) advocates a problematizing of the dichotomy of theory and experiment when writing histories of experimentation. To write such a history,

one has to go behind the opposition of theory/experiment, where ready-made theory usually occupies the place of the essence of the phenomena that are manipulated in scientific practice. One has to come to show that scientific activity is a game with things, that theories, concepts and ideas are not the Other to the things, but that they . . . are the material instances of theories, concepts and ideas. (p. 307, fn. 9)

These probings of the tensions between the poles of the theory/experiment, science/technology, and material/discursive dichotomies are required for the technological infrastructure in order to avoid correspondence realisms, convergent realism, confusions regarding human/nonhuman agency, and confusions regarding the opposition of nature and normativity.<sup>130</sup>

Rouse (2002a) provides the most philosophically developed argument for why we should problematize the theory/experiment and material/discursive dichotomies. According to Rouse (2002a), theory and experiment have no meaning without each other:

From the failure of empiricism, and also in a different way from the difficulties of scientific realism and the associated causal theories of reference, I draw the moral that relations between theory and experiment should not be conceived as relations between linguistic representations and something nonlinguistic, whether those nonlinguistic relata are conceived as experiential observations or as causal interactions. From the failure of theoretical holisms, I draw the moral that theory and experiment should also not be conceived as entirely intralinguistic. . . . Empiricists and scientific realists have each tried to connect . . . larger intralinguistic systems to something extralinguistic; theoretical holists have tried to incorporate experience and reference to objects as intralinguistic or intratheoretical. A better alternative to both approaches is to think of linguistic performances as inextricable from larger patterns of practice that are not themselves understood on the model of a language or a theory. Such an approach is thus the inverse of theoretical holism: instead of assimilating

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<sup>130</sup> Rheinberger (1995a) argues that developments in the fields of medicine, genetics, molecular biology, and related fields, have shattered the “traditional dichotomy between ‘nature’ and ‘nurture,’ between ‘biology’ and ‘culture’ . . .” (p. 257). That is, he notes that, on the one hand, we have “come to realize that the *natural* condition of mankind itself will turn into a *social* construct, with the result that the distinction between the ‘natural’ and the ‘social’ no longer will make sense.” However, on the other hand, we could also say “that the *social* condition of man will be a *natural* construct. The ‘natural’ and the ‘social’ are no longer ontologically different. We are becoming aware that we live in a world of hybrid things, or monsters, that do not belong to either realm. . . . We have come to realize that our tracing and writing techniques, these signposts of culture par excellence, have turned into techniques of writing our genes, of literally writing our bodily constitution” (*ibid.*). Donna Haraway (e.g., 1991, 1997) has also argued along these lines. In addition, recall Eco’s ([1966] 1982) interpretation of Joyce’s *Finnegans Wake*—Joyce’s novel challenges the distinction between things and words, and constructs an account of the world based entirely on words and poetics. In effect, he *wrote the world*, yet the reader is free to garner its meaning, with the main constraint being the prevalent cultural meanings of words, phrases, puns, etc.

experimentation and observation within networks of implicitly presupposed theoretical sentences, it treats discursive articulations as extensions of other practical capacities and the norms to which they are accountable. (pp. 268-9)

Hence, we should see nature and culture as mutually implicated in a way that they help to constitute each other:

The material-inferential relations between patterns of talk and particular practical interactions (including experimental practices) both articulate the meaning or content of what is said and express what is going on in the practical interactions. In the case of experimental science . . . , this expressive role of scientific discourse is not something external to the phenomenon investigated, but is a constitutive component of the phenomenon itself. Thus, the common presumption, that the natural phenomena studied by the sciences are what they are entirely independent of language and culture, turns out to be mistaken. In particular, . . . the boundaries of causally interactive systems only become determinate through their incorporation of discursive patterns and norms. . . . [M]aterial phenomena [do not] have definite bounds, [and] discursive practices [do not] express definite meanings, apart from their mutually constitutive interrelations. (pp. 269-70)

This philosophical prescription that specifies that we blur the traditional boundaries between epistemology and politics, nature and culture, discourse and practice, theory and experiment, is a key component of Rouse's (2002a) philosophical naturalism that is to be incorporated into the technological infrastructure of science.

Rouse's (2002a) system provides a framework for how to go beyond the experimental system of Rheinberger, for how to think about culture and norms. To an extent, Rouse has developed a philosophical blueprint for how to launch a theory of the *cultural*: how should we think about material entities, natural phenomena, human intentions, and social (i.e., shared by groups of people) norms? Think of them, he says, in this way: Give ontological primacy to natural phenomena over particular material entities, as those entities only take on meaning by existing within the larger patterns of phenomena. Those natural phenomena, however, only take on meaning within the larger patterns of discursive articulations about them, and should be considered to be in part constituted by them. Moreover, discursive and material performances should be considered to be embedded in larger patterns of (nondeterministic) causal intra-action, which themselves are irreducibly normative. Norms, however, should be considered to be futural in the sense that no synchronic snapshot of

patterns of causal intra-action is sufficient to “determine” or account for what those norms are, and norms do not have to be consciously, or intentionally, held to be operating at a particular time and place. That is, while all participants in a particular practice are subject to those norms—what matters in a particular context is normatively binding upon those participating—the norms cannot be bindingly authoritative on those practicing, because what matters (i.e., what is at stake) is subject to change, revision, and always outruns any attempt to specify it at a particular time. We should, instead, see practices (including scientific and experimental practices) as expressed by the notion of *real possibility*, according to which the grounds of normative authority of a particular context are the real world in which the participants exist. Those grounds, however, cannot be given a synchronic final accounting, but must wait for future developments in order to be specified more fully (and hence are, I argue, narrative in nature).

Notice that in summarizing Rouse’s (2002a) system of philosophical naturalism, we started from a philosophical need to account for change in science and ran head-on into the problems of narrativity. That is, with his acceptance of the radical contingency of history and his specification that diachronic analyses are needed to fully account for practices, Rouse (2002a) has, so to speak, taken himself home to narrativity and all the problems related to how to construct diachronic (or temporal, for Pickering) *stories* of scientific change. However, as I explored in Chapter III above, Rouse does not analyze explicitly *how* to construct those stories. If the normative authority operating in the particular context in which a scientific development occurs cannot be fully accounted for until “after the fact,” or sometime in the future, then how do we use Rouse’s naturalism to tell stories about science? Moreover, Rouse himself uses stories of past developments in order to adduce evidence for the efficacy of his own accounting for how science changes. But Rouse does not problematize how he uses those stories. The framework for Rouse’s naturalism is Heideggerian (and Neurathian) naturalism,<sup>131</sup> but his crucial evidence for why such an

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<sup>131</sup> Rouse (2002a) also pays debt to two other naturalists and states that his system of philosophical naturalism is governed by “the Quinean metaphilosophical commitment to avoid arbitrary impositions upon the development of science and the Nietzschean philosophical commitment not to accept or rely upon what is mysterious or supernatural. . .” (p. 4). The former commitment can be seen as a plea for stories about science

orientation is compelling is History-3, that is, stories about the past. Yet Rouse's (2002a) main goal is to tell us a story about how we should tell stories about science and its relation to the world in which we live: "Outlining . . . a reconception of science, the natural world, and their philosophical interpretation in the spirit of naturalism is the aim of this book" (p. 5). Nevertheless, Rouse's system is not sufficiently reflexive, as it does not problematize adequately the various levels of history (History-0, -1, -2, -3, -4, -5), nor does it seem able to handle adequately the Rheinbergerian concerns regarding recurrence and historicity. How do we apply Rouse's naturalistic prescriptions in order to tell a story about the past?

Just as Rheinberger needs Rouse to specify how to make sense of science outside of the laboratory or the experimental system, so does Rouse need Rheinberger and his commitments to historicity and a reflexive position on the historiography of science. Narrativity is perhaps the central problematic in this dissertation, and it has pervaded nearly all the analyses in this and preceding chapters. Indeed, narrativity is the key to providing a compelling (post?)naturalist,<sup>132</sup> postnationalist,<sup>133</sup> postconstructivist,<sup>134</sup> postanalytic,<sup>135</sup>

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that are faithful to the actual history of science, and the latter a plea to tell stories that are faithful to our everyday experiences as humans.

<sup>132</sup> Rouse (2002a) states of his own radical naturalism: "A central theme of this book is that taking these [Quinean and Nietzschean] constraints seriously compels substantial revisions in the most familiar conceptions of philosophical naturalism" (p. 5). In this sense, we might view Rouse's philosophical system as "postnaturalist."

<sup>133</sup> Rowe's (2000) edited volume provides an excellent analysis of *post-nationalist* inquiry, at least in the context of American Studies. By postnationalist, as the contributors explain in the introduction, the authors mean "a version of American Studies that is less insular and parochial, and more internationalist and comparative. In this sense, a post-nationalist American Studies respond[s] to and seek[s] to revise the cultural nationalism and celebratory American exceptionalism that often informed the work of American Studies scholars in the Cold War era" (p. 2).

<sup>134</sup> Rouse (2002b) provides a critical analysis of the significance for science studies of the notion of postconstructivism, and he mentions several works that fit into this mold, including those by Pickering (1992), Haraway (1989, 1997), Rheinberger (1997), and Galison (1996), all of whom take a cultural studies type of approach. He dissects in detail two of them, which he believes nevertheless still have vestiges in them of the "philosophical undead"; they are Andre Kukla (2000), *Social Constructivism and the Philosophy of Science*, London: Routledge; and Ian Hacking (1999), *The Social Construction of What?* Cambridge: Harvard University Press.

<sup>135</sup> See the volume edited by Rajchman and West (1985) for a beginning point on postanalytic philosophy, including contributions by Rorty, Putnam, Davidson, Hacking, Kuhn, and Cornel West. West (1985), in particular, argues that Quine, Goodman, and Sellars were those most instrumental in the destruction of logical positivism (he describes Hempel, who should be included in this group, as a logical positivist with Carnap); Kuhn (1962) and Rorty (1979) helped popularize one alternative, epistemological holism. More significantly, West (1985) argues that one main result of the demise of logical positivism was a resurgence of pragmatism, at least in North America, which he calls "neo-pragmatism" (p. 267), and which is best



posthumanist,<sup>136</sup> and postmodern view of science, including scientific experimentation. The technological infrastructure of science needs Rousean naturalism, Rheinbergerian historiography, Ricoeurian temporality, *and* the narrativity of White and Kermode. With these main components, we can fulfill Pitt's plan for providing a construct we can locate, in particular historical contexts, that accounts for successful scientific developments over time and how they were facilitated by technological instrumentation, experimental techniques, professional and governmental institutional structures, political structures, and other technologies (in the sense of Pitt's definition of technology as "humanity at work"). In

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exemplified by Rorty. West sees this as a positive development, but urges that "American neo-pragmatic philosophers should not settle simply for shedding old self-images and breaking out of professional modes; they also can contribute to the making of a new and better global civilization" (p. 272).

<sup>136</sup> Pickering's (1992) edited volume is still the best introduction to posthumanism in studies of science and technology. This plea for posthumanism, however, should not be confused with the characterization of "posthuman" as a danger and threat to human societies that could result from the negative effects of technologies, as in the traditional philosophy of technology that Pitt (2000) criticizes (see, for example, Mitcham and Mackey [1972] 1983). Recently, Francis Fukuyama (2002) has resurrected this type of argument in his warning against the possible cultural effects of "the biotechnology revolution." One might conclude that his laudable policy recommendations for more regulation and control of biotechnology are striking for a neoconservative (or perhaps former neoconservative; see Fukuyama's (2006) *America at the Crossroads: Democracy, Power, and the Neoconservative Legacy*, New Haven: Yale University Press; and "After Neoconservatism," *The New York Times Magazine*, 19 February 2006, pp. 62-67, in which he discusses a post-Bush foreign policy—"realistic Wilsonianism"—according to which a commitment to universal human rights would be retained, but American exceptionalism in the form of a hegemonic and over-militarized policy stance would be rejected for a more "realistic" view of the global economic, cultural and power situation). However, Fukuyama's (2002) neo-Hegelian commitment to a view that has ever-improving "stages" to history (see, for example, Fukuyama's (1992) *The End of History and the Last Man*, New York: The Free Press); his grounding of cultural values and human rights in human nature to avoid cultural relativism (see esp. ch. 7); and his masked commitment to a form of technological determinism (see, for example, pp. 14-17), suggest he retains elements of his neoconservative heritage, at least as far as he justifies American exceptionalism, in the form of cultural superiority, through a universalist formulation of *human nature*. One problem with this is that it ignores the contestedness of the scientific results that underlie his commitment to traditional naturalism (see pp. 112-128 for his rejection of the "naturalistic fallacy"). Indeed, Fukuyama argues against the scientific evidence against essentializing human nature, including the arguments of Richard Lewontin (pp. 135-7), and justifies his conclusions on this essentializing of human nature in the form of an argument that shows how it is ostensibly superior to the alternatives, including utilitarianism, revealed religion, and what he terms "contemporary positivistic rights, located in law and social custom" (p. 111). Hence, he uses contested scientific results to form the foundation of an essentialist theory of human nature, and then uses this theory to argue for a policy position on the control and regulation of biotechnology in the contingencies of the present. In effect, he replaces militaristic hegemony with cultural hegemony as the new post-neoconservative weapon. For all his ironically sympathetic citations of Nietzsche (Allan Bloom was one of Fukuyama's teachers; in *The Closing of the American Mind*, Bloom [1990] blamed Nietzsche's supposed nihilism for many of the ills of twentieth century American culture), Fukuyama (2002) violates the core Nietzschean commitment to not base theories of history or culture on superstitious constructs. Fukuyama's superstitions are an essentialized human nature, technological determinism, and stages to history. In this dissertation, I present arguments that suggest such superstitions are not necessary and may have negative consequences.

addition, since nature and normativity are no longer viewed as separable, and since the epistemic and the political are also inseparable, science and technology policy are to be seen as inseparable from the doing of science or the development of technologies.<sup>137</sup> Most importantly, though, *culture* is to be seen as the stories we tell ourselves about how things hang together. And those stories, again, must be *told*. Moreover, the telling of stories is, as we have seen, an activity that involves recurrence. And recurrence, the ubiquitous human activity of assessing—consciously or unconsciously—our past, so we can take action in the present to shape the future, is subject to the traditional problems of narrativity:

I have said before  
That the past experience revived in the meaning  
Is not the experience of one life only  
But of many generations—not forgetting  
Something that is probably quite ineffable:  
The backward look behind the assurance  
Of recorded history, the backward half-look  
Over the shoulder, towards the primitive terror.<sup>138</sup>

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<sup>137</sup> On the issue of the inseparability of scientific practice and policy, refer back to the question in note 13 regarding the dangers of Fukuyama’s (2002) grounding of policy in an essentialized notion of human nature: why is it dangerous? It is dangerous because such a move tends to reinforce the dualisms of modernity—especially those of good/evil and right/wrong. In essentializing human nature, one risks adopting the privileged, politically/epistemically sovereign position of justifying one’s own political (or moral, ethical, etc.) stand as “given” by nature, in this case, “human nature,” or an essentialized version of it. This fails *reflexivity*, because at the level of a meta-policy prescription (i.e., denying the “naturalistic fallacy” and choosing particular contested scientific results for the grounding of a political position), it does not apply to itself, since the supposedly scientifically-grounded policy strategy is privileged and sovereign and therefore ostensibly immune from criticism; it is a one-way street. This type of strategy is not new for the neoconservatives. According to Peter Beinart, it was inherited not only from neoconservatives, such as William Kristol, but from Ronald Reagan and dates even to “the birth of the modern conservative movement itself” in the mid-1950s with William F. Buckley and the journal *National Review* (Peter Beinart, “The Rehabilitation of the Cold-War Liberal,” *The New York Times Magazine*, 30 April 2006, pp. 40-45; quotation from p. 42; see also Beinart 2006). The strategy involves, in its present George W. Bush-era instantiation, the following: “Where Bush . . . goes wrong is in believing that America can unilaterally declare a moral standard while exempting itself. For President Bush, freedom is a one-way conversation. The United States calls on other countries to embrace liberty; we even help them in the task. But if they call back, proposing some higher standard that might require us to modify our actions, we trot out [U. S. Delegate to the United Nations] John Bolton. For the rest of the world, freedom requires infringements upon national sovereignty. But for the United States, sovereignty trumps all” (*ibid.*, p. 44).

<sup>138</sup> T. S. Eliot, “The Dry Salvages,” (1941) lines 96-103, in *Four Quartets* (©1943, 1971), San Diego: Harcourt, Inc., p. 39; reprinted by permission of Harcourt, Inc.

## **Lucas, Hodgson, and Einstein's Special Theory of Relativity**

In this section, I deploy the technological infrastructure construct to deconstruct the arguments Lucas and Hodgson (1990) present in their book on Einstein's Special Theory of Relativity (STR), in order to provide an example of how to use the technological infrastructure in practice. The authors' main purpose in this book is to construct a philosophical and historical argument about causality in STR in order to support both ontological realism and convergent realism. To do this, the authors consider an example from the history of physics—the transition from Newtonian mechanics to Einsteinian Special Relativity. Specifically, in their book *Spacetime and Electromagnetism: An Essay on the Philosophy of the Special Theory of Relativity*, Lucas and Hodgson, the former a philosopher and the latter a physicist, both at Oxford, claim to give an argument supporting causality as a basic concept of STR. That is, they believe Minkowski spacetime can be explained by or reduced to causality. However, in order to specify causality as the physical basis for temporal relations, they must give an independent specification of the proposed causal relation without recourse to concepts of spacetime or time ordering. Lucas and Hodgson, however, fail to do this. With this example, I show how to deploy the technological infrastructure to unpack and deconstruct the philosophical and historiographical arguments the authors present or presuppose. Ultimately, the authors fail to give a coherent account, largely because they have neglected to specify the technological infrastructure of the science they are examining. At most, they refer only passively to the relevant experimental contexts, and when they do, their philosophical assumptions regarding the import of the experimental arguments are easily deflated. Clearly, they do not critically analyze the historical processes by which signal was separated from noise; their purposes are more toward justifying a particular view of physical reality by appropriating a spurious historical account of the development of STR.

The authors present a narrative that suffers from numerous conceptual problems. In defending a version of convergent realism, according to which new scientific theories—in this case, the supposed replacement of classical Newtonian mechanics by STR—are to be seen as successive approximations to the truth, Lucas and Hodgson (1990) present a

philosophical and historical interpretation of STR that fails on a number of counts.

Foremost, their arguments suffer from internal philosophical flaws, flaws that detract from the coherence of their views. Additionally, they employ the history of science—in this case, Albert Einstein, his development of STR, and his supposedly realist philosophical views (see Fine 1986)—to support realist claims about science and the philosophy of science. In doing so, the authors employ an historiography that results in a distorted and inaccurate history, one that amounts to little more than a Lakatosian rational reconstruction. Finally, these authors set forth a view of scientific change that does little justice to the actual practice of science.<sup>139</sup>

### I. *Philosophical Problems*

To begin this deployment of the technological infrastructure construct, I first deconstruct the internal philosophical arguments presented by Lucas and Hodgson (1990). In their interpretation of STR, Lucas and Hodgson (1990) follow a rationalist methodology, one that steers ground closer to neo-Kantian apriorism than to empiricism. The authors claim, among other things, to give an argument for the view that some notion of causality is basic to STR, and that the resulting concept of spacetime that emerges from their arguments, should be considered real. These views pose serious philosophical problems. In order to explore the notion of causality the authors formulate, I focus on chapter 3 of their work. In this chapter, Lucas and Hodgson present their argument supporting causality as a basic concept of STR. The causal relation Lucas and Hodgson (1990) think will do the job is *causal influenceability*, pioneered by Robb (1914, 1921). According to Lucas and Hodgson, there is

one ordering relation [that] is absolute and the same for all frames of reference, and thus plays a more fundamental role [than do equivalence relations] in the Special Theory [of Relativity]. The one ordering relation is that of *causal influenceability*, or conversely *being a potential cause of*: influenceability is taken in a very wide sense; one event is causally influenceable by another not just if it actually is, or reasonably ought to be, influenced by it, but if it conceivably could. (pp. 30-1)

At first glance, by offering a version of causality that seemingly has a futural orientation to it,

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<sup>139</sup> Interestingly, Lucas and Hodgson (1990) make the following claim: “As philosophers of science we should take the actual practice of scientists seriously” (p. 269). It is difficult to see how in their book that they have been successful in this endeavor.

Lucas and Hodgson appear to echo Rouse's (2002a) naturalism and his notion of *real possibility*. Lucas and Hodgson seem to want to characterize causal influenceability in terms of our everyday experience of time and events, and not through an ostensibly mathematical or logical deduction. Indeed, they state there are several ways to characterize (define) the light cone of STR, and hence spacetime and temporal relations (pp. 86-8). One can do so in terms of electromagnetic radiation, showing whether a photon could go from A to B (i.e., B is on the forward light cone of A). One could also take a materialist approach, and characterize the light cone in terms of "being causally influenceable by material bodies." This is the approach Lucas and Hodgson take, yet they want to remove any doubt that they are advocating a reductionism; they claim *not* to be reducing (in a strict, deductive sense) temporal relations to causal relations (p. 89). Hence, they endorse a less strict view of causal influenceability, as defined above, one that eschews materialistic characterizations and the problem of infinite forces that would be necessary to change instantaneously the velocity of a rigid, material body.

Nevertheless, Lucas and Hodgson (1990) do not offer a view of causality that problematizes the material/discursive or nature/normativity dichotomies, as is specified by the technological infrastructure. Instead, they are firmly embedded in modernity with its disembodied view of causality and events. Hence, after reviewing some attempts by other philosophers to derive the light cone structure of Minkowski spacetime (pp. 89-108), Lucas and Hodgson (1990) take up criticism of their "absolute approach." It is interesting to note that they deny that their approach to "deriving" the fundamental topology of Minkowski spacetime is unique. Indeed, they claim that of the three ways to derive the light cone, none "suggests itself on grounds of logic alone as more basic than the others—whichever one we start with, we need to define the others in order to develop a causal approach. . ." (p. 88). However, later on the authors claim that the light cone structure resulting from causal influenceability "is part of the fundamental topology of Minkowski spacetime, not a particular standpoint we can take up if we please" (p. 93). Somewhere in their argument, Lucas and Hodgson have made a transition, they believe, from an inductive inference to a tightly-established conclusion, one that is apparently unique and nonarbitrary. Nevertheless,

they have not established their conclusion on independent grounds, but have assumed causal influenceability, at least implicitly, from the beginning.

As Nerlich (1982) points out, STR does not require causal influenceability. One main component of Nerlich's argument, however, is that the Limit Principle (nothing can go faster than light, including causal processes; also designated as Finite Maximum Speed) is not a primitive term or entailed by STR. If this is so, as Nerlich argues, STR gives no specific significance to causality. Nevertheless, it seems there is a problem here right from the beginning regarding definitions. According to the Limit Principle, nothing can go faster than light, *including* causal processes. This seems to stipulate a firm notion of causality right from the beginning and, as Nerlich points out (pp. 368-71), would render spurious the large body of literature on tachyons (particles that travel faster than light, or superluminal particles). It is not clear, however, that we should equate the Limit Principle with the principle of Finite Maximum Speed, which is presupposed by the postulate of Universal Speed, or in Nerlich's vocabulary, the Invariance Principle.<sup>140</sup> According to Universal Speed, the speed of light is the same in all frames of reference; clearly, Finite Maximum Speed follows from Universal Speed, unless we presuppose that the speed of light is infinite (which contradicts experimental results). Universal Speed (or the Invariance Principle, for Nerlich) must be taken as a basic postulate of STR. This point does not seem to be in debate. Lucas and Hodgson (1990) presuppose Universal Speed in their "derivations" of the Lorentz Transformation, even if they take the Limit Principle to be more basic (see pp. 7, 112-3, 284-7). What, then, is at stake here?

Part of the problem is that it is not clear that we should equate the Invariance Principle with Universal Speed. As Nerlich (1982) suggests, Einstein, in advocating the Invariance Principle, did not intend "that the finite speed of light is the limiting speed for every real process, but, in effect, that it is the *invariant* speed for all Lorentz frames" (p. 363). In his 1905 paper, Einstein ([1905] 1981) claimed to "have deduced the essential

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<sup>140</sup> To be precise, the Invariance Principle states that the laws of physics are invariant under Lorentz Transformations. Nerlich (1982) includes Universal Speed in his definition of the Invariance Principle (p. 362). In his original 1905 paper on STR, Einstein ([1905] 1981) separates these principles (pp. 394-5).

theories of the kinematics corresponding to . . . two principles. . .” (p. 404). These principles he gave as follows:

1. The laws by which the states of physical systems undergo changes are independent of whether these changes of state are referred to one or the other of two coordinate systems moving relatively to each other in uniform translational motion.
2. Any ray of light moves in the ‘resting’ coordinate system with the definite velocity  $c$ , which is independent of whether the ray was emitted by a resting or by a moving body. (p. 395)

It seems Nerlich’s (1982) argument is cogent as long as he is taking the Limit Principle to include the propagation of causal influence by material bodies. Indeed, this is what Nerlich has in mind; he states that the traditional motivation to make the Limit Principle basic to STR stems from a desire to reduce “the metaphysically uncomfortable ideas of space and time to the single familiar idea of physical material cause” (p. 361). However, this does not apply as a criticism of the approach of Lucas and Hodgson (1990), for they do not claim to be *reducing* space and time to causality. Their notion of causal influenceability is wider and less rigid than the concept of causality as the impact of material bodies. Nevertheless, this does not free Lucas and Hodgson from the charge of circularity. In order to demonstrate that causality (or causal influenceability) is basic to STR, one must give an independent specification of causality without recourse to the concepts of space and time embodied in STR’s light cone. Lucas and Hodgson have assumed causal influenceability as a premise, and they apparently do this willingly. Yet they claim they have given justification, somewhere along the way, for causality as a feature basic to STR. As they state:

At the very least, [the absolute approach] shows that, granted a reasonable ontology and a satisfactory notion of causal influenceability, we can establish the Special Theory [of Relativity] on quite different grounds from those normally adduced in its support, and that even if these are not claimed to be metaphysically prior, they would still constitute a constraint on the form that the theory could take, and explain some of its peculiar features. (p. 110)

It is not clear why Lucas and Hodgson (1990) claim to have shown anything more than a *petitio principii*. Of course, if causal influenceability is assumed, it will have certain logical implications for STR. However, as far as upholding causality as basic to STR, this

argument has failed; it constitutes premise circularity. It seems this argument is a weakened form of Lucas's (1973) earlier argument, in which he claimed to give a transcendental deduction of causal influenceability (cf. Angel 1980, pp. 110-15). Lucas and Hodgson do not claim, however, that their derivations of the Lorentz Transformation or of the light cone structure of Minkowski spacetime are strict, necessary deductions. Nevertheless, they do claim to have shown something profound about STR and the underlying nature of reality.

Apparently, Lucas and Hodgson (1990) want us to take their view of causality as intuitively necessary for any physical theory, as a Kantian synthetic *a priori* proposition (pp. 276-9). Nonetheless, they contend that "physical arguments cannot be completely formalized, and in any case are not deductive because their conclusions can characteristically be denied without inconsistency" (p. 276). We therefore should take synthetic *a priori* propositions such as causality not as rules necessary for experience itself, but as somehow inductively justified (pp. 278-9). However, Lucas and Hodgson do not want us to take them as mere stipulations; yet, even if viewed as such we must take them as somehow imbued with rational justification:

Even when viewed as stipulations, synthetic *a priori* propositions are not just arbitrary *fiats*, but reasonable requirements for which a rational justification, partly in terms of the aims, partly in terms of the presuppositions, of physical inference, can be given. (p. 277)

While it would be unfair to criticize Lucas and Hodgson (1990) as irrational for assuming the uniformity of nature, what they have not done is demonstrate why it is necessary to consider causality as basic to STR and required to make conceptual sense of it. With respect to quantum theory, this may not even be desirable. As Nerlich (1982) states, "to commit [STR] to causality as a basic concept is to make a problem of its inconsistency with quantum theory" (p. 363). Indeed, Lucas and Hodgson (1990) believe the Copenhagen Interpretation of quantum mechanics<sup>141</sup> has "serious philosophical difficulties, and cannot be

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<sup>141</sup> According to the Copenhagen Interpretation of quantum mechanics, developed by Niels Bohr and others, quantum mechanics is complete, despite the inherent indeterminism and unpredictability it ascribes to the microworld. That is, it gives a complete description of the state of a physical system, and no amount of hypothesizing "hidden variables" will help the matter, as Einstein (apparently; see below) and others thought; see Albert Einstein, Boris Podolsky, and Nathan Rosen [EPR] (1935), "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" *Physical Review* 47: 770-80; Bohr (1935) responded with a



taken to be definitively established” (pp. 116-17; see also 114-21). They suggest that “the collapse of the wave function happens independently of our coming to know about it,” and that “Schrödinger’s cat either is alive or else is dead before we look at it, and cannot be a superposition of an alive wave function and a dead wave function.” Hence,

we are committed to there being a difference between the future on the one hand and the present on the other in much the same way as those philosophers who have claimed that they are modally different, and that the future is open whereas the past is unalterable and fixed. (p. 117)

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paper defending completeness and explaining his principle of complementarity, “Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?” *Physical Review* 48: 696-702. This interpretation can, however, lead to situations that seem counterintuitive, as with “Schrödinger’s cat,” a paradox originally formulated by the developer of quantum mechanics, Erwin Schrödinger (1887-1961), and motivated, at least in part, by Einstein’s paper cited above; see Erwin Schrödinger (1935), “Die gegenwärtige Situation in der Quantenmechanik,” *Naturwissenschaften* 23: 807-12, 823-28, 844-49. Consider a quantum process arranged so it releases a particle with a 50% chance of striking a detector. The detector, if struck by the particle, will cause the release of poison into a closed box containing an initially live cat. After the release of the particle, but before the condition of the cat is observed by opening the box, the state of the system containing the cat is given by the superposition of two quantum mechanical wave functions, one for “dead cat,” and one for “alive cat,” resulting in a system that is described as a cat in an “alive-dead” state. Only after the cat is actually observed, by opening the box, does the wave function of the system collapse to either a dead state or an alive state. This paradox expresses Bohr’s view that the quantum world is indeterminate and unpredictable, and any specific parameters remain so until an observation or measurement is made; furthermore, by apparently extending that indeterminacy to the macroscopic world, it also expresses the view, attributed to Einstein, that the description is incomplete, by suggesting the absurdity of requiring, it seems, an alive-dead cat before the observation is made. In their criticism of Bohr’s interpretation, Lucas and Hodgson (1990) are echoing Einstein’s apparent suggestion that quantum mechanics might not be a complete description of physical reality (i.e., of a quantum state), thus ostensibly putting quantum mechanics on philosophical (and empirical?) ground that is not as firm as the ground of STR. However, Fine’s (1986) in-depth analysis of this story shows that Podolsky wrote the EPR paper and that Einstein believed his own central argument became obscured (ch. 3, esp. pp. 35-6). Furthermore, Fine shows that Einstein was neither a realist in the sense of correspondence to reality nor in the sense of convergence to truth (ch. 6).

In addition, insofar as they are committed to *locality* (pp. 2-5)—the notion that there is no instantaneous action at a distance—Lucas and Hodgson (1990) argue against *nonlocality* in quantum mechanics only to the extent that it supposedly contradicts the Copenhagen Interpretation and its allegedly inevitable entailment that superluminal speeds are necessary to explain the results of experiments; the seminal paper in the nonlocality controversy is John S. Bell’s (1964) “On the Einstein-Podolsky-Rosen Paradox,” *Physics* 1: 195-200. That is, while Lucas and Hodgson are committed to locality to the extent they need to uphold their rationalist view of causality—with no propagation of causes faster than the speed of light—they are ironically willing to accept nonlocality as a feature of the microworld so long as it can be interpreted to be inconsistent with instantaneous action at a distance, and hence as support for what they believe is Einstein’s realist interpretation (pp. 114-17). In doing so, however, they would nevertheless still be sacrificing *predictive completeness*, which they clearly believe must be preserved (pp. 117-21); they claim that it can be preserved by attaching “to Special Relativity some realist version of quantum mechanics in which the collapse of the wave function is a real event, not just a change in the knowledge of an observer” (p. 119). So, their desire to accommodate their interpretation of STR to a realist version of quantum mechanics results in a contradiction: one cannot have *both* simple causal locality in STR and in quantum mechanics, *and* retain the notion of predictive completeness specified by EPR; for more on this, see L. E. Ballentine and Jon P. Jarrett (1987), “Bell’s Theorem: Does Quantum Mechanics Contradict Relativity?” *American Journal of Physics* 55: 696-701.

Whatever position we want to take on the Copenhagen Interpretation, it remains that the pseudo-rational derivations of STR by Lucas and Hodgson provide no compelling argument for causality as a fundamental aspect of STR or for it being uniquely specified by STR.

Lucas and Hodgson (1990) further conclude that their derivations and arguments support the view that the concept of spacetime that emerges from STR should be considered *real*. They believe that STR “is not in the least anti-realist, but on the contrary a great stride towards discovering the underlying structure of reality” (p. 261). They give their reasons as follows:

Thanks to the Special Theory [of Relativity] we see reality more clearly than before, because we have identified more closely the invariant realities behind our various observations. Instead of absolute date, defined by absolute simultaneity, and absolute duration, we have absolute spacetime separation and proper time. (p. 260)

This mode of arguing suggests Lucas and Hodgson (1990) are taking their rationalist arguments quite seriously. Concerning the absolute approach given in their chapter 3, the authors state that the light cone, the fundamental feature of spacetime, flows from a few reasonable premises: the partial ordering relation of causal influenceability, with its fundamental entities, possible events; and assumptions concerning continuity and dimensionality (p. 266). The result, they conclude, is “a thing that claims interpersonal validity and has an ineluctability vindicated by experimental observation, while possessing a high degree of rational transparency” (p. 266). How this flows from an argument they admit is not deductive or uniquely specified is unclear. Lucas and Hodgson take a metaphysical leap in advocating a form of ontological realism for Minkowski spacetime.

Again, this leap seems to be motivated by their view of causality. In order to uphold the partial ordering relation of causal influenceability, Lucas and Hodgson (1990) must answer Hume and his empiricist criticisms of cause and effect. Again, they answer Hume not by adopting a Kantian viewpoint according to which certain synthetic *a priori* propositions are necessary for experience, but by suggesting that such propositions are necessary for doing physics, at least a rationally acceptable physics. Hence, they claim that they are

led to a certain sort of relationism—causal relationism we might call it—not by some materialist doctrine that only material objects exist or [by] doubts about the real existence of time or space or spacetime, but as the only available way of allowing spatiotemporal factors to enter into causal explanations in a form sufficiently limited to exclude complete arbitrariness. (p. 281)

What apparently empowers Lucas and Hodgson to take this transcendental leap is that the ordering relation in causal influenceability is “faithful to the ordered structure of our ordinary temporal experience” (p. 112). Therefore, it is rational to presuppose causal influenceability. Furthermore, concerning reality, they claim:

Quite apart from the pragmatic unwisdom of forswearing inductive inferences, it is incoherent to do so if I have any notion of a reality other than myself I need to come to terms with, and unreasonable to do so if I have any hope of reality’s being rational and capable of being understood. (p. 280)

Apparently, our “ordinary temporal experience” justifies causal influenceability, which justifies establishing Minkowski spacetime as real, and the ontology of spacetime events justifies our making inductive inferences in science. This is an interesting argument, but it fails on its own philosophical grounds, in addition to those of any sufficiently postmodern interpretation of the philosophy of science or of science itself as a historical/cultural phenomenon. Besides, as Nerlich (1982) argues, “causality is too indefinite a concept to play the part which the standard view [that causality is basic to STR] envisages for it” (p. 363). In his analysis, Nerlich (1982) shows that one can

define the light cone in ways quite independently, conceptually, of causality and even by a means of a relation which is not even co-extensive with causality, whether the Limit Principle is true or false (or entailed by [STR]). (p. 363)

Nerlich (1982) takes his fundamental relation to be the *elsewhen* or *elsewhere* relation. In any event, one need not imbue spacetime with material properties, at least not from the perspective of STR. Accordingly, STR

can be seen as telling us, not something about the relations of matter to light, but about the interesting union of the spacelike, timelike and lightlike relations in spacetime. Metaphysically, we can best understand its message, not as a materialist, but a physicalist one about a physical particular—spacetime. (p. 387)

## II. *Historical and Historiographical Problems*

The second component of this deployment of the technological infrastructure construct involves the deconstruction of the historical and historiographical problems associated with traditional attempts to account for Einstein's development of STR in terms of convergent realism. To do this, I consider the views of Angel (1980) in addition to those of Lucas and Hodgson (1990). In upholding convergent realism, Lucas and Hodgson (1990) and Angel (1980) argue for the commensurability of classical Newtonian mechanics and STR. By showing the philosophical commensurability of the two theories, these authors claim to support the thesis that rational theory choice is preserved in the alleged "transition" from Newtonian mechanics to STR, and therefore that the thesis supports convergent realism. That is, the authors argue that this case study lends credence to the philosophical view that STR is a closer approximation to the truth (of the nature of the world) than is Newtonian mechanics. In addition, these authors suggest that the *actual* history of physics lends credence to this view.

For example, Angel (1980, p. 140) states that the "view of the development of scientific theory in terms of successive approximation to the 'truth'" is "fairly widespread," but has been called into question, notably by Kuhn (1962, 1970) and Feyerabend (1962). Angel devotes an entire chapter to demonstrating the commensurability of Newtonian mechanics and STR (ch. 6), and he argues that even though Newtonian mechanics cannot be reduced (in the sense of deductive entailment) to STR, the two theories are comparable, and hence commensurable. Angel, however, rejects the often-argued view that Newtonian mechanics is a special case of STR, with the speed of light infinite, and the Lorentz Transformation transformed into the Galilean Transformation. This claim is worth analyzing to see how it bears on convergent realism.

Angel (1980) believes Kuhn and Feyerabend have rightly shown that Newtonian mechanics is not a special case of STR (p. 142). Put simply, at least some of the terms in STR that also appear in Newtonian mechanics have undergone a change in meaning. For example, relativistic mass in STR is clearly one term with a different meaning as compared to classical mass in Newtonian mechanics (p. 143). However, it is the "denotation" of a term

that has relevance for Angel's realism. As he states,

the connotation of a scientific term such as 'electron' will depend heavily on the theory in which it occurs. On the other hand, from the standpoint of scientific realism, which denies that the world is either a creation of the mind or of language, the denotation of such a term may be relatively constant across the boundaries of several theories. (p. 143)

This argument may apply relatively well to entities such as electrons, if we consider them natural kinds or support their reality with the pragmatic realism of Hacking (1983), for example. Accordingly, the *kind* (or entity) remains constant over the course of theory changes, even if the meaning of the term, as defined by the theory, changes. Hence, the *reality* of electrons (as natural kinds) is defended. However, the situation for mass is different. It is difficult to see how we could consider mass a natural kind, or an *entity*. Furthermore, concerning STR, it is spatial and temporal coordinates that Angel wants to argue are real, since they "have the same denotation in different theories" (p. 143).

Angel's (1980) argument rests on a clever comparison of the theories of Newtonian mechanics and STR, with crucial experimental results deciding the matter in favor of STR (ch. 6). In this formal comparison, Angel introduces a number of assumptions into his "derivation," assumptions that he claims are testable, and hence vindicated by experiment. One assumption is the introduction of the relativistic spacetime four-vector  $F^\mu$  into the comparison, in order to have a basis for comparison of Newtonian mechanics and STR in the language of Newtonian mechanics (p. 147). While Angel warns us against viewing this move as a *definition* of force, his claim that it is a "testable physical hypothesis" and therefore justified as a basis for comparing the two theories, is suspect. This view presupposes the possibility of a crucial experiment<sup>142</sup> and thereby violates the theory-ladenness and underdetermination theses. That is, if a crucial component of Angel's argument for rational, logical theory choice between Newtonian mechanics and STR (with STR coming out as superior) is an experimental result whose unambiguous result guarantees the logical comparison of the theories, then Angel must deal with theory-ladenness and

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<sup>142</sup> Angel (1980) believes crucial experiments are possible. See, for example, his discussion of time dilation (pp. 145-6).

underdetermination. If he does not, one could respond by arguing that instant falsification is untenable, that auxiliary hypotheses could be formulated to account for the discrepancy, and that other theories could equally well accommodate the experimental data. Angel's *realism* could hence be in jeopardy, since he takes rational theory choice to be an argument for convergent realism.

Moreover, Angel's (1980) argument for convergence further rests on an argument from history. This is where the historiographical problems arise. Angel apparently supports rational theory choice in that he believes that his version of the STR episode, with the proper philosophical interpretation of the events, was the way it *actually* happened. That is, that there was an historical *choice* between two theories, Newtonian mechanics and STR, is a crucial component of his argument. After all, *convergent* realism specifies an *historical* process with a fixed chronological order. By choosing the better theory, we are not only getting closer to the truth (in the sense of epistemological realism), but this somehow also guarantees that terms whose denotations are preserved over the course of time are denoting real aspects of the world (entity or ontological realism). However, the assumption that STR and Newtonian mechanics were rival theories up for grabs after Einstein's 1905 paper on STR, is dubious historically. How does this affect Angel's argument?

Physicists, historians, and philosophers of science have argued that STR and Newtonian mechanics were never rival theories.<sup>143</sup> By the time Einstein proposed STR in 1905, developments in electrodynamics had already called into question basic tenets of classical mechanics, including the notion that there is no upper bound to the propagation of causal influence. That is, electrodynamics presupposes Finite Maximum Speed, whereas it cannot be accommodated in Newtonian mechanics. If this broad thesis is correct, there was no rational theory choice between Newtonian mechanics and STR, because no such choice existed. What Einstein did was to accommodate the Finite Maximum Speed (of light) into

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<sup>143</sup> See, for example, Born ([1920] 1962) and Goldberg (1984). This argument for the commensurability of Newtonian mechanics to STR is predicated on the assumption that STR and Newtonian mechanics were rival theories for physicists to choose from in the early twentieth century. However, the historical evidence shows that STR and Newtonian mechanics were never rival theories. If there was a choice, it was a choice between Lorentz's theory and Einstein's (see, for example, Goldberg 1984, esp. pp. 149, 325). According to Goldberg (1984), the choice was between Lorentz's theory and Einstein's Special Theory of Relativity, yet the two theories "make identical predictions" (p. 149).

classical dynamics, but the prior development of electrodynamics helped him do this. Hence, the historical development from Newtonian mechanics to STR did not involve two well-defined, conflicting theories, one of which was declared superior on logical and/or empirical grounds, as adherents of epistemic sovereignty would presume. Concerning Angel's convergent realism, it is difficult to see how his argument can be maintained. It fails historically and historiographically.

Angel (1980) could respond by claiming that a philosophical, logical comparison can still be made, regardless of the facts of history. But now his argument is open to charges of theory-ladenness and underdetermination. We are right back to the ahistorical question of the interpretation of the logical status of STR. Hypothetical crucial experiments do not help, since logical foundations are at stake, not historical development. Without the historical context of a *choice* between two rival theories, with the superior one chosen on the basis of empirical comparison, it is difficult to see how convergence is possible or compelling (and this is granting all of Angel's other assumptions).

Lucas and Hodgson (1990) present an argument for convergent realism that faces problems equally as serious as those facing Angel (1980). In addition, they also abuse history to support their interpretation of STR and its development. To begin with, Lucas and Hodgson fall into the trap of supporting the view that Newtonian mechanics is a special case of STR, with the speed of light infinite, and the Lorentz Transformation reducing to the Galilean Transformation (see p. 144). They require this for their argument for a number of reasons. First, they take this assumption to support their "Communication Argument" (ch. 4). For if it is assumed that  $c$  is infinite in Newtonian mechanics,

then the communication argument yields the Galilean transformation and a simple Newtonian view of time flowing uniformly and equably, independent of anything else. Indeed, it is an intuitive communication argument adopting this assumption which makes the Newtonian view seem so compelling. (p. 144)

Even more fundamental is Lucas and Hodgson's (1990) view that STR could have been rationally derived directly from Newtonian mechanics (pp. 1-2, 25-6, 298-9). Ultimately, they take this argument, like Angel's, to support convergent realism and, for Lucas and Hodgson, the reality of spacetime. Indeed, the commensurability and logical

connections between Newtonian mechanics and STR are crucial for their argument.

However, as noted, the distortion of history is one consequence of this approach. Consider the following claim of Lucas and Hodgson:

The integration of space and time was really required by Newtonian mechanics, and the Special Theory [of Relativity] can be seen in one way as the culmination of Newtonian principles. Although in its historical development the Special Theory was seen as being forced upon physicists as the only way to accommodate the facts of experimental observation, we can, also and more illuminatingly, see it as something to be aimed for in any case, and which, as it happens, yields electromagnetism as a corollary. (p. 2)

There are historical as well as conceptual problems with these views. First, we have already considered some aspects of the historical absurdities that result, namely that Newtonian mechanics and STR were never rival theories competing for superiority among scientists. In addition, the claim that Newtonian mechanics requires  $c$  (the speed of light) to be infinite is a conceptual error. Lucas and Hodgson (1990) want  $c$  to be infinite so that a logical (and empirical?) incompatibility would obtain between Newtonian mechanics and STR, permitting rational theory choice and convergence. However, Newtonian mechanics does not demand  $c$  to be infinite. It *does* specify no upper bound to the propagation of causal influence, but it does not follow from this that  $c$  is infinite. Furthermore, in order to uphold their basic notions of causality—spatial continuity and locality—Lucas and Hodgson claim that Finite Maximum Speed, or even Universal Speed, could have been rationally “derived” directly from Newtonian mechanics (see ch. 1, esp. pp. 25-6, 298-9). They even claim that Einstein could have rationally predicted the result of the Michelson-Morley experiment without recourse to experimental results (pp. 25, 298-9).

The problem with these claims is that electrodynamics is left out of the picture. Developments in electrodynamics, as embodied in Maxwell’s equations, were required before physicists saw Finite Maximum Speed as a problem. There seems to be no place in Newtonian mechanics for Finite Maximum Speed, either conceptually or historically. On the one hand, Lucas and Hodgson (1990) seem to recognize this basic fact. As they argue, Einstein realized that “[o]nly if the Lorentz transformations were involved would Maxwell’s equations be the same from all those points of view which should be regarded as being on a



par” (p. 298). On the other hand, Lucas and Hodgson maintain that Finite Maximum Speed flows directly from Newtonian mechanics (pp. 144-5).<sup>144</sup> Again, they need this latter claim for their argument to succeed regarding the logical incompatibility between the two theories. Then, having decided that their argument succeeds, they take that success to support convergent realism. Clearly, by abusing history, the authors have offered a spurious interpretation of STR and its development.

### III. *History, Scientific Change, and Technological Infrastructure*

This deconstruction of the story of Einstein and STR illustrates how the technological infrastructure construct can make a difference in not only how to critically evaluate a text involving the history and philosophy of science—it also points to how one ought to go about *constructing* an appropriate version of the story of STR, at least by way of negative example. Indeed, both works considered here, Lucas and Hodgson (1990) and Angel (1980), use the history of science to draw support for their ahistorical philosophical accounts of scientific change. The result is failure, because the History-3 generated is anecdotal and distorted; it is not based truthfully on the available History-1 and -2. This is not to say that had they used contextualized historical accounts of the development of STR, they would have succeeded in arguing for convergent realism. The concept of convergent realism has grave philosophical problems that these authors do not seem to have considered.<sup>145</sup>

Perhaps Don Howard (1993) has best stated the lesson we should learn from this story of attempting to do philosophy of science by using (a usually distorted) history of science. In his account of the development of Einstein’s scientific thought, Howard shows that many of the traditional views concerning the author of STR are myths. For example, Einstein was never a hard-core Machian positivist who later in life adopted a realist and deterministic view

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<sup>144</sup> Lucas and Hodgson (1990) suggest that it is Finite Maximum Speed, the “finite maximum velocity for the propagation of causal influence,” (p. 144) that flows directly from Newtonian mechanics. Two paragraphs later, they claim that “we may be led by arguments of continuity to a universal speed, . . . and Newtonian mechanics, although consistent, may none the less lead us on naturally to the Special Theory [of Relativity]” (p. 145). The authors seem to confuse the definitions of Finite Maximum Speed and Universal Speed in these passages. Angel (1980) argues that the assumption of Finite Maximum Speed is not required by Newtonian mechanics (p. 114).

<sup>145</sup> See, for example, Laudan (1981). Popper’s (1963) account of convergent realism, embodied in his ostensibly deductive scheme of falsification, was shown to be internally inconsistent.

of physical science.<sup>146</sup> In fact, he was a Duhemian holist who adopted a philosophy best characterized as “underdeterminationist conventionalism” (p. 221). Lucas and Hodgson (1990) and Angel (1980) all uphold Einstein’s supposed ontological realism as somehow justifying their versions of convergent realism. Furthermore, they discount (or ignore) the conventionalist language Einstein employed in his 1905 relativity paper.<sup>147</sup> Clearly, it serves these authors’ purposes to view Einstein as a realist, and to muster this argument in defense of their philosophical interpretations of STR. In Howard’s view, however, this is not how philosophy of science ought to be done. According to Howard (1993),

the convergentist answer to Duhemian underdeterminationism derives its plausibility only from the frailest of analogies to the mathematical notion of a convergent sequence. It is not obvious that a sequence of theories is like a sequence of numbers. If, at *every stage* in the history of inquiry, one encounters a multiplicity of empirically equivalent theories, then it is not clear why, suddenly, in the infinite long run, that multiplicity should disappear. It seems equally likely that, in the long run, inquiry could take us in any one of many different directions, any one of which would “work” just as well as any other. (p. 220)

Einstein, as Howard shows, accepted this critique of realism, and broke philosophically with Schlick on this point (pp. 220-32).

The lesson Howard draws from his analysis of Einstein’s thought, which clearly applies to the interpretations of STR by Lucas and Hodgson (1990) and Angel (1980), is this: “Too many philosophers of science think that the kind of contact between epistemology and science recommended here involves the philosopher’s searching around in the history of science for thinkers and episodes that serve to validate one’s own methodological claims” (pp. 242-3). Instead of such rational reconstructions, Howard (1993) recommends the following:

Do not ask the Einsteins and the Maxwells, the Newtons and the Aristotles for answers to our own, late twentieth-century questions. Ask them instead what their questions were, and then listen carefully to the answers. We may just find that their

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<sup>146</sup> Lucas and Hodgson (1990) assert that “Einstein himself showed a commitment to there being some sort of reality which it is the scientist’s aim to discover and to understand. Though influenced by Mach’s positivism, he came to repudiate it, and thought of himself not as simply following after sense-experience and cataloguing it, but as aspiring to a God’s-eye view of the world. . .” (p. 252).

<sup>147</sup> See, for example, Lucas and Hodgson (1990), pp. 250-2, 259; and Angel (1980), pp. 125-38.

questions were more interesting than ours. (p. 243)

Following Howard's direction into the twenty-first century, and probing the philosophical, historical, and historiographical issues of the STR story with the framework and tools of the technological infrastructure of science developed in this dissertation, we can come to the following conclusions regarding this particular deployment of the technological infrastructure of science in deconstructing Lucas and Hodgson's (1990) arguments.

#### IV. *How the Technological Infrastructure Makes a Difference for the STR Story*

The purpose of the technological infrastructure construct, as developed in this dissertation, is to show both (1) how stories constructed from the perspective of postmodern naturalism make a difference when considered in the light of a number of philosophical problematics, dualisms, and perennial issues in the philosophy of science; and (2) why focusing initially on a science's technological infrastructure—rather than its theoretical superstructure, as in traditional philosophy of science—including the practice of separating signal from noise, will lead to narratives that resist the dualisms of modernity and their serious conceptual problems. These two activities, focusing on History-4 and -3, respectively, when performed with reverence for truth—that is, conscious adherence to the best available History-1 and -2—ought to result in stories about science that are both empirically compelling and freed from the metanarratives of modernity and the myths of the Enlightenment.

The modern(ist) historiographical framework, underlying Lucas and Hodgson's (1990) and Angel's (1980) attempts to account historically for the supposed transition from Newtonian classical mechanics to the acceptance of Einstein's STR, fails as both History-3 and -4, because of its commitments to convergence and to a view of history that dehistoricizes (or dehistorializes) events in the past. That is, even though Lucas and Hodgson, for example, claim that their notion of causal influenceability helps make sense of some of the everyday experiences we humans have of space and time, and of past, present, and future, they nevertheless adopt a view of historical time and place according to which the events of the past are unproblematically *given* and further can casually, meaningfully, and

*unproblematically* be manipulated, compared, and interpreted from the perspective of a later historical context.<sup>148</sup> Lucas and Hodgson retain the modernist, humanist, objectivist, and privileged stance that dichotomizes the disinterested, disembodied physicist, on the one hand, and the real, objective (in the sense of unproblematically given) world *out there*, on the other hand. This position leads them to claim, with steadfast assurance, the following:

Reality is contrasted not only with what subjectively appears to me, but with what I subjectively aspire to or want. Hence a . . . mark of reality is that it is ineluctable; it exists independently of me, and will continue to exist whether I will it or no[t]. It is potentially recalcitrant to my will, and could force itself upon my attention in spite of my wish that it should not. Reality is something I cannot wish away. (p. 261)

In contrast, the Rousean naturalistic perspective problematizes the objectification of the natural world *out there* and considers us humans as always already in the world. Nature is irreducibly normative and natural phenomena and discursive articulations of them are both required to make sense of scientific practices, which have a fundamentally futural orientation. This perspective precludes the ontological realism of spacetime Lucas and Hodgson (1990) advocate, in addition to their static, atemporal view of history. Local, contextual, and future-oriented knowledge is what those committed to the technological infrastructure are after, not objectified, decontextualized, or synchronically static knowledge that one can manipulate across space and time with the hope or assurance that the unproblematic meaningfulness of those manipulations will be maintained without justification.

Furthermore, Rheinbergerian historicity and its commitment to the rejection of any global structure for science, and to the narrative characteristics of recurrence, compel a rejection of the convergent epistemological realism lurking behind Lucas and Hodgson's

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<sup>148</sup> Lucas and Hodgson (1990) claim that the anisotropy or "directedness of time not only is given to us in experience as a deep, non-adventitious fact, but is fundamental to the presuppositions of much of our conceptual structure. We think that the future is open and to some extent under our control, whereas the past is unalterable and fixed. . . . We can, however, remember the past, whereas we can only predict the future. We distinguish causal explanations, in terms of antecedent conditions and scientific laws or general regularities, from purposive, or . . . teleological, explanations, in terms of some end aimed at. Any view of reality in which the peculiarly temporal characteristics of time played no fundamental part would be one so much at variance with our ordinary experience and understanding that we should have to abandon all our normal ideas about ourselves, our interactions with the world and our knowledge of it" (p. 219).

(1990) and Angel's (1980) philosophizing about STR. Replacing the assumption of a global structure of science with the naturalistic view that one ought to look at knowledge generation in its local context(s) requires that we reject the notion of convergence. Ultimate truth and/or "progress" toward such an ideal are not required to explain the successes of the sciences, nor are they philosophically or historically tenable. We look instead to the scientists' actual historical contexts—but we must remember that these contexts themselves must be reconstructed and are therefore subject to the narrative properties of recurrence, including the notion that such recurrence must be (or should be) performed with the reflective realization that distortions will be inherent in reconstructions of the past. That is, reconstructions must be from the perspective of the present; they are reconstructions for a purpose (i.e., they *matter*); and hence they are inherently political, in the Foucauldian sense that they are embedded in context(s) that presuppose power relations, and in the Rousean sense that even the epistemological concerns of the natural sciences are political in the same way Foucault has shown the social sciences to be characterized by an epistemic politics.

The view of scientific experimentation offered by Lucas and Hodgson (1990) should also be rejected. The view that crucial experiments—which they believe can and have been performed in the history of science—have the disembodied epistemic power to decide unequivocally between two competing theories, reflects a deep-seated philosophical commitment to epistemic sovereignty that permeates the authors' discussions of physics and philosophy. Their urgent need for the convergence of physical scientific knowledge toward truth seems to force them to depend upon such a role for experimentation. For example, in their criticism of the concept of "theory-ladenness," the authors claim that supporters of theory-ladenness argue that "the concepts used to describe any experiment are 'theory-laden,' and therefore there can be no way in which two competing theories can be distinguished, because any experiment designed to choose between them must be described by terms with different meanings" (p. 262). The authors then consider what they take to be a "knock-down refutation" of theory-ladenness—that supposedly given by Franklin (1986)—regarding what will happen to two balls of equal mass, one initially at rest, if they collide and the collision is elastic (p. 263). They agree with Franklin that Newtonian mechanics predicts that the angle

between the balls will be  $90^\circ$  while STR predicts that the angle will be less than  $90^\circ$ , and that since the experiment can be performed in “theory-neutral ways,” theory-ladenness fails and such an experiment would have to be taken to be crucial in deciding between the two theories (*ibid.*).

Clearly, the assumptions built into this argument for crucial experiments beg deconstruction—whether the assumption of elasticity in practice is warranted, whether gravitational influences have been adequately controlled, whether the measurement of the angle’s divergence from  $90^\circ$  may be attributable to the specific materials used in the actual experiment, and so on—in other words, has all the background been eliminated? Has signal been separated from noise? Lucas and Hodgson (1990) seem confident that such crucial experiments are unproblematic.<sup>149</sup>

Although there are no absolutely neutral experiments—to measure an angle presupposes a certain theory of space and rigid motions within it—it does not follow that there are no experiments or measurements that are neutral as between Newtonian mechanics and the Special Theory [of Relativity]. Time and again physicists have been able to perform a crucial experiment which decides between two theories. The experiment is theory-laden, but the theory it presupposes is not either of those being put to the test. (p. 263)

The technological infrastructure requires that such questions regarding the historical development of experimentation not be severed from their historical context. In this case, no such experiment was performed to decide between the two theories, so this is not the actual reason that STR was accepted (and again, evidence shows they were never rival theories). Furthermore, the philosophical view of experimentation embodied in the idea of objective experimenter standing as neutral arbiter above the “outside world” in order to simply test the

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<sup>149</sup> By focusing on theory-ladenness, Lucas and Hodgson (1990) ignore the bigger problem of the Duhem-Quine thesis, or undetermination. Theory-ladenness is a thesis about perception: holders of incompatible theories will interpret the same data or observation in different ways, because each holder could not possibly understand the observation from the point of view of the other’s incompatible theory. According to underdetermination, the same evidence, observation, or data can be accommodated by different, incompatible theories that equally well explain or account for the evidence. Even if the authors have defeated theory-ladenness, this does not result in the vindication of crucial experiments, since the usual argument against naive falsification applies here, namely that there is no logical basis for our throwing out the entire theory of Newtonian mechanics; we could easily admit an ad hoc modification that would allow the theory to fit the evidence. In practice, we could say that it has not been shown that all background has been eliminated and that there is still the possibility the experiment is flawed.

choice between two theories, should be rejected. Lucas and Hodgson's (1990) view of experimentation (see esp. pp. 287-92) reveals a commitment to traditional philosophical thinking about science, rather than a commitment to focusing on actual scientific practice, something toward which even Franklin made strides:

Experiments are made for a variety of reasons: to obtain a more accurate value of some physical quantity, to decide whether one theory or another is correct, or simply to see what happens in an unexplored region of phenomena. Very often the experimentalist will have a fairly clear idea of what to expect; if he did not, he would not be able to arrange the experiment so as to detect it. Most of the time the experiment, if it works at all, gives a result within an expected range: occasionally a completely different result is obtained, and then the experimentalist's first thought is that it is just due to a malfunction of the apparatus, which indeed often it is; if, however, it is not, then he has to start again, to collect himself, to revise his ideas, his models, and his theories. (p. 291)

One need only to compare this view of experimentation with Galison's (1987, 1997) taxonomy of physics experiments to see the divergence in presuppositions between those who are committed to epistemic sovereignty and those who can do just fine without it.<sup>150</sup>

That narrativity is an immensely important concept that should be reflectively and reflexively applied is evident when considering Lucas and Hodgson's (1990) reliance on historical arguments. Crucial experiments, Einstein's realism, experimental specifics—the authors' arguments for these all depend significantly on arguments involving the strategy of specifying “that was the way it actually happened,” or relating the past *wie es eigentlich gewesen ist*. In deploying the technological infrastructure of science, narrativity as an historiographical issue should be a prime concern, before other philosophical concerns are undertaken, when constructing accounts of past science (or the past in general, one could argue).

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<sup>150</sup> Galison's (1987) account of physics experiments lends itself to an interpretation that would have him rejecting epistemic sovereignty and convergent realism. However, as indicated in the discussion of Galison above, he is silent on the question of nonhuman agency, indicating only that reducing all that physicists do to sociological interests is untenable. Galison's (1987) discussions of relativism and rationality (e.g., pp. 11-12, 278) lead to some doubt about what it is he thinks is “really” going on in a physics experiment, even if he unequivocally rejects the notion that experimental results can be “crucial” in the sense that Lucas and Hodgson (1990) want them to be. Galison's (1997) more recent book, as argued above, does not clarify the matter.

Lucas and Hodgson (1990) have a firm philosophical position on science,<sup>151</sup> especially physical theory, one that they wish to generalize in order to say something that, if empirically adequate and coherent, would have profound implications for how science works and for what constitutes its place in our culture:

We are not quite there yet, but so much has been achieved that, in the physical sciences at least, we can now say that we have a rather detailed understanding of nearly all phenomena we encounter, not only by simple observation, but also by sophisticated experiments that create conditions that occur nowhere else, in order to put our theories to the most searching tests imaginable. (p. 292)

However, their stories about science (their History-3s), especially insofar as they are empirical evidence for their defenses of philosophical positions, are inadequate even on a charitable reading. The authors' grounding of the past in memory (p. 219) does not help, since memory as an ostensible repository of "facts" about the past is a notion that itself begs deconstruction. *Geschichtlichkeit* and *Umsicht* both clearly involve memory, yet neither should be reduced to memory. To do so would be tantamount to denying any interpretive role to memory recall or recurrence; in addition, as discussed in Chapter II above, recurrence seems invariably to involve both indeterminacy and distortion. As we know from our own experiences in the world, memory is notoriously unreliable and subject to what Rouse would call the normative authority of the present. In any event, treating memory as a hypostatizing force compelling us to relive the *given* past is fraught with its own epistemological and practical problems.<sup>152</sup>

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<sup>151</sup> Lucas and Hodgson (1990) claim that an "adequate philosophy of physics must be both rationalist and empiricist, espousing a rationalism more chastened than Plato's in the face of recalcitrant facts, and an empiricism more enlightened and explanatory than that of the Logical Positivists. But whereas the extremes are simple to state and easy to embrace, the middle view is complex to articulate and difficult to defend" (p. 269).

<sup>152</sup> Spiegel (2002) shows the inadequacies of recent attempts at grounding or conflating history in/with memory. She argues that "the turn to memory so pervasive in academic circles today forms part of an attempt to recuperate presence in history—a form of backlash against postmodernist/poststructuralist thought, with its insistence on the mediated, indeed constructed, nature of all knowledge, and most especially of the past. In this sense . . . memory has displaced deconstruction as the *lingua franca* of cultural studies. Memory, by becoming virtually hypostatized as a historical agent (one hears talk of how 'archives remember,' of how monuments are materialized embodiments of memorial consciousness, and the like), makes it possible to essentialize and hypostatize the 'reality' which it narrates" (pp. 149-50). The major reason for Spiegel that memory will not work as a ground or substitute for history is that memory and history have different temporal structures (pp. 160-1); that is, insofar as "memory 'reincarnates,' 'resurrects,' 'recycles,' and makes the past



Finally, to the extent that Lucas and Hodgson's (1990) efforts at formulating an historically based philosophy of physics is an attempt (conscious or not) to fit supposed historical "facts" into a framework that supports their philosophical views, it has failed to demonstrate a "reverence for truth." That is, while we can say that insofar as all historians have norms or rules—historiographies or History-4s, implicit or explicit—their History-3s will indeed be struggles to fit History-1s and History-2s into frameworks, it nevertheless remains that the *practice* of reflectively and reflexively engaging the historical evidence with the tools of the technological infrastructure can help the historian to construct more coherent and faithful, if not innocent, narratives. Yes, historians must select the evidence they use; they cannot possibly use all available resources, and the available resources are not guaranteed to reveal anything definitive or even significant about the past. However, if the historical interpretation of an episode is contested in practice, the historian should engage that contestedness and probe its limits, as Hayden White admonished us, in order to capture something of the normative authority of the *present* situation so that the *past* can be reconstructed more fully. Sometimes this will mean entering worlds (in the present) that are alien; how we approach those worlds *matters*. We can approach such a world with cavalier or dismissive attitudes, content with the smugness that our interpretations are superior, or we can self-consciously strive to make sense of that alternative world and present its attributes in a way that most strongly challenges the norms or framework we are trying to advocate. Similarly, we can approach that world with inadequate tools or training, or we can deliberately acquire the tools or training or gain access to someone else who possesses them. Moreover, if in probing the limits of the normative authority of the present situation, there results a scenario in which the framework or assumptions operating are called into question, then the historian should rethink or question his/her own framework and subsequently decide if the contested interpretation of the evidence warrants a major revision or rejection of the favored interpretation.

In the case of Lucas and Hodgson (1990), it is difficult to see how they can be considered to have a reverence for historical truth. Their narrative seems to be constructed

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'reappear' and live again in the present, it cannot perform historiographically, since it refuses to keep the past in the past, to draw the line, as it were, that is constitutive of the modern enterprise of historiography" (p. 162).

with the primary goal of defending the view that the history of the physical sciences can be used to argue that those very sciences are converging over time toward the truth (in the traditional sense of correspondence) of the way the world really is. What is astonishing is that Lucas and Hodgson do not explicitly admit this. The closest they come to an articulation of convergent epistemological realism is when they assert that

science is not just a series of disconnected subjects, but a unified whole. As our theories become steadily more sophisticated, Newtonian mechanics, Maxwellian electromagnetism, quantum mechanics, special and general relativity, grand unified theory and so on, we glimpse the goal of all science, the unification of all phenomena under a single all-encompassing theoretical scheme. (p. 292)

The authors do admit that “an ultimate contingency of things . . . would be to abandon the attempt to understand why things are as they are” (p. 300). But neither a philosophical nor even a brief discussion of convergent epistemological realism is anywhere to be found in their book.

It is instructive to compare Lucas and Hodgson’s (1990) attempt to argue for the reality of STR and for the convergence of physical theory based on (at least in part) historical argument, with Sokal and Bricmont’s (1998) attempt to argue for the intellectual and cultural dangerousness and emptiness of what they call “postmodernism” in their criticisms of how many postmodernists use scientific themes and terms in their work. Here, as with Lucas and Hodgson, these authors enter an alien world not with a reverence for truth, but with an acknowledged *otherness* they have no desire to overcome:

It goes without saying that we are not competent to judge the non-scientific aspects of these authors’ [mostly French poststructuralists] work. We understand perfectly well that their “interventions” in the natural sciences do not constitute the central themes of their *œuvre*. But when intellectual dishonesty (or gross incompetence) is discovered in one part—even a marginal part—of someone’s writings, it is natural to want to examine more critically the rest of his or her work. We do not want to prejudge the results of such an analysis, but simply to remove the aura of profundity that has sometimes intimidated students (and professors) from understanding it. (p. 7)

Sokal and Bricmont (1998) do for postmodernism what Lucas and Hodgson (1990) do for the historical development of STR—they adopt a stance of scientific (and cultural) and epistemic privilege, enter domains of inquiry of which they have insufficient understanding and with

which they share few foundational assumptions, and make ostensibly authoritative pronouncements based on their gross misunderstandings of what they believe “postmodernism” to be.<sup>153</sup> Their book stands as a clear example of how to react intensely to a perceived threat to one’s own “turf” and produce an attack that demonstrates not only a thorough misunderstanding of what those authors would claim to be doing, but also how strong the bifurcation of the perspectives of epistemic sovereignty and postmodernism really is.

The significance of these examples lies in how they relate to the stance one should adopt when committed to the technological infrastructure of science. If the French postmodernists did not sufficiently probe the worldview of mathematics or the physical sciences before using terms and concepts from those fields, so too did Sokal and Bricmont (1998) not penetrate the postmodern worldview. If you want to tell a good story, enter the world of the characters you want to describe as best as you can; that is, do it with the narrative strategies described in Chapters II, III, IV, and above. Indeed, enter their world and tell a story from within their world. Do not adopt a stance you believe gives you the privilege to make objective pronouncements from your disembodied position. In other words, use postmodern naturalist narrativity, not the pre-modernist, nineteenth century historiography of the epistemic sovereigns. In this way you might be able to find some truth in the otherness of the world you are attempting to describe and interpret, instead of producing the self-defeating polemics that were probably the reason you wanted to enter that other world in the first place:

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<sup>153</sup> Ironically, Sokal and Bricmont (1998) do not claim to be bashing the “postmodern left” in their attack on scholars such as Baudrillard, Deleuze, Derrida, Guattari, Irigaray, Kristeva, Laçan, Latour, Lyotard, Serres, and Virilio. Instead, they claim that their “aim is not to criticize the left, but to help defend it from a trendy segment of itself” (p. xiii). Moreover—ironically again—they claim only to be criticizing these scholars for misusing “scientific” terms and ideas in their work, that is, for using concepts from mathematics and the hard sciences in unwarranted ways. This is not to say that they have not found examples of incoherence or absurdity in the writings of these mostly French intellectuals. Nevertheless, what they do not seem to accept is that they have committed the same transgressions they attribute to the French postmodernists. Furthermore, it is difficult to see their book as anything but an angry reaction to what those committed to epistemic sovereignty and the cultural and methodological superiority of science have called “attacks on science” by those who do not have the sovereignty or authority to engage in such attacks (e.g., the “Science Wars”). Sokal and Bricmont are guilty of the very same charge they make of others: “Our aim is, quite simply, to denounce intellectual posturing and dishonesty, from wherever they come” (p. 16).

The deliberately obscure discourses of postmodernism, and the intellectual dishonesty they engender, poison a part of intellectual life and strengthen the facile anti-intellectualism that is already all too widespread in the general public. (Sokal and Bricmont 1998, p. 207)

To resist this duplicitous practice, we should instead, following Umberto Eco ([1983] 1994), create postmodern stories,<sup>154</sup> the construction of which “demands, in order to be understood, not the negation of the already said, but its ironic rethinking” (p. 531).

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<sup>154</sup> Ironically, there is some truth to the claim that postmodern discourse can be difficult. Since postmodernists are trying to break the boundaries of modernity (or modernism), the obscurity of some postmodern discourse may be attributable, at least in part, to attempting to say things that cannot be better expressed in our familiar modern use of language. Katz (1995) provides a brief, ironic analysis of postmodern language use. He shows that the sentence, “Contemporary buildings are alienating” could be thus in postmodern language: “Pre/post/spacialities of counter-architectural hyper-contemporaneity (re)commits us to an ambivalent recurrentiality of antisociality/seductivity, one enunciated in a de/gendered-Baudrillardian discourse of granulated subjectivity” (p. 94). The result is difficult to understand and seemingly absurd. However, one could argue that the following passage from Lucas and Hodgson’s (1990) book is equally as absurd: “If this difference of *rôle* is accepted [between physics and geometry]—and this is a big ‘if’—geometry needs to be subject to more symmetries than physics. As characterized by geometry, space is more homogenous than we hitherto specified, being scale-indifferent as well as origin- and orientation-indifferent: whereas physics, if it is to be mathematical physics, explaining events economically in mathematical terms, needs to regard at least magnitudes, if not absolute spatiotemporal locations, as relevant” (p. 235).

## CHAPTER VI

### Epistemological Problems of the Adaptationist Research Program

Prescriptive, as nearly as I could tell, was like an honest cop, and descriptive was like a boozed-up war buddy from Mobile, Ala.

—Kurt Vonnegut, Jr.<sup>155</sup>

In this chapter, I analyze a selection of the epistemological problems of the adaptationist research program of evolutionary biology in order to apply the technological infrastructure construct to historical case studies involving this research program. The adaptationist research program is central to the theorizing and experimentation of the disciplines of radiation and population genetics; historical episodes in these disciplines I explore below and in Chapters VII and VIII. Specifically, I explore in this chapter the epistemological and scientific issues of *falsifiability* and *testability* to see if the traditional (Popperian, Lakatosian, or other) methodologies offer prescriptions that may aid in resolving these epistemological problems. The issues of falsification and testability are particularly relevant here, as biologists themselves in many fields have taken these issues to be central to their scientific methodology (cf. Platnick 1978). In addition, in debates over creationism from the 1925 Scopes Trial (see Chapter II for an analysis) to Henry Morris's (1918-2006) "scientific creationism" to twenty-first century debates over "intelligent design" and the scientific status of evolutionary theory, testability in particular has been upheld as the major criterion for demarcating science (evolutionary theory) from nonscience (creationism or intelligent design).<sup>156</sup>

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<sup>155</sup> "New Dictionary," in *Welcome to the Monkey House: A Collection of Short Works by Kurt Vonnegut* ([1967] 1998), New York: Dell Publishing, quotation from p. 120. Vonnegut died on 11 April 2007, while I was in the last stages of this dissertation (see Dinitia Smith, "Kurt Vonnegut, Novelist Who Caught the Imagination of His Age, is Dead at 84," *The New York Times*, 12 April 2007, p. A1). He is widely regarded as one of the preeminent postmodern American writers, along with, for example, Philip K. Dick, Toni Morrison, Thomas Pynchon, and Philip Roth.

<sup>156</sup> See, for example, Rick Weiss and David Brown, "New Analyses Bolster Central Tenets of Evolution Theory: Pa. Trial Will Ask Whether 'Alternatives' Can Pass as Science," *The Washington Post*, 26 September 2005, p. A8; Dennis Overbye, "Philosophers Notwithstanding, Kansas School Board Redefines Science," *The New York Times*, 15 November 2005, p. F3; Peter Slevin, "Kansas Education Board First to Back 'Intelligent Design': Schools to Teach Doubts About Evolutionary Theory," *The Washington Post*, 9 November

I show that the adaptationist program *and* many of its alternatives suffer major epistemological difficulties when confronted with traditional falsifiability and testability criteria. I further show that Lakatos' (1978) version of falsifiability, which is perhaps the most sophisticated, offers little help in resolving these difficulties, either by way of prescriptive aid to scientists or in the form of the historical assessment of past science. However, Lakatos's "hard core"—those principles not subject to falsification in practice—does offer a useful springboard for analyzing the epistemological problems of adaptationism,

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2005, p. A1.

In the case of "intelligent design," the creationists' efforts again switched—the Supreme Court ruled in 1987 that creationism could not be part of public school curricula—this time from a focus on showing how creationism *is* scientific, which was the main focus of *scientific creationism* from the 1960s to the 1980s, to a focus in the 1990s and beyond of attempting to deflate the scientific status of evolutionary theory in order to support the argument that "intelligent design," a re-packaged version of Biblical creationism that resists mention of God or scripture, should be taught in public school science classes (see, for example, Peter Slevin, "Teachers, Scientists Vow to Fight Challenge to Evolution: Creationists Seek Curriculum Change; Kan. Education Hearings Open Today," *The Washington Post*, 5 May 2005, p. A3; George J. Annas, J.D., M.P.H., "Intelligent Judging: Evolution in the Classroom and the Courtroom," *The New England Journal of Medicine* 354: 2277-81, 25 May 2006). Phillip E. Johnson (b. 1940), a law professor emeritus at the University of California, Berkeley, has been mentioned as the "father" of the intelligent design movement (Michael Powell, "Doubting Rationalist: 'Intelligent Design' Proponent Phillip Johnson, and How He Came to Be," *The Washington Post*, 15 May 2005, p. D1). However, the Seattle-based Discovery Institute and its Center for Science and Culture, a Christian think tank founded in 1990 and financed in part by some of the same Christian conservatives who helped George W. Bush win the U. S. Presidency in 2000, is regarded as the intellectual and institutional hub of the intelligent design movement, although the Kansas-based Intelligent Design Network and the late Jerry Falwell's Liberty University in Lynchburg, Virginia are two other major supporters (Jodi Wilgoren, "Politicized Scholars Put Evolution on the Defensive," *The New York Times*, 21 August 2005, p. A1).

For Johnson, who is also the author of *Darwin on Trial* (1991) and *Defeating Darwinism by Opening Minds* (1997), adherence to the Henry Morris version of scientific creationism, with its Biblical literalism and young earth, is not necessary. The main point for Johnson is to uphold Christian faith and God's role in designing living things by casting doubt on Darwinism—that is, evolutionary adaptationism with its adherence to natural selection—by arguing that the complexity of life cannot adequately be explained by Darwinism: "Well, I don't have the slightest trouble accepting microevolution as the cause behind the adaptation of the peppered moth and the growth of finches' beaks. But I don't see that evolutionists have any cause for jubilation there. It doesn't tell you how the moths and birds and trees got there in the first place. The human body is packed with marvels, eyes and lungs and cells, and evolutionary gradualism can't account for that" (quoted in Powell, *op. cit.*). Paul Krugman, the columnist for *The New York Times*, has argued that the neoconservative Irving Kristol, former editor of *The Public Interest*, should be considered to be the father of intelligent design, not because he developed the doctrine, but because "he is the father of the political [and rhetorical] strategy that lies behind the intelligent design movement—a strategy that has been used with great success by the economic right and has now been adopted by the religious right." According to Krugman: "The important thing to remember is that like supply-side economics or global-warming skepticism, intelligent design doesn't have to attract significant support from actual researchers to be effective. All it has to do is create confusion, to make it seem as if there really is a controversy about the validity of evolutionary theory" ("Design for Confusion," *The New York Times*, 5 August 2005, p. A15). President Bush himself weighed in on the evolution/intelligent design controversy in August of 2005, by publicly proclaiming that "both sides ought to be properly taught" (quoted in Wilgoren, *op. cit.*).

as it focuses on those commitments that scientists do not and/or cannot subject to falsification. Alternatively, I suggest that an alternative way of “solving” these epistemological problems involves applying the technological infrastructure construct and rejecting the traditional notion that the grounds for the epistemological superiority of science are to be found in a sovereign epistemology or methodology. The key is to focus on the *practice* of science, and practice includes experimenting *and* theorizing. Following the prescriptions of the technological infrastructure construct, scientific practice must be seen as embedded in historical, cultural, and narrative context—epistemic sovereignty should be abandoned for epistemic authority.

Moreover, when exploring adaptationism and historical episodes in which it is involved, a salient principle of the technological infrastructure that should be considered is *the promise for future research*. That is, when considered in its historical/epistemological/political context, scientific practice involving adaptationism must develop a promise for acceptable future research activity in order for it to be successful, yet this promise, as it captures diachronic futural norms in the Rousean (2002a) sense, must await future developments in order to assess its viability. Without this futural prospect, success in practice is difficult and doubtful, since research programs depend on such goals, including for funding purposes. And if it turns out that *testability* is the major criterion in practice that scientists, philosophers, and the general public declare to be what makes a proposition or theory scientific, and further if adaptationist evolutionary theory is fraught with epistemic problems regarding whether it is testable in some more or less well-defined sense of the term, then we are justified in asking: what is it that has made adaptationism (and other scientific research programs) such a successful and well-defended scientific research program (or metanarrative), while at the same time constantly under attack, at least in the United States, from individuals and groups holding various religious positions? I explore how the technological infrastructure of science, with its focus on locating how signal/noise is separated and on how successful research programs contain the prospects for future research, in addition to its postmodern historiographical and epistemic/political configuration, can help to answer significant historical questions regarding the classical/balance controversy of

population genetics and its related historical contexts. In Chapters VII and VIII below, I undertake reconstructing the classical/balance controversy using the tools of the technological infrastructure. First, however, I consider the adaptationist research program and its epistemological problems.

### **The Adaptationist Research Program**

The authority of the adaptationist program has repeatedly come under attack since the advent of the modern evolutionary synthesis of the 1940s and 1950s, particularly by scientific creationists. As outlined in Chapter II above, after Ronald Reagan made teaching creationism in public schools a plank in his 1980 presidential campaign, the tactics of creationists became not so much to show that evolutionary theory is *unscientific*, but rather to demonstrate that creationism *is* scientific, and further that it is more empirically successful than evolutionary theory, which has limits to what it is able to explain about the diversity of life on earth (Numbers 1986). However, the creationists' arguments invariably also hinge on nibbling away at the epistemological status of evolutionary theory, thereby ostensibly exposing it as unfalsifiable. As Ruse (1984a) states: "There are, we are told, absolutely no facts whatsoever that would count against evolutionism." (p. 350) Ruse's characterization of the creationists' position is probably an overstatement; creationists such as Henry Morris devoted more effort to showing that creationism is more probable (or more empirically successful, to put it in the terms of the philosopher of science) than evolution, than to debating with philosophers of science on *their* turf, as with falsification and testability (Morris 1993, Numbers 1986, 1992). Moreover, it was Ruse himself (with others) who was called in 1981 as an expert witness by the American Civil Liberties Union to testify against creationists in Arkansas (Ruse 1984b). In this case, Ruse invoked falsificationism as a weapon against the creationists, arguing ironically that creationism is not scientific because it is not falsifiable. The judge in the case agreed (Overton 1982), and Popper's (1963) version of naïve falsification was thereby legitimized as a legal criterion to demarcate science from non-science.



The question to be explored is, what are the epistemological problems of evolutionary theory that lend credence to the notion that adaptationism *itself* has problems responding to criteria of falsifiability and testability? To explore this, I concentrate on the adaptationist program, which has been the predominant view in evolutionary theory since the “hardening” of the modern evolutionary synthesis in the 1940s and 1950s (Gould 1982). The adaptationist research program, when sufficiently scrutinized, can be shown to have serious epistemological problems when considered from the viewpoint of traditional philosophy of science. Moreover, the very concepts of adaptation and adaptedness, not to mention fitness, have been fraught with difficulties concerning their usage and appropriate definitions.<sup>157</sup>

Richard M. Burian (1983) provides a useful corrective to what he considers the prevalent misuse by evolutionary biologists and philosophers of science of the concepts of “fitness,” “adaptation,” and “adaptedness.” According to Burian (1983),

the development of the modern neo-Darwinian (or synthetic) theory of evolution has, in fact, minimized the role of Darwin’s concepts of adaptation and adaptedness and obscured their importance by running them together, in a confused and confusing fashion, with a whole battery of other related notions. . . . The widespread use, and occasional misuse, of these notions has, at times, contributed to the illusion that evolutionary theory is viciously circular or tautologous. (pp. 288-9)

This compelling description of the widespread perception of the tautologous nature of evolutionary theory fuels the notion that it is unfalsifiable. For example, Darwin’s theory of natural selection is often equated with the view that the fittest organisms survive—“survival of the fittest.” Fitness, in turn, has been interpreted by many as referring to actual survival and reproductive success (Mills and Beatty [1979] 1984, p. 38). From this definition comes the charge of tautology. As Mills and Beatty ([1979] 1984) explain,

where fitness is defined in terms of reproductive success, to say that type A is fitter than type B is just to say that type A is leaving a higher average number of offspring than type B. Clearly, we cannot say that the difference in fitness of A and B *explains* the difference in actual offspring contribution of A and B, when fitness is defined in terms of actual reproductive success. (p. 38)

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<sup>157</sup> Significant analyses of these concepts, in addition to the works considered here, include works by Gould and Eldredge (1977); Brandon and Burian (1984); Richardson and Burian (1992); Brandon (1996, section I); Sober (1998); Hull (2001, part I); Hull, Langman and Glenn (2001); Grene and Depew (2004, chs. 8-10); and Burian (2005).

Clearly, nothing here has been explained in the Hempelian sense (Michod 1986). Natural selection, or “survival of the fittest,” has become the “‘survival of those which survive,’ because the fittest are by definition those which survive” (Ruse 1984a, p. 351). Creationists then seize upon this tautology, and claim that the theory of natural selection is unfalsifiable. What experiment, they ask, can be devised or what evidence can be adduced that will count against natural selection? If the fittest are by definition those organisms that survive, because they are better adapted to the environment in question than other organisms of the same species, how does one make testable predictions about which organisms will be better adapted? Notice how adaptedness has been smuggled into this discussion of fitness in order to show the relevance of this analysis for adaptationism. The creationists delight in this example, and exploit it in undermining the status of evolutionary theory in order to argue that creationism is just as empirically successful, or even more so (cf. Ruse 1984a).

As it turns out, there are other grounds for rejecting the interpretation of fitness as actual reproductive success. An often-cited example (in varying forms) is the following: Two genetically and phenotypically similar (or even identical) organisms inhabit the same environment (ecological niche or ecospace). Naturally, we would want to assign them the same fitness value. One of them, however, is killed by some accidental, physical act of nature (lightning, meteor, aliens, or the like). The remaining organism goes on to reproduce successfully. On this interpretation, we would be forced to conclude that the unharmed organism was much more fit than the one killed—it had good reproductive success, while the latter did not (see, for example, Brandon 1978, pp. 193-4; Mills and Beatty [1979] 1984, p. 40; Burian 1983, p. 290; Beatty 1984, p. 192).

This difficulty can be removed by reinterpreting fitness as a *propensity* to reproduce (see, for example, Brandon 1978, Mills and Beatty [1979] 1984, Sober 1980, Burian 1983, Richardson and Burian 1992). On this interpretation, fitness is not to be measured by counting the actual number of offspring produced, but should be regarded as the organism’s “*propensity to survive and reproduce in a particularly specified environment and population*” (Mills and Beatty [1979] 1984, p. 42). There is no longer a tautology, because there is no simple identity between fitness and actual reproductive success (although significant issues

remain; see, for example, Beatty and Finsen 1989, Richardson and Burian 1992).

Referring to the above example, we are no longer forced to assign a higher fitness value to the surviving organism as compared to the one accidentally killed. Since fitness is now an expectation value based on the propensity—the *expected* number of progeny to be produced (Sober 1993, p. 58)—there are grounds for assigning both organisms the same fitness value. In making this claim, one can now allow for circumstances in which the most fit might not survive (such as a chance lightning strike). More importantly, the propensity interpretation of fitness points out that there are more factors involved in determining fitness values than actual numbers of offspring produced. The environment and the organism's relation to the environment must be taken into consideration. This brings us back to the concepts of adaptation and adaptedness. We are still in search of a theory of evolution that is testable.

### I. *Adaptation and Adaptedness*

As indicated above, Burian (1983) argues that the modern evolutionary synthesis has made a mess of the concepts of fitness, adaptation, and adaptedness. Given that fitness is to be considered a propensity to survive and reproduce, and that natural selection operates to select the more fit, what then is the basis for selection? The answer normally given is that natural selection selects those individuals that are better adapted to their environment. Borrowing Burian's (1983) usage, let us define 'adaptedness'—or "relative engineering adaptedness"—as the following:

A (type of) feature or a (type of) individual possesses higher relative engineering fitness [or adaptedness] than an alternative type if, and only if, its design manifests a better engineering solution within the appropriate (real) design constraints to a specific (real) challenge or range of challenges posed by the environment. (p. 295)

A feature or trait, then, is an "adaptation"

if, and only if, its design characteristics were produced as a *causal consequence* of their relative engineering fitness as compared with those of relevant alternative types, as a solution to a problem or range of problems posed by the environment in the evolutionary history of the organism in question. (p. 295, emphasis added)

Notice that 'adaptedness' is used in an ahistorical, or synchronic, sense. That is,

claiming that an organism is well adapted to its environment, or that a trait or character of an organism is well adapted to its particular function, effectively says nothing about the process by which the trait was produced. Furthermore, the term ‘adaptation’ *is* used in an historical, or diachronic, sense, for it is a claim about the causal, historical process that produced the trait. A trait is an adaptation if it was the causal consequence of natural selection. That is, the fact that the trait conferred an advantage to the organism that was translated into survival and better reproductive success, explains the origin of the trait as well as its current condition of adaptedness to the particular design problem. A significant outcome of this analysis is that a trait with a high value of adaptedness or fitness need not be an *adaptation* (see, for example, Lewontin 1977, 1978; Brandon 1978; Burian 1983; and Beatty 1984). This suggests that other mechanisms may have been at work in producing the characteristics of organisms. Natural selection is not the only possible agent of evolution and, for certain traits in certain organisms in certain environments, it might not even be significant at all.

## II. *The Hardened Evolutionary Synthesis*

In their now classic paper, Stephen Jay Gould and Richard C. Lewontin (1979) argue that the chief characteristic and outcome of the modern evolutionary synthesis has been a firm commitment to “adaptationism” as a research program by scientists in many fields. Developments in the modern synthesis—the unification of diverse fields of science for grounding and legitimating Darwinian evolution (in the 1930s, 40s, and 50s)—served to uphold natural selection as the ubiquitous mechanism of evolutionary change (cf. Gould 1982, 1983). Population genetics, paleontology, systematics, morphology, and other fields joined forces, so to speak, to legitimate and claim scientific status for Darwinian evolutionary theory (see, e.g., Mayr and Provine 1980, Cain 1993, Larson 2004, ch. 10).

In this manner, adaptationism, with its ubiquitous micro-level mechanism—natural selection—became established as the modern metanarrative<sup>158</sup> of the newly emerging

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<sup>158</sup>Again, I draw upon the work of Lyotard (1984 [1979]) and his conception of a metanarrative as a master-narrative that consciously or unconsciously structures or guides one’s way of thinking. Lyotard instructs us to maintain “incredulity toward metanarratives” (p. xxiv). One way of conceptualizing this is to think of a metanarrative as a totalizing philosophical, historical, and/or scientific methodology or grand theory (see, for example, White 1991, and the examples in Chs. II and III above). Accepting a totalizing metanarrative amounts to

synthesis of a variety of historical natural sciences. That is, adaptationism was legitimated as a totalizing, overarching research norm that was seen as guaranteeing testability and, in addition, the promise of future experimental research for numerous scientific disciplines, including population genetics. It had a mechanism that could be applied to all the relevant experimental situations in an ostensibly law-like manner. However, as the following analysis reveals, major epistemological and experimental problems plagued (and plague) the evolutionary sciences when guided by the adaptationist metanarrative.

In their deconstruction of adaptationism, Gould and Lewontin (1979) argue that the adaptationist program fails to distinguish between adaptedness and adaptation, or “current utility from reasons for origin” (p. 581). In doing so, the adaptationist program

regards natural selection as so powerful and the constraints upon it so few that direct production of adaptation [or adaptedness] through its operation becomes the primary cause of nearly all organic form, function, and behavior. Constraints upon the pervasive power of natural selection are recognized of course. . . . But they are usually discussed as unimportant or else, and more frustratingly, simply acknowledged and then not taken to heart and invoked. (pp. 584-5)

What we have, then, is that *every* instance of adaptedness is an adaptation. Hence, the research program proceeds as follows:

- (1) If one adaptive argument fails, try another. . . .
  - (2) If one adaptive argument fails, assume another must exist; a weaker version of the first argument. . . .
  - (3) In the absence of a good adaptive argument in the first place, attribute failure to imperfect understanding of where an organism lives and what it does.
  - (4) Emphasize immediate utility and exclude other attributes of form.
- (pp. 586-7)

In addition, Gould and Lewontin (1979) object to the adaptationist program because they consider it to be not falsifiable: “We would not object so strenuously to the adaptationist programme if its invocation, in any particular case, could lead in principle to its rejection for want of evidence” (p. 587). Using the strategy outlined above, how is one to adduce evidence that would count against the adaptationist program? We are led back to the

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holding (not necessarily consciously) to the truth or finality of such a methodological constraint or metatheory. If adaptationism can be successfully “told” (i.e., told as a story or series of stories) in this way, then the usefulness and appropriateness of literary theory in general, and postmodernism in particular, for analyzing scientific practice is supported, as the need for having a theory of the story of stories, or metahistoriography, is highlighted.

creationist's claims of unfalsifiability, although the situation is now changed. Instead of unfalsifiability resting on a naïve claim of tautology (concerning fitness and “survival of the fittest”), the landscape is more complicated and potentially serious, assuming Gould and Lewontin's characterization of the adaptationist research program is accurate. Their claim of unfalsifiability is not, however, based on Popper's naive falsificationism (see, e.g., Lakatos 1978, pp. 20-47). We should not expect one experiment to falsify an entire research program. Nevertheless, a problem remains in that it seems no empirical evidence would count against adaptationism. Adaptationism, it seems, is the “hard core” of the adaptationist research program of evolutionary theory.

Again, a major reason for the adoption of adaptationism as the hard core of the adaptationist program (an hypothesis not only *not* subject to falsification by its adherents, but if accepted, perhaps unfalsifiable in principle) is that it ensures the promise for future research. Put another way, although the hard core of the program might be unfalsifiable, it at least provides the illusion of testability, or we might say, it allows experimental testing of hypotheses in *practice*, even if there are epistemological problems with the adaptationist metanarrative that is structuring those experimental tests. Moreover, if certain traits were not adaptations, it seems choosing among the possible alternatives would complicate the situation to the extent that a practical basis for choice would not be realizable—that is, the alternatives would not be experimentally testable (Lewontin 1978, Platnick 1978, Mayr 1983). Before such issues are examined, let us ask, what are the alternatives or challenges to the adaptationist hard core of evolutionary biology?

### **Alternatives to the Adaptationist Program**

One possible alternative to adaptationism as an explanation of adaptedness is that no adaptation or selection need be invoked at all (Gould and Lewontin 1979, p. 590). Random genetic drift of alleles in a population is one candidate. With genetic drift, unfavorable alleles could become fixed in a population, and this could happen in the absence of any selective pressures. However, as Beatty (1984) points out, to distinguish between the role of

natural selection versus the role of genetic drift in a given instance of evolution can be problematic. In one example, Beatty considers a species of moths with two types, light-colored and dark-colored, in an environment with trees, 40% of which have light-colored bark and 60% of which have dark-colored bark. The moths have a natural predator that is color-sensitive. Hence, the dark moths have a higher fitness since the environment provides them with better camouflage. However, we find that in one generation the predator killed a greater percentage of dark moths than we would expect based on the population. Further, we find that the remains of the moths indicate that dark moths were killed while on light trees, and light moths were killed while on dark trees. Dark moths happened to land on light trees more often than dark, although the percentage of dark trees was greater (pp. 194-5).

The question Beatty (1984) asks is whether the “change in frequency of genes and genotypes in question [is] a matter of natural selection, or a matter of random drift?” (p. 195). Is this natural selection discriminating upon the basis of fitness differences, or is the “indiscriminate” sampling process of random genetic drift at work? Beatty concludes that it is “difficult to distinguish between random drift on the one hand, and the *improbable results of natural selection* on the other hand” (p. 196). This again raises the issue of testability. Even if we soften the hardened adaptationist program and allow for random drift, we do not necessarily gain anything on the testability front. For example, Gould and Lewontin (1979) chide certain adaptive hypotheses for being “untestable speculations” (p. 587). It seems they really want to say unfalsifiable, since tests can be performed to “confirm,” yet this still leaves open the question of adducing negative evidence. In this case, however, the alternative to adaptationism makes no guarantee for falsifiability or testability.

A second alternative to adaptationism is that the part or trait in question is “a correlated consequence of selection directed elsewhere” (Gould and Lewontin 1979, p. 591). Examples of this are pleiotropy and allometry. The problem of developmental constraints and their role in explaining adaptedness has received serious consideration by biologists, philosophers, and others (e.g., Maynard Smith *et al.* 1985). These alternatives add to the epistemological problems of adaptationism.

Concerning pleiotropy, it may be that a gene mutation (or recombination or drift) may

cause multiple changes in the physiology of the organism. Natural selection may select a particular gene because of one specific effect it produces that confers an advantage on the organism, yet another physiological effect produced by that same gene may be carried along (Lewontin 1978, p. 228). An instructive example of Lewontin's concerns a gene that codes for an enzyme that "detoxifies poisonous substances by converting them into an insoluble pigment. . ." (p. 228). The pigment causes the color of the organism to change, yet "no adaptive explanation of the color per se is either required or correct" (p. 228). In this example of pleiotropy, natural selection was operating. However, the trait or function in question was not *selected for*, to use Sober's (1984) terminology. There was *selection of* the color of the organism, but it was a result of color being correlated to the action of selection on the gene for another of its effects. This example seems to necessitate that an acceptable theory of genetics and its role in evolution be developed. Lewontin (1974) has explored the epistemological problems of evolutionary genetics in *The Genetic Basis of Evolutionary Change* and found numerous serious problems. Again, the role of natural selection in evolution, or in particular instances of evolution, is in question. How can scientists test the relative contributions of selection, chance, and other evolutionary mechanisms?

Another candidate for "selection directed elsewhere" is allometry, or the differential growth rates of different parts of an organism. Again following Lewontin (1978), allometry is such that it "shows up both between individuals of the same species and between species" (p. 228). One example is brain size in primate species. Brain size for primates increases more slowly than body size, so smaller apes have comparatively larger brains than larger apes. And, as "the differential growth is constant for all apes, it is useless to seek an adaptive reason for gorillas' having a relatively smaller brain than, say, chimpanzees" (p. 228). Lewontin (1977) also offers a similar argument for tooth size in primates. For primate teeth, the size "grows larger more slowly between species than does body size. . ." (p. 22). So, larger primates have relatively smaller teeth than smaller primates. If this trend holds constant for all primates, then we would err in concluding that natural selection has selected small tooth size in gorillas. The trend would instead be adequately explained as "purely allometric." Lewontin concludes by stating that since developmental correlations are often



“quite conservative in evolution,” a proper examination of many “adaptive trends” will reveal them as actually being allometric relationships (p. 22).

In their study of developmental constraints, Maynard Smith *et al.* (1985) have shown that Lewontin’s above conclusion may not be justified (pp. 227-8). They challenge the claim that “the presence of an allometric relation across adults demonstrates the operation of a developmental rather than a selective constraint” (p. 277). As they report, comparative studies reveal that allometric relations themselves may be the product of natural selection. In the primate tooth example, Kay (1975) found an adaptive relationship between tooth size and diet in primates. The significance of this is that the demonstration of allometry need not imply *ipso facto* that selection rather than developmental constraints is at work. Maynard Smith *et al.* (1985) conclude: “What requires further examination is the ease with which allometry can be broken. Only then will we understand the degree to which it is properly interpreted as a constraint” (p. 277). Once again, how to choose between these alternatives is problematic; experimental testing on one of them might not be able to discriminate among the alternatives. In addition, devising a test to decide which is in operation might not be possible.

A third alternative to the adaptationist program involves the “decoupling of selection and adaptation” (Gould and Lewontin 1979, p. 592). One possibility is natural selection without adaptation. For example, natural selection will favor higher fecundity, but it may not be adaptive in every instance, as when resource limitation kills off excess offspring (Lewontin 1979, p. 13). A second possibility is adaptation without selection. Particular examples of Burian’s (1983) relative engineering fitness, or adaptedness, may turn out to be cases of “phenotypic plasticity” (Gould and Lewontin 1979, p. 592). That is, the good engineering design observed in a particular case, which appears to be an adaptive fit (an adaptation, implying historical causation), may instead be the result of physical constraints in the environment acting on phenotypic plasticity, as when sponges take on particular shapes as a result of the local currents (p. 592). What appears to be an adaptation is in fact (or may be) an example of adaptedness *without* a causal history of natural selection. Testing whether an organism is genetically coding for the characters in question, or whether the characters are

the result of developmental constraints, is not necessarily a straightforward matter in every case (Maynard Smith *et al.* 1985).

Gould and Lewontin's (1979) fourth alternative to the adaptationist program is "adaptation and selection but no selective basis for differences among adaptations" (p. 593). Different groups of organisms within the same population may develop different engineering design solutions to the same environmental or ecological problem. If the situation is such that there is no adequate basis for deciding which adaptation is the better solution to the problem, we have the historical problem of having to answer why one design was "chosen" over another (p. 593). It seems that tests could be devised to show if, in fact, one design is more superior to the other in terms of relative engineering fitness, or adaptedness, but reconstructing the past to explain equivalent solutions to the same problem could lead to further problems for falsifiability and testability.

One final alternative to the adaptationist program concerns the operation of adaptation and selection, but with the adaptation in question being "a secondary utilization of parts present for reasons of architecture, development or history" (Gould and Lewontin 1979, p. 593). Gould and Lewontin's example is instructive: "If blushing turns out to be an adaptation affected by sexual selection in humans, it will not help us understand why blood is red" (p. 593). In response to this problem, Gould and Vrba (1982) distinguish between traits that were selected for their present function or engineering design solutions, from those that were produced as indirect correlations with the present adaptation or from natural selection for an entirely different use. The former they term "adaptations," while the latter they wish to call "exaptations." This once again points out the crucial difference between adaptedness and adaptation: "The immediate utility of an organic structure often says nothing at all about the reason for its being" (Gould and Lewontin 1979, p. 593). How often is "often," and how to decide in particular cases whether the condition of adaptedness is an adaptation, is not always clear-cut in practice.

## Further Epistemological Problems

In the above overview of alternatives to the adaptationist program, I reviewed some of the epistemological problems of the adaptationist research program and some of its alternatives. I now focus on several further problems of adaptationism, especially as they pertain to the metanarrative of the adaptationist program, the preeminent evolutionary research program that arose out of the hardened modern evolutionary synthesis of the 1930s, 40s, and 50s. It is this research program that has “dominated evolutionary thought in England and the United States” since the formation of the modern synthesis (Gould and Lewontin 1979, p. 581). What this analysis seems to suggest is that the *historical* nature of the questions for which scientists are attempting to elicit answers—these questions invariably involve reconstructing various accounts of causality in the history of life—has caused epistemological problems for the practice of science. That is, many of the same problems confronting historians seem to be in practice challenging evolutionary biologists in their attempts to reconstruct the history of life. These problems include whether to presuppose a definitive account of this history can be constructed, or if it could be identified as such; how to appropriately situate a past story of adaptation in a suitable (narrative or historical) context, now a context with an exponentially longer time span, yet without History-1 in written form; and many of the other epistemological problems inherent in historical practice that were analyzed in Chapter II above. The scientific narrative strategy of atomizing particular stories of selection in the past—that is, objectifying the story by treating it as a *ceteris paribus* story, and manipulating it as if its *context* does not matter—this *narrative* strategy of creating textual stories from the manipulation of material/discursive arrangements, has contributed to the problems in scientific practice of falsifiability and testability. These problems, in turn, have contributed to the arguments for why adaptationism is seen as the untestable grounding for evolutionary theory, rather than what is at stake in the practice of experimental biology.

### I. *Environmental Tracking Theory*

As Gould and Lewontin (1979) point out, one of the central research tactics of the

adaptationist program is to “atomize” an organism into “traits” and then explain these traits as “structures optimally designed by natural selection for their functions” (p. 585). As Burian (1983) suggests, it is instructive to view the concept of relative adaptedness as the “relative engineering adequacy of a design (given the appropriate constraints) as a solution to a particular problem” (p. 293). Nevertheless, as Lewontin (1977, 1978) argues, there are difficulties with this formulation. To atomize or divide up organisms presupposes that one can adequately decide what are the units of selection (Lewontin 1978, p. 217-20). Furthermore, this view presupposes that there are preexisting “problems” to which evolution provides “solutions” (Lewontin 1977, p. 4). What, then, is the preferred way to cut up the world in order to define the ecological niche that serves as the basis for the engineering design solution? As Lewontin (1977) argues, “there is a non-countable infinity of conceivable ecological niches” (pp. 4-5). What remains, then, is an epistemological problem concerning how to specify the various ecospace, and how to determine which of them are relevant to the particular problem at hand.

One possible way out of this quandary involving niches is the theory of environmental tracking, or the “Red Queen hypothesis”<sup>159</sup> (Lewontin 1978, pp. 215-20). According to this theory, one should hold that environments are “constantly changing with respect to the existing organisms so that the organisms must evolve to maintain their state of adaptation [or adaptedness]” (Lewontin 1977, p. 5). This solves the problems of sudden increases in the population size and geographical range of a species, and of the inevitable extinction of all species (p. 5). As Lewontin (1977) argues, over “99.9% of all species that ever existed are extinct and all are sure to be extinguished eventually” (p. 7). Environments are certain to change over time in such a way that the organism’s tracking ability to respond adaptively to changes will be outpaced by the rate of environmental change, especially given the conservatism of environmental constraints and the limitations of phenotypic and genotypic plasticity (p. 7).

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<sup>159</sup> Grene and Depew (2004) indicate that this concept originated with Sewall Wright’s (1931) seminal paper, but was later named “‘The Red Queen Hypothesis,’ after the character in *Alice in Wonderland* [the red chess queen from Lewis Carroll’s *Through the Looking Glass*] who must run very fast just to stay in place” (pp. 251-2). They cite Leigh Van Valen (1973), “A New Evolutionary Law,” *Evolutionary Theory* 1: 1-30. See also Lewontin ([1998] 2000), pp. 57-8.

A major problem remains unsolved, however. Environmental tracking cannot explain the diversity of life (Lewontin 1977, 1978). Therefore, it does not adequately explain evolution by natural selection. For adaptation (and the adaptationist program) to be “scientific,” it must satisfy the condition of “epistemological applicability” (Brandon 1978, p. 190). That is, the concept of relative engineering adaptedness must be testable; it must possess predictive power. However, without constructing *a priori* ecological niches, this condition cannot, it seems, be met. In Burian’s (1983) terminology, we need a notion of “selected engineering fitness” (or adaptedness), that holds when relative engineering fitness is the result of specific adaptations (p. 295). Lewontin (1977) describes the problem as follows:

There is no way to use adaptation as the central principle of evolution without recourse to a predetermination of the states of nature to which this adaptation occurs, yet there seems no way to choose these states of nature, except by reference to already existing organisms. (p. 8)

Again, this epistemological problem highlights both the practical problems with the conduct of experimentation under the umbrella of the adaptationist program, as well as problems with popular and professional conceptions of the meanings of “scientific” and “testable.”

## II. *Ceteris Paribus Arguments*

The previous discussion of environmental tracking leads us back to the “atomization” research strategy described by Gould and Lewontin (1979), that is, a discussion of specific adaptations. The adaptationist metanarrative guides researchers to presuppose that all aspects of an organism’s physiology, morphology, and even behavior have been produced by natural selection as engineering solutions to various design problems given by the relevant ecological niche (Lewontin 1978, pp. 216-17). Hence, each organism is atomized into parts, and the task of the evolutionary biologist is to explain how each part functions as an adaptation (in the present and historical senses). Again, there are difficulties with this approach.

In explaining what parts of an organism are adaptations, and what the problem is that the part “solves,” the evolutionary biologist is making an argument about the historical origin

of the part or trait (Lewontin 1978, p. 218). In doing so, the evolutionary biologist must decide whether the trait is a primary adaptation directly selected for, or an exaptation, a secondary correlated character. To make this decision, one usually employs an engineering analysis of the organism and its relation to its ecological niche. As Lewontin (1978) argues, a “basic assumption” of such analyses is that of *ceteris paribus*, or “all other things being equal” (p. 220). That is:

In order to make an argument that a trait is an optimal solution to a particular problem, it must be possible to view the trait and the problem in isolation, all other things being equal. If all other things are not equal, if a change in a trait as a solution to one problem changes the organism’s relation to other problems of the environment, it becomes impossible to carry out the analysis part by part, and we are left in the hopeless position of seeing the whole organism as being adapted to the whole environment. (p. 220)

Again, the situation seems “hopeless” because of the implications of the traditional notions of testability and falsifiability. Indeed, experimentation itself—separating signal from noise—requires *ceteris paribus* arguments. The atomization thesis, which provides predictive power to natural selection by predicting *in advance* those organisms that will be more fit (better adapted), removes the tautology of “survival of the fittest” discussed above. The engineering design and solution ensure that such predictions can be made. Without them, things get much messier. We are led to the alternatives to the adaptationist program, each of which has its own difficulties concerning falsifiability and testability.

### III. *Frequency-dependence and Density-dependence*

Two other problems concerning the adaptationist program are worth mentioning. One involves natural selection that is frequency-dependent. That is, the genotypic and phenotypic makeups of the population in question can, in certain environmental situations or experimental setups, affect the reproductive success of different variants. Following Burian’s (1983) example concerning replicate populations of *Drosophila* raised at different temperatures, one must be careful in making determinations involving which variants are better adapted to certain temperatures on the basis of observing the resulting frequency of types at stable equilibrium. For if reproductive success of one type changes as a function of

the proportion of each type in the population, then the selection coefficients are frequency-dependent (p. 300). One must instead use a notion of fitness as a *propensity* to survive and reproduce (Mills and Beatty [1979] 1984). In Burian's (1983) terminology, this means invoking a notion of relative or selected engineering fitness (or adaptedness). Along with this come the problems of adaptedness and adaptation discussed above.

Concerning density-dependence, Lewontin (1978) shows that natural selection need not, in all cases, lead to greater adaptedness (pp. 222, 225). For example, greater fecundity does not necessarily imply greater fitness or adaptedness. While individuals with greater fecundity might, for example, be protected against random fluctuations in temperature, if the greater numbers of offspring produced made them more susceptible to disease or predation, it is problematic to conclude that those with higher fecundity are better adapted to the environment. As Lewontin (1979) concludes, "there is no way we can predict whether a change due to natural selection will increase or decrease the adaptation [adaptedness] in general" (p. 225).

In the case studies explored below in Chapters VII and VIII, the present analysis of the epistemological problems of the adaptationist research program are fleshed out with the historical example of radiation genetics in the Cold War. First, however, I explore the work of Theodosius Dobzhansky, a founding theoretician of population genetics, as well as the controversy over how to interpret the experimental findings on population genetics in the 1950s and 1960, the classical/balance controversy.

### **Theodosius Dobzhansky and Adaptationism**

In order to explore how the technological infrastructure can help probe the adaptationist metanarrative and the issues of testability and falsifiability, in addition to the classical/balance controversy of population genetics, I consider the work of the population geneticist Theodosius Dobzhansky (1900-1975) as interpreted by Lewontin (1974, 1981,

1987, 1995b, 1997a, 2006), and others.<sup>160</sup> First, it is worth mentioning again one of the main tenets of evolutionary theory as often given by philosophers of science. According to Brandon (1978), the following “law of nature” is required as foundational to evolutionary theory:

(D) If *a* is better adapted than *b* in environment *E* then (probably) *a* will have more (sufficiently similar) offspring than *b* in *E*. (p. 189)

In order to ensure the scientific status of (D), according to traditional philosophy of science, (D) should meet the following conditions, which were explored above: “(a) independence from actual reproductive values; (b) generality; (c) epistemological applicability; and (d) empirical correctness” (p. 189). Brandon (1978) concludes that *no* formulation of the concept of relative adaptedness will satisfy all of the above requirements (p. 203). He proposes we give up hope of trying to falsify (D), for “no amount of falsification of instances of (D) even begins to falsify (or disconfirm) (D)” (p. 203). Particular instances of (D) are testable, but (D) must first be *presupposed*—that is, experiments must be performed by first assuming (D) is true. If Gould and Lewontin (1979) are correct, this contrasts sharply with the following critique of the dominant view among scientists of the adaptationist program:

If we find a feature that is adaptive but is not selected for, then and only then will selective theory itself be falsified. In practice, this never occurs, because lack of success is itself tautologically taken as evidence of nonadaptiveness. If we refuse to adopt such ad hoc defense of selective theory, it may well be both falsifiable and false when applied at the level of features. (Platnick 1978, p. 348)

The above statement, however, presupposes a naive view of falsification, one that ignores the obvious problems with Popper’s (1963) version, especially the problem of *what* is to be falsified. Do we falsify only the hypothesis or theory, or the background assumptions and ad hoc modifications as well? Are we really to believe one instance of nonadaptiveness will falsify natural selection, even at the level of features—a true *experimentum crucis*? It

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<sup>160</sup> The preeminent work on Dobzhansky and his work is the volume edited by Mark B. Adams (1994), entitled *The Evolution of Theodosius Dobzhansky: Essays on His Life and Thought in Russia and America* (Princeton: Princeton University Press). It includes contributions by historians and philosophers of science, including Mark B. Adams, Garland Allen, John Beatty, Richard M. Burian, Robert E. Kohler, Diane Paul, William B. Provine, and Michael Ruse, as well as by former students and associates, including Costas B. Krimbas, Charles E. Taylor, and Bruce Wallace (but not, unfortunately, Richard C. Lewontin).



seems we must accept Brandon's (1978) conclusion and hold that the core of the adaptationist program is unfalsifiable. His reasoning for this is that we need (D) to conduct research in evolutionary theory—presupposing it *makes research possible* (p. 204). With (D), the adaptationist program is a progressive research program in the Lakatosian sense. It predicts novel facts, and these facts are confirmed over and over again. As Dobzhansky (1973) wrote: “Nothing in biology makes sense except in the light of evolution” (see, e.g., Burian 2005, ch. 6).

### I. *Dobzhansky and the Classical/Balance Controversy*

To illustrate further the problems with adaptationism in evolutionary research and how the technological infrastructure can help to make sense of it in historical analyses, I consider Dobzhansky's role in the scientific debates over the issue of genetic variability in natural populations that took on public policy significance in the 1950s and 1960s. Dobzhansky was one of the principal architects, along with the systematist Ernst Mayr, the paleontologist George Gaylord Simpson, the morphologist Bernard Rensch, and the plant geneticist G. Ledyard Stebbins, Jr., of the modern evolutionary synthesis.<sup>161</sup> According to Richard Lewontin (1995b), who was also a graduate student under Dobzhansky, we should not view Dobzhansky as mainly an *experimental* population geneticist (as he believes is normally done), but instead as a theoretician. The grand theory that Dobzhansky adhered to was adaptationism, yet his view on it changed during his career. His commitment to natural selection as the most important evolutionary mechanism was rooted in Wright's (1931) analytic theory of evolution, a formulation Lewontin considers “untestable” (p. 92). However, it was Dobzhansky's unfamiliarity with mathematical and statistical formalism that causes Lewontin to characterize him as a “theoretician without tools.” Dobzhansky's aim was to “demonstrate selection in action, not to elucidate its mechanisms” (p. 90).

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<sup>161</sup> The seminal texts are Dobzhansky (1937); Mayr (1942), *Systematics and the Origin of Species*, New York: Columbia University Press; Simpson (1944), *Tempo and Mode in Evolution*, New York: Columbia University Press; Rensch (1947), *Neuere Probleme der Abstammungslehre*, Stuttgart: Enke (appeared in English as *Evolution above the Species Level*, New York: Columbia University Press, 1960); and Stebbins (1950), *Variation and Evolution in Plants*, New York: Columbia University Press. According to Grene and Depew (2004), Rensch's text “was translated into English in 1960 on Dobzhansky's recommendation. . .” (p. 257).

Dobzhansky was clearly committed to the metanarrative of the adaptationist program, so much so that Lewontin presents his adherence to it as akin to dogmatic acceptance (e.g., Lewontin 1974). Lewontin implies that Dobzhansky's adherence to the adaptationist metanarrative must be explained on the basis of *nonevidential* reasons—nonscientific reasons (Lewontin 1995b, p. 99; 1974, chs. 2, 5).<sup>162</sup>

Dobzhansky adhered to the balance model of natural selection (see Chapter I above), according to which selection preserves and perhaps even increases the heritable variation within populations. Hence, Dobzhansky considered most genetic loci in populations to be heterozygous. Genetic variation and adaptations are both explained in terms of natural selection:

The balance school sees the maintenance of variation within populations and adaptive evolution as manifestations of the same selective forces, and therefore it regards adaptive evolution as immanent in the population variation at all times. Because the alleles that are segregating in a population are maintained in equilibrium by natural selection, they are the very alleles that will form the basis of adaptive phyletic change or speciation. (Lewontin 1974, p. 196)

By 1951, Dobzhansky had formulated a theoretical account of the balance model, called *coadaptation*. On this theory, natural selection preserves heterozygosity because of overdominance, the higher fitness of a heterozygous pair of alleles over either of its homozygous counterparts. As his student Bruce Wallace (1994) put it, “events at one gene locus affect those occurring subsequently at other loci” (p. 161). But Dobzhansky did not take the superior fitness of heterozygotes to be an *intrinsic* property of heterozygosity itself; at this time he believed overdominance to be a contingent consequence of natural selection (Lewontin 1995b, pp. 94-7).

By 1953, however, Dobzhansky had abandoned coadaptation for *heterosis*, the

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<sup>162</sup> It does not seem controversial to argue that Dobzhansky's theoretical perspectives were underdetermined by the available data on population genetics. In addition to Lewontin's views presented above, Beatty (1994) reinforces this point in his chapter on Dobzhansky's concerns regarding the moral and political implications of his work (p. 214). According to Beatty (1994), Dobzhansky was concerned, among other things, that his scientific views were consistent with a Democratic worldview, and that the paradox in population genetics concerning what is good for individuals, versus what is good for a population as a whole, resulted in palatable, consistent, and morally acceptable viewpoint.

*intrinsic* superiority of heterozygotes.<sup>163</sup> Experiments by Dobzhansky's students indicated that heterosis was at work in all fitness components measured (Lewontin 1995b, pp. 97-8).

According to Lewontin (1995b):

From 1955 onward . . . the theory of coadaptation disappears from the central dogma of the Dobzhansky school. In its place is an equally assured and unquestioned adherence to a belief in the intrinsic superiority of heterozygotes, and a long series of demonstration experiments in support of this view.<sup>164</sup> (pp. 97-8)

Lewontin's analysis of Dobzhansky's work is caustic and unflattering. Indeed, Lewontin claims that "Dobzhansky knew at all times what the experiments were intended to demonstrate and what the conclusion was from the results" (p. 93). In fact, "these conclusions were already in existence *before* the experiments were done" (*ibid.*).

Lewontin's analysis of Dobzhansky's work fits in well with the present assessment of the adaptationist program. The central issues here are the unfalsifiability and testability of the core of the research program—in effect, the research program itself. Adaptationism, and in Dobzhansky's case heterosis, is the grand metanarrative imposed on the data and theory to unify and to attain explanatory power (cf. Lewontin 1995b, pp. 98-9). At stake are the issues of testability and prospects for future research. With heterosis, Dobzhansky (and Wallace)

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<sup>163</sup> The concept of heterosis, or hybrid vigor, developed out of plant breeding. See, for example, John W. Gowen, ed. (1952), *Heterosis: A Record of Researches Directed Toward Explaining and Utilizing the Vigor of Hybrids*, Ames: Iowa State College Press. Dobzhansky (ch. 13) and Crow (ch. 18) both wrote chapters for this volume. The term was coined by the corn hybridizer George Harrison Shull in 1914 (Shull, "Beginnings of the Heterosis Concept," *ibid.*, ch. 2, p. 44). Shull defined heterosis as "the interpretation of increased vigor, size, fruitfulness, speed of development, resistance to disease and to insect pests, or to climatic rigors of any kind, manifested by crossbred organisms as compared with corresponding inbreds, as the specific results of unlikeness in the constitutions of the uniting parental gametes" (*ibid.*, p. 48).

<sup>164</sup> Wallace (1994) disagrees with Lewontin's claim here, and argues that coadaptation remained a part of the Dobzhansky school after 1955. He believes Jack King, himself, as well as Dobzhansky, retained coadaptation as an interpretation of some of their work. Wallace (1994) states that Dobzhansky eventually developed "a recognition that the superiority of inversion heterozygotes was itself dependent upon the genetic milieu of the population" (p. 161). However, he cites only Dobzhansky's paper from 1950 ("Genetics of Natural Populations. XIX. Origin of Heterosis Through Natural Selection in Populations of *Drosophila pseudoobscura*," *Genetics* 35: 288-302). Indeed, it is possible to have coadaptation of genes with or without heterosis as an interpretation of the genetic structure of populations; they are not necessarily mutually exclusive interpretations. Wallace (2000) later reinforces this position (pp. 44-7), yet states: "Unlike Dobzhansky (apparently), I saw no obvious conflict between the coadaptation of genes within populations and the heterosis of  $F_1$  interpopulational hybrids" (p. 46). However, Wallace then cites a 1968 paper by Dobzhansky, which "clearly refers to coadaptation in its most general sense," and suggests the debate "can be terminated" on the basis of this citation (p. 47). In any event, the historical evidence shows that heterosis engendered much controversy among geneticists *and* nongeneticists, in the contexts of the classical/balance controversy and of the dangers of low-level radiation (see Chapters VII and VIII below).

had a mechanism that promised future experimental inquiries, inquiries that would provide demonstrations and some testable predictions, at least in practice. Acceptance of heterosis, I argue, guaranteed future testable predictions from the viewpoint of the adaptationist metanarrative; it made the *practice* of doing Darwinian evolutionary genetics possible. However, what was *not* decidable in practice, at least in the 1950s and 1960s, was how to devise adequate tests that could discriminate successfully among the competing theories of the genetic structure of natural populations.<sup>165</sup> It seems, we should conclude from the historical evidence, that heterosis, coadaptation, or whatever theory that was utilized in practice to illustrate natural selection in action, had to be *assumed* to hold in order for the particular tests to be performed. As Brandon (1978) concludes, without adaptationism “there is no theory of evolution, there are only low level theories about the evolution of certain organisms in certain environments. . . . With [adaptationism] Darwinian theory is possible” (p. 204). The story of Dobzhansky and population genetics suggests we can add: with adaptationism and heterosis, Darwinian *practice* is possible.

## II. *The Classical/Balance Controversy*

In order to probe adequately the issues regarding Dobzhansky and adaptationism, it is instructive to delve more deeply into the classical/balance controversy, a controversy over how to model genetic variation in populations and the role of natural selection. This intra-scientific controversy emerged in the 1950s and coincided historically with the advent of the atmospheric testing of hydrogen bombs and increasing concerns about nuclear fallout. By the early 1950s, two predominant positions had emerged in theoretical population genetics. According to the classical view, whose main proponent was the influential Nobel Prize-winning geneticist Hermann J. Muller (1890-1967), most genetic loci (genes) in natural populations are homozygous for “wild-type” genes. That is, identical alleles (the gene components from each parent) exist at these loci, and the particular alleles in the homozygous condition may be taken to be the “normal” and even “optimal” condition in terms of fitness (a measure of the organism’s ability to survive in a particular selective

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<sup>165</sup> On the critical problem in evolutionary biology of the relationship (or lack thereof) between theory and experiment, see Brandon (1994) and Burian (1994a).

environment). For Muller, what this boiled down to was the position that favorable mutations are rare in natural populations, and that evolution operates to eliminate deleterious mutations from the population. Hence, natural selection would have to wait for the occasional favorable variation, as most mutations would be bad. In terms of the genetic effects of radiation from nuclear fallout, the implication is clear: any increase in the mutation rate would be harmful to future generations. Hence, any amount of increased radiation exposure would be expected to increase the “genetic load” of deleterious mutations in the population resulting in future generations with an increased number of individuals with varying amounts of genetic damage. Muller (1950) set forth his position in detail in his paper “Our Load of Mutations.” Here Muller originated the “genetic load” concept<sup>166</sup> and gave his detailed theoretical arguments against increased exposure to radiation based on that theoretical position. Muller, who won the 1946 Nobel Prize for his work on inheritable mutations in *Drosophila melanogaster* (Muller 1927), could not have presented his arguments in a more emotionally charged social and political context: the Soviet Union had exploded its first atomic bomb in 1949, Truman ordered the crash-program on the hydrogen bomb in 1950, the Korean War began, and the era of programmatic atmospheric atomic testing was to begin in 1951.

Again, the main adherent of the balance view was Dobzhansky, who outlined and labeled the opposing positions the “classical” and “balance” positions at the 1955 Cold Spring Harbor Symposium, an international gathering of population geneticists (Dobzhansky 1955). According to the view that Dobzhansky held by that time, most genetic loci in natural populations are heterozygous. That is, the genes have different allelic contributions from each parent and it is incoherent to speak of the “optimal allele” at a particular locus, since changing environments and selective pressures may redefine “normal.” Such variation in natural populations provides the raw material upon which natural selection can operate. Dobzhansky believed that heterozygotes were better protected against fluctuations in

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<sup>166</sup> Wallace (1970) credits Muller with originating the term ‘genetic load’ (p. vii). Muller first used the term in his Presidential Address before the American Society of Human Genetics in New York City on 29 December 1949. Muller’s (1950) paper, “Our Load of Mutations,” is based on that address. However, Wallace (1991) suggests the concept of genetic load goes as far back as Haldane’s (1937) paper, “The Effect of Variation on Fitness” (pp. 1-2).

environmental conditions than homozygotes; therefore, in changing environments they were more likely than homozygotes to have increased fitness (Lewontin 1987, pp. 344-5). The implication for the genetic effects of radiation is again clear. If, in the extreme interpretation of the balance position, most new mutations are of the balanced type, one could predict that an increase in heterozygosity caused by mutations resulting from increased exposure to radiation might lead to increased fitness of the population as a whole, even if most individuals would not be better off. Experiments that seemed to confirm this were performed by several of Dobzhansky's students and former students, including Bruce Wallace. These results could be interpreted to suggest that a modest increase in radiation exposure might actually be beneficial to future generations, depending on how one were to measure and interpret fitness. Obviously, such an interpretation differed radically from that of the classical view, according to which any new mutations would be harmful to a population. In terms of justifying increased radiation exposure in the atomic age, the balance view seemed to offer positive support.

Beginning in the mid-1950s, the United States Atomic Energy Commission (AEC) used this interpretation to deflect increasing public opposition to atomic testing (cf. Beatty 1987, pp. 301-11; Seltzer 1993). This is discussed in more detail in Chapters VII and VIII below. What is significant is that AEC officials publicly offered the argument that low levels of radiation might be genetically beneficial to the human race. This reassurance was offered at the height of the fallout controversy when the candor of the AEC, the government agency responsible for ensuring the public's safety from weapons testing, was in question. Indeed, after the experiments of Bruce Wallace (e.g., Wallace and King 1951; Wallace 1956, 1958, 1959, 1962, 1963; Wallace and Dobzhansky 1962), Mikhail Vetukhiv (1953), and others, such arguments had at least indirect evidence from experiments on *Drosophila*, and they became standard components in the tactics of the AEC. Moreover, similar arguments have been offered since that time, for example, when the safety of nuclear power plants is questioned:

There is a lot of evidence that low radiation doses not only don't cause harm but may in fact do some good. After all, humankind evolved in a world of natural low-level radiation. (quoted in Cobb 1989, p. 411)

This statement, made by an MIT nuclear engineer, ignores, or presupposes mistaken answers to, many of the foundational questions of evolutionary genetics, including the issue of individual versus population fitness. A fundamental question remains, however. While today we may be justified in concluding that such a statement goes well beyond the available evidence and amounts to misleading rhetoric, was such a statement reasonable in the mid-1950s? As a basis for policy formulation or justification, was the AEC warranted in making such an inference based on the data then available? These questions are complex and require historical as well as epistemological arguments in order for them to be adequately answered. In searching for such answers, the inextricability of the epistemic and the political is illuminated, and the importance of using the technological infrastructure is illustrated (see Chapters VII and VIII below).

### **Interpreting the Classical/Balance Controversy**

Only a handful of works are available that deal directly with the classical/balance controversy (Beatty 1987, Crow 1987, Dietrich 1994, Lewontin 1974, 1981, 1987, Seltzer 1993). Crow and Lewontin are trained geneticists; not surprisingly, they focus mainly on the scientific developments in population genetics and on the underlying conceptual problems. A few of these works (Beatty 1987, Crow 1987, Lewontin 1987) attempt to relate the classical/balance controversy to the wider social and political context. However, while these attempts are useful in this regard, they do not situate the controversy within some of the broader historical contexts. How the classical/balance controversy became intertwined with the controversies over low-level radiation hazards, with developments in policy concerning exposure guidelines, and with official government attempts to defend publicly its policies, are questions that have not been fully explored. Only when such a wider perspective is gained are attempts to answer the question—whether the AEC’s use of the scientific data was warranted on scientific grounds—situated in their proper contexts, contexts that consider more fully the range of issues revealed by the historical record. With that perspective, one can evaluate the significance of this episode for broader concerns, such as policy formation,

studies of scientific controversies, historiography, and the nature of scientific change. What emerges is a story in which the practice of population genetics should be seen as inextricable, or “intratwined” as Rouse (2002a) would say, with other practices, including the setting of radiation exposure guidelines based on genetic dangers, and the constant concern for how to maintain one’s research program in terms of future experimentation, funding, and cooperation with colleagues (see Chapters VII and VIII below).

### I. *Epistemological Problems of the Classical/Balance Controversy*

To explore the historical issues connected to the classical/balance controversy, the technological infrastructure specifies that the epistemological issues involved be thoroughly analyzed. Lewontin’s (1974) *The Genetic Basis of Evolutionary Change* provides the best analysis of the philosophical problems underlying the classical/balance controversy.<sup>167</sup> Here he outlines what is necessary for evolutionary theory to give an adequate account of the evolution of some specified system (ch. 1). What is needed, to start with, is an account of *variation* within and between species, and how this is determined genetically. Such an account requires an adequate description of the state of the system under investigation. His criterion of adequacy has two components. First, a *dynamically sufficient description* of the state must be given; this ensures that one will be able to find the laws of transformation

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<sup>167</sup> Many recent works by Lewontin (e.g., 1991, 1995a, [1998] 2000, 2000, 2001, 2002) probe the epistemological problems of population genetics and evolutionary biology. However, Lewontin’s (1974) *The Genetic Basis of Evolutionary Change*—which he dedicated to his mentor Dobzhansky and, like Dobzhansky’s (1937) classic, *Genetics and the Origin of Species*, grew out of the Jessup Lectures given at Columbia (Dobzhansky in 1936, Lewontin in 1969)—still stands as the classic on the epistemological problems of population genetics. Lewontin’s (1997a) assessment of the legacy of Dobzhansky’s classic is, however, sharply critical: “While so much evidence was placed on the importance of intrapopulational genetic variation in the first edition, the actual evidence was pretty thin” (p. 352). In addition: “Population genetics, then as now, is an observational and statistical science, not an experimental one. As a consequence, while it can offer statistical evidence supporting the past action of one or another of the evolutionary forces having operated, it cannot cash these inferences out in the form of actual biological mechanisms” (p. 354). Similarly: “An irony of the intellectual history of *Genetics and the Origin of Species* is that Dobzhansky came into evolutionary genetics from the study of morphological diversity in nature and so was able to relate the abstractions of genetic theory to the biology of organisms, yet in the end he and the field he founded became captives of the abstractions. Despite 40 years of the study of the chromosomal variation in natural populations of *Drosophila*, Dobzhansky published no observations from nature on the possible biological mediation of the natural selection he had detected. There is, in the entire corpus of 43 papers on the *Genetics of Natural Populations*, no paper on the ecology and life history of *D. pseudoobscura*. The closest he came was to measure the rate of movement of genetically marked, laboratory-raised flies along a trap line of attractive banana bait” (p. 354).



necessary to specify the future states of the system (pp. 6-7). Second, the state description must be *empirically sufficient*; that is, the state variables or parameters involved must be measurable in principle and in practice. Problems with measurement generally include matters of precision. Low precision in the measurement of a state variable will result in comparatively high error for the corresponding predicted values of the variable at future times (pp. 9-11).

However, it may be that precision is not the problem. Perhaps the very measurement cannot in principle be made. According to Lewontin (1974), the measurement of the *fitness* of genotypes is one such variable. One component of this variable is

the probability of survival from conception to the age of reproduction. But by definition the probability of a survival is an ensemble property, not the property of a single individual, who either will or will not survive. To measure this probability we then need to produce an ensemble of individuals, all of the same genotype. But if we are concerned with the alternative genotypes at a single locus, we need to randomize the rest of the genome. In sexually reproducing organisms, there is no way known to produce an ensemble of individuals that are all identical with respect to a single locus but randomized over other loci. Thus a theory of evolution that depends on the characterization of fitness of genotypes with respect to single loci is in serious trouble, trouble that cannot be cleared up by a quantitative improvement in the accuracy of measurement. The theory suffers from an epistemological paradox. (p. 10)

Regarding gene frequencies, Lewontin (1974) gives the requirements he believes any “technique that is to enumerate genotypes in populations must satisfy. . .” (p. 96). The technique must be able to measure gene frequencies in natural populations. His requirements are:

1. Phenotypic differences caused by the substitution of one allele for another at a single locus must be detectable as an unambiguous difference between *individuals*.
2. Allelic substitutions at one locus must be distinguishable in their effects from allelic substitutions at other loci.
3. All, or a very large fraction of, allelic substitutions at a locus must be detectable and distinguishable from each other, irrespective of the intensity or range of their physiological effects.
4. The loci that are amenable to attack must be a random sample of genes with respect to physiological effects and with respect to the amount of genetic variation that exists at the locus. (p. 96)

Requirements 1 and 2 seem to be satisfied by the technique of gel electrophoresis, first used in the mid-1960s by Lewontin and Hubby (Hubby and Lewontin 1966, Lewontin and Hubby 1966), and independently by Harris (1966), to investigate genetic variation.<sup>168</sup> As Lewontin (1974) states, this “method is capable of distinguishing different homozygotes not only from each other but also from heterozygotes” (p. 105). Gel electrophoresis therefore satisfies, with some qualifications, the requirement that in practice there be a “one-to-one correspondence between phenotype and genotype. . .” (p. 96). The phenotypes in this case are the amino acid sequences of proteins that are coded for by different alleles, and a “single allelic substitution is detected unambiguously since it results in a discrete change in phenotype—the substitution, deletion, or addition of an amino acid” (p. 99). This holds true for every substitution, but depends upon the assumption that loci coding for soluble proteins—those the technique of gel electrophoresis are able to detect—are representative of the entire genome. Since testing this question was not possible in practice, requirement 3 was not unequivocally met. Moreover, there was (is) the problem of “redundant code changes,” but Lewontin (1974) believed these to be “irrelevant for our problem” (p. 99). However, the problem of possibly large fractions of DNA being *neutral* with respect to natural selection arose with Motoo Kimura and his work on random drift as a possible alternative to natural selection (rather than merely a source of variability upon which selection acted), and transformed the classical/balance controversy into the neutralist/selectionist debate by the

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<sup>168</sup> Though developed some ten years earlier, the technique was first used by these researchers for this purpose. Hubby and Lewontin (1966) explain their method and the argument on which it was based: “Any mutation in a structural gene should result in the substitution, deletion, or addition of at least one amino acid in the polypeptide produced by the gene. Such a substitution, deletion, or addition will, in some fraction of the cases, result in a change in the net electrostatic charge on the polypeptide. This change will in turn change the net charge on the enzyme or other protein of which such a polypeptide is a constituent. Since enzymes and proteins are, as far as we know, made up of polypeptides from one or sometimes two different structural genes, then we can expect that electrophoretic differences in enzyme proteins will segregate as single Mendelizing genes. Thus, if we survey a large number of enzymes and other proteins and if we determine the electrophoretic mobility of such proteins from single individuals, it should be possible to detect variability from individual to individual at single loci” (p. 579). There are limitations, however, to what questions the technique can answer (see, for example, Lewontin 1991 and Barbadilla, King and Lewontin 1996). Barbadilla, King and Lewontin (1996) conclude that “neither single, nor sequential, nor high resolution protein electrophoresis characterize adequately the real level of protein polymorphism when the polymorphism is due to several segregating positions” (p. 431).

end of the 1960s.<sup>169</sup> Hence, Lewontin's requirement 4 was also not unequivocally met.

It seems, following Lewontin (1974), there were serious epistemological problems underlying the theory and practice of evolutionary genetics of the 1950s and 1960s. It simply was not possible in practice to answer certain essential questions.<sup>170</sup> Moreover, with respect to the classical/balance controversy, Lewontin devotes an entire chapter to showing how the experimental and theoretical developments in population genetics were in the 1950s (and still were in the early 1970s) inadequate in deciding between the two general theories of the classical/balance controversy (ch. 2). These developments go well beyond the period of atmospheric atomic testing and continue to the present day.<sup>171</sup>

Lewontin (1987) elsewhere puts the classical/balance controversy into an historical outline. Here he shows the progression of the views of his former mentor, Dobzhansky. Lewontin's main point, however, is to argue that the classical/balance controversy was not resolved in the mid-1960s, the earliest point at which direct experimental evidence, through gel electrophoresis, could be brought to bear on the question of whether populations are mostly homozygous for "wild-type" genes, or whether large numbers of genes are

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<sup>169</sup> The work by Kimura that initially caused controversy in the mid-1960s was published in 1968 as "Evolutionary Rate at the Molecular Level," *Nature* 217: 624-626. See also Kimura's (1983) book, Provine's (1986, pp. 463-77) analysis of Kimura's work, and the article by Michael R. Dietrich (1994), "The Origins of the Neutral Theory of Molecular Evolution," *Journal of the History of Biology* 27: 21-59. Kimura came to the U. S. from Japan in 1954 to work with James Crow at the University of Wisconsin (he had wanted to work with Wright), where he had contact with Sewall Wright and took his population genetics class. However, Kimura ultimately disagreed with Wright on the role of random drift and its importance for natural selection (Provine 1986, pp. 468-77).

<sup>170</sup> According to Lewontin (2001): "The actual operation of inference in population genetics is a form of the usual method of statistics. A concrete model of the population process is constructed involving causal processes and entities at the genetic and organismic levels that cannot be observed directly but will have predictable consequences for the intergenerational observations of genetic and organismic state variables. In constructing the model, a distinction is made between structural features that are fixed parts of the model and a variety of parameters associated with these structural features, which may vary from case to case. . . . Thus, population genetics begins as a deductive scheme that estimates parameters of causal processes from evolutionary results. This sounds like what goes on in much of science and seems harmless enough until it is realized that the inferential process is severely underdetermined" (p. 15).

<sup>171</sup> As Lewontin (2000) argues: "The problem of directly measuring fitness differences in nature is one that has plagued population genetics for nearly a century, and our ability to identify genotypes at the DNA level has not made it disappear" (p. 11). In addition: "It is [a] common assumption that constraints on amino acid variation are the consequence of requirements on the physiological function of the protein in question. But if we suppose that the DNA sequence itself is constrained over even short stretches, then the amino acid sequence will be held constant as a consequence of the constancy of the underlying nucleotide sequence. At the moment we do not know how much of the variation and the conservation of the protein sequence is a secondary consequence of requirements on the nucleotide sequence itself. Nor do we know how to find out" (p. 18).

polymorphic.<sup>172</sup> Again, the controversy became transformed into the “neutralist/selectionist” debate.<sup>173</sup> Hence, the main problems of the classical/balance controversy were not resolved by the time the controversy disappeared as an historical construct, although Lewontin and Hubby (1966) rejected widespread heterosis as an interpretation of their electrophoretic results (pp. 606-8). Lewontin (1987) argues that while it has been generally agreed that there is “a lot” of genetic polymorphism, a certain amount of genetic variation might be neutral with respect to selection. On this view, “polymorphism might be for genetic variation that has no connection with selection, because the genetic differences would not be reflected in differences in physiology and morphology” (p. 338). Therefore, even if it is agreed that the problem of variation has been adequately resolved, how that bears on the problem of deciding upon the correct mechanism of evolution, at least in particular cases of adaptation, remains an open question.

Again, Lewontin (1987) has put his finger directly on the relevant epistemological problems underlying his discipline. One further problem, though, involves evaluating the narrative context of the atomic testing controversy in order to determine what principles and positions were explicitly or implicitly entertained by the important players at particular times. Lewontin shows, interestingly enough, that Dobzhansky held a “neutralist” view for chromosomal inversions up until about 1943. Before that time, he also held the position that genic variation “was deleterious and was maintained in natural populations only because it

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<sup>172</sup> A polymorphic population is one with two or more recognizably different types of individuals within one interbreeding group, which occur in frequencies that cannot be accounted for by recurrent mutation (Jepsen, Simpson, and Mayr 1963, p. 458).

<sup>173</sup> Dobzhansky wrote to Bruce Wallace in 1964: “To me, the last Crow-Kimura [Kimura and Crow 1964] opus is the most weighty argument in favor of the “Party Line” they have produced. The question of how much genetic load an animal can stand arose, you may recall, in Cold Spring Harbor in 1955 [see Dobzhansky 1955], and for some years I kept urging Howard Levene to investigate this issue. . . . All I can say is that unless Crow-Kimura have slipped in some hidden assumption, as they often did before, they have proven their point. The gene number in *Drosophila* is 10,000 rather than 5,000. What we still do not understand is this: do we need [to] suppose that any selection always means mortality? Could it be that the really ‘optimal genotype’ could make a woman produce 100 children, by making the pregnancy term 1 month and making all the children survive? Crow himself has shown that ‘opportunity for selection’ exists with no mortality at all, merely with parents producing different numbers of children. Is it possible that the really existing population with all its balanced loads has, if well adapted, the reproductive potential which we observe in it? What about the alleles  $A_1$  and  $A_2$  each being slightly advantageous and slightly disadvantageous at different seasons, or at different stages of the life cycle, or in different ecological niches which the population exploits? These are the lines along which my thoughts move, but you know I am not enough of a mathematician to be able to test the validity of these thoughts” (Dobzhansky to Wallace, 27 April 1964, Bruce Wallace Papers, APS).

was recessive and therefore protected in heterozygous condition. . .” (p. 341). As Lewontin points out, this position is close to what Dobzhansky (1955) later dubbed the “classical” position.

Lewontin (1987) recalls an experiment Dobzhansky often mentioned while Lewontin was his student from 1951 to 1954 (p. 341). This experiment (Dobzhansky and Spassky 1947), published in the first volume of the journal *Evolution*, seemed to confirm the very antithesis of the balance view. Dobzhansky and Spassky (1947) used strains of *Drosophila pseudoobscura*, which were homozygous for certain chromosomes carrying deleterious gene complexes. They raised 50 generations of the flies, some in homozygous condition, some in the “balanced,” or heterozygous condition. Convinced that the “genetic diversity which arose in [the] chromosomes during the fifty generations of the experiments must have appeared by mutations,” (p. 213) and not by recombination, the authors found that most of the homozygous strains had increased viability while most of the balanced lines degenerated. They offered the following interpretation:

Balanced strains accumulate deleterious mutations because recessive mutations, that arise in balanced chromosomes, are sheltered from natural selection, regardless of how deleterious they may be in double dose [homozygous condition]. In homozygous strains, deleterious mutants are eliminated while beneficial mutants are multiplied, eventually supplanting the original genotypes. (pp. 208-9)

So by early 1947, Dobzhansky had not yet adopted the view that heterosis was common in natural populations.<sup>174</sup> Indeed, Dobzhansky at that time proposed a limited role for natural selection, one that “sheltered” balanced chromosomes from its effects and “eliminated” harmful mutations from homozygous populations. Furthermore, Dobzhansky believed that “beneficial mutations are much less frequent than the deleterious ones” (Dobzhansky and Spassky 1947, p. 215). These views appear to be not so far from Muller’s classical position. It is interesting to note, however, Dobzhansky’s view on the ability of

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<sup>174</sup> Dobzhansky and Spassky (1947) refused to accept heterosis as an interpretation for their experiments. To explain the “appearance of recessive lethals in the balanced chromosomes which did not contain such lethals at the start of the experiments,” (pp. 213-14) the authors rejected natural selection as the cause. They argued: “Unless one supposes that individuals heterozygous for recessive lethals are favored by natural selection and replace the lethal-free genotypes in the populations of the balanced flies, the spread of the lethals can be ascribed only to chance” (p. 214).

natural selection to alter the effects of irradiation on heterozygous populations. He believed that,

with unrelenting selection, living populations may not merely purge themselves of deleterious mutants induced by X-radiations, but may undergo progressive improvements. (p. 214)

This line of thinking, involving selecting flies that became adapted to an environment of high levels of radiation, became a research program for one of Dobzhansky's students, Bruce Wallace (see Chapters VII and VIII below).

Later in to the 1940s and early 1950s, Dobzhansky became increasingly convinced that natural selection acted on inversions with heterozygotes to make them more fit than homozygotes; that is, *coadaptation*, or the superior fitness of hybrids, was at work. Again, he did not yet claim that the superior fitness of heterozygotes held in general. This view—*heterosis*—came only after experiments he designed for one of his students, Mikhail Vetukhiv, showed that *overdominance*, the superior fitness of a heterozygous locus in comparison with each of its homozygous counterparts, apparently must hold in general (Vetukhiv 1953). Other experiments also seemed to confirm this (Lewontin 1987, pp. 343-4). So by 1953 or 1954, Dobzhansky was fully committed to what Lewontin takes to be the most extreme version of the balance theory.

Lewontin (1987) believes that Dobzhansky's conversion to the view that balancing selection was equivalent to heterosis was underdetermined by the experimental evidence (pp. 344-5). As evidence for this, Lewontin cites problems with the design of the Vetukhiv experiments and Dobzhansky's dismissal of the results of other conflicting experiments. Furthermore, he implies that other interpretations of the balance view were not given adequate consideration. Lewontin believes that Dobzhansky may have become convinced that heterosis explained balancing selection after the publication of *Genetic Homeostasis* by his friend Michael Lerner (Lewontin 1995b, pp. 98-9). In this work, Lerner (1954) argues that heterozygotes would be more fit than homozygotes in a changing environment, because they would be better protected against environmental fluctuations. That is, they would have an intrinsically beneficial phenotypic plasticity.

Therefore, by the time of the onset of the international fallout controversy in 1954,

Dobzhansky was fully committed to the balance view of selection general, and heterosis in particular. It was almost as if events in population genetics and concerns over nuclear fallout were conspiring toward confrontation and controversy. Yet, more explanation regarding Dobzhansky's conversion to heterosis is needed beyond merely showing that other experiments pointed to alternative interpretations of the experimental data involving genetic variation and natural selection. Lewontin's conjecture that Lerner's influence may have played a role is instructive. Dobzhansky himself (1962, p. 296) supports this view when he cites Lerner as a proponent of the balance theory, along with himself (1955) and Bruce Wallace (Wallace and Dobzhansky 1959).<sup>175</sup> Moreover, other historical evidence is available to shed light on these questions, and on broader historical questions.

## II. *Historiographical Problems of the Classical/Balance Controversy*

In this section, I review previous attempts at historically interpreting the classical/balance controversy. This historiographical analysis provides part of the foundation for the historical analyses in Chapters VII and VIII, in which I utilize the technological infrastructure construct to interpret the classical/balance controversy and its significance for broader cultural concerns, including the genetic effects of radiation. To show how the technological infrastructure construct differs historiographically from other accounts of this controversy, it is instructive first to explore how others have interpreted it, including the geneticists Richard Lewontin and James Crow, and the historians John Beatty and Diane Paul.

As indicated in the previous section, Lewontin generally presents a harshly critical view of Dobzhansky's work, and he believes there were significant epistemological problems with the science of population genetics in the 1950s and 1960s, many of which Dobzhansky

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<sup>175</sup> In his textbook on adaptation, coauthored with Cornell colleague Adrian Srb, Wallace and Srb (1961) present an argument for adaptationism that favors homeostasis (or "phenotypic flexibility," p. 93), heterosis, and coadaptation, even if these terms are never mentioned (see, for example, pp. 2, 8-9, 30-3, 35-44, 60, 89-95, 100-105). They cite Lerner (pp. 102-3) and explain his findings as supporting homeostasis and the coadaptation of genes (p. 104). They state (and admit as an assumption) that "[p]hysiological mechanisms for meeting environmental stresses can usually be made . . . ; just what these mechanisms are in specific instances is relatively unimportant." They conclude that the "successful solution to the evolutionary aspect of individual adaptation comes about when virtually all individuals of a population possess the ability to make these adaptations when the need arises." (p. 104)

did not grasp. For example, Lewontin (1981) concludes that the classical/balance controversy was the result of the “faulty” theories of Muller and Dobzhansky (p. 115). Muller’s (1950) work, for Lewontin, was “by its nature, unable to cope with the realities of natural populations.” Conversely, Dobzhansky’s work and that of his collaborators, for Lewontin

was unable to refute the theoretical arguments on which the load theory was built. The experiments were largely incapable of resolving the problems to which they were addressed. The lesson, it seems, is that a faulty theory, coupled to inadequate experimental tests, can lead only to controversy. (p. 115)

Lewontin (1981) concludes with this assessment of Dobzhansky’s efforts:

Dobzhansky worked throughout his career at the limits of resolution of the techniques available to him. Anyone who does so risks describing things and events that others cannot see clearly; only time and the development of still better techniques will resolve the dispute over the genetics of natural populations. (p. 115)

In this caustic assessment of Dobzhansky’s work, Lewontin (1981) is more revealing concerning broader historiographical concerns that go beyond the epistemic status of the data on population genetics. Indeed, regarding Dobzhansky’s eventual commitment to generalized overdominance, he states that Dobzhansky’s conversion “did not come easily,” and that he initially found the 1953 Vetukhiv results supporting heterosis difficult to accept. Lewontin quotes Dobzhansky as saying, as late as 1955, that instead of accepting the results of Wallace and King (1951; which also supported overdominance and heterosis), that he “would rather believe in Muller” (p. 103). However, without further explanation, this is not sufficient in accounting for Dobzhansky’s transformation to heterosis and for assessing answers to questions of wider historical significance. Again, further historiographical considerations, beyond the epistemological analysis of the scientific data, are necessary, and these should be specified explicitly.<sup>176</sup>

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<sup>176</sup> Burian (1994b) provides illuminating information on Dobzhansky’s changing views on natural selection in the 1940s and 1950s. Burian argues that Dobzhansky’s growing belief in the power of selection and in its operation at all levels (and not only at the species level) must be “interpreted against the background of earlier disputes in Russia as well as the more familiar ones in the United States and England, and that it marks a major departure from the Russian biological traditions within which Dobzhansky was raised” (p. 130). Burian (2005, ch. 6) also argues that Dobzhansky’s commitment to evolution as the central framework for all of



Other attempts at placing the classical/balance controversy in a broader historical context have been more successful. James Crow (1987) is as equally puzzled as Lewontin about Dobzhansky's switch to the view that overdominance is prevalent in natural populations. Crow (e.g., 1948, 1964) had held a theoretical position closer to the classical view of Muller, and he

could not understand the need to postulate that most loci were overdominant. [A] minority of such loci could still exert the major influence on population fitness, and to postulate that 50-100 percent of loci were overdominant seemed unnecessary. Furthermore, I thought that this would cause an excessive genetic load. (Crow 1987, p. 359)

Crow (1987) continues at great length to criticize Dobzhansky on this point, arguing that Dobzhansky "consistently overinterpreted his data" (pp. 372-3). Crow bases his charge on the fact that he and others could not replicate Dobzhansky's results in the 1950s and 1960s. He even goes as far as to say that subsequent experimental results have been closer to the classical position than to the balance position (pp. 371-9). He unequivocally states that

we can rule out the possibility that the bulk of the variation [in natural populations] is maintained by overdominance. The data are most consistent with a mixture of neutral and deleterious alleles, with perhaps a small contribution from balancing selection. (p. 379)

So far Crow (1987) seems to present his analysis as definitive. However, he does not explicitly use his later personal reflections to explain the historical context. Many of his comments are derived from his thinking at the time of the height of the controversy. Moreover, Crow attempts to place the classical/balance controversy into the wider context of the debates over radiation hazards, the debates over the establishment of radiation exposure guidelines, and the contemporary concerns over eugenics of Dobzhansky and especially of Muller (pp. 370-1, 379-80). Unfortunately, other than mentioning these contexts, Crow offers little in the way of historical interpretation. This does raise, however, some important questions. Crow and Lewontin agree that some decisive transformations in theoretical interpretations of experimental data in population genetics were underdetermined by the

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biology did not change, in addition to "his constant efforts to ensure an institutional footing (and funding) for evolutionary studies" (p. 112).

available data. Given this agreement, what then are the important factors contributing to these developments? Other than stating that such developments took place within complex political and social contexts, what can be said about the influences or possible influences among these factors?

Crow (1987) maintains that the U. S. National Academy of Sciences Committee on the Biological Effects of Atomic Radiation (BEAR I 1956, BEAR II 1960), on which both Dobzhansky and Muller served (on the Genetics Subcommittee), “had a large influence in determining radiation protection policy and setting radiation exposure limits” (p. 370). Given this, and given Muller’s crusade for radiation safety, the underlying interests in human evolution of both Dobzhansky and Muller, their left-wing leanings, and Muller’s interest in a program of eugenics, can anything positive be said about these “external” factors? The reluctance to attempt explicit answers to such questions is a common characteristic of the actors involved in the controversy, and of historians as well. It is as if these writers wish to acknowledge the wider historical context, yet they do not wish to, or cannot, go further in their interpretation. This stopping-short probably results from their motivation to answer questions relating to the narrower context of a “scientific controversy.” One needs further historiographical criteria, beyond interpretations of experimental data, not to mention an enriched historical context. These criteria must go beyond the scientific or epistemological status of the experimental data. Identifying such criteria is not necessarily a simple or straightforward matter. While many of the chapters in the Adams (1994) volume mark a step in the right direction in providing contextual historical information on Dobzhansky’s personal, professional, religious, and political viewpoints, none explicitly addresses the question of his acceptance of heterosis, how that transformation fits into his overall worldview, and its possible connections to other historical questions.

Perhaps the fullest contextual account of the classical/balance controversy is John Beatty’s (1987). After his interpretation of the technical issues involving the experimental evidence, Beatty devotes considerable effort to analyzing the policy considerations inherent in the controversy. His opening remarks in this connection are revealing:

Both Dobzhansky and Muller took advantage of the ambiguities in the issues and in the evidence to hold to their respective positions in the face of each other’s best

findings. Their intransigence is at least in part explained by their shared belief that the scientific issues had strong implications for public policy. They both feared that a mistakenly premature resolution to their controversy might have dangerous social consequences. Thus they acted to forestall a resolution by demanding extremely high empirical standards for assessing each other's work. (p. 301)

This type of historical analysis largely absent from the other accounts here considered. Beatty claims that certain social implications of the scientific controversy were instrumental in preventing a resolution of the controversy. He considers the important factors to be Dobzhansky and Muller's deep-seated interest in human evolution, the connection of such interests to eugenic concerns, and the concerns in the 1950s over radiation safety (pp. 301-3). Moreover, he seems to argue that the "social policy" considerations had a role in the ongoing scientific practices of population genetics.

Beatty (1987) is convincing in showing that such "social policy" issues motivated Dobzhansky and Muller, and he provides evidence to support his contention that these issues played a role in continuing or maintaining the classical/balance controversy (pp. 303-318). In what sense, however, should we take this claim? On the one hand, Beatty does not dismiss the importance of the perceived empirical issues in the classical/balance controversy (p. 318). On the other hand, he maintains that "there was more to the classical/balance stalemate than just the empirical underdetermination of the theoretical issues. . ." (p. 318). Do we take this to mean that nonevidential issues were the controlling factor? While Beatty is not clear on this point, he does suggest, however, that this controversy was unique in that it involved "blatant" social policy considerations. As he states: "Social policy considerations no longer play a role in keeping the dispute alive" (p. 318).

In terms of the technological infrastructure construct, this claim raises red flags, for it seems to suggest that scientific practices can proceed insulated from their wider cultural contexts. In addition, such a claim suggests that analyses of scientific practice can be made with little or no examination of the practice's cultural context. While epistemic disputes are indeed central to analyzing scientific practices, treating them as epistemically sovereign violates the postmodern principles analyzed in this dissertation, and typically results in impoverished narratives of the history of science. In her article on Muller's eugenics, Diane

Paul (1987) suggests that later geneticists resisted seeing their work as having social or political implications. She believes that Muller and Dobzhansky can be “distinguished from the current generation of population geneticists in having social views linked to their science” (p. 323). Paul concludes that

for both Muller and Dobzhansky, scientific and social values were inextricably linked. The controversy about the nature of selection [the classical/balance controversy] has not disappeared, or even diminished in intensity; but no one any longer sees it as linked to the social issues that obsessed these men—or, apparently, to any social issue at all. (pp. 333-4)

Clearly, for a scientist (or historian) to adopt the notion that the scientific debates over variation and selection are, or should be, devoid of social or cultural implications, is suspect for several reasons.

First, this contention stands in contrast to Lewontin’s (1974) epistemological analysis of the ongoing controversy. If Lewontin is right, there may be reasons to suspect that the controversy is in principle irresolvable. If the conditions of dynamic and empirical sufficiency cannot be met in the experimental practice of evolutionary and molecular genetics—and they could not be met in the period under consideration, given the experimental techniques available in population genetics—then any resolution of the controversy will be by nonevidential means, by overinterpretation of data, or some combination of both. It may be that no experimental results can unequivocally decide the matter. This state of affairs, however, may be what Beatty refers to when he states that social considerations no longer act to keep the controversy alive. If so, his agreement with Lewontin presents an additional problem.

Second, the view that social policy considerations no longer play a role in the classical/balance (or neutral/selectionist) controversy creates an unusual dichotomy between the past and the present. To say that policy considerations were only important in the past, and that in the present we are exempt from them, seems suspect, even in the limited context of the classical/balance controversy. This implies that as evolutionary genetics has developed, its social or cultural implications have become increasingly irrelevant or nonexistent. While historical contexts change and the particular epistemic and cultural

factors relevant to any science may change, to conclude that such factors do not play any role (in the present) is curious. Perhaps the problem here is that with hindsight the relevant social and cultural factors appear more obvious, while at present they remain unarticulated; the norms operating might be ineffable and capable of articulation only in the future. Nevertheless, one could point to a number of possibilities that might plausibly have relevance, including issues in certain biotechnologies, such as human cloning and the privacy issues raised by the realization that the information contained in one's genome could now be commodified and possibly land in the wrong hands (see, for example, Haraway 1997, Fukuyama 2002).

Clearly, from the perspective of the technological infrastructure, with its commitments to historicity, postmodern naturalism, and Rousean causal intra-action, analyses that attempt to abstract scientific practices from their historical/cultural contexts and claim the irrelevance of non-epistemic factors, should be rejected. They should be rejected because they presume, among other things, that the epistemic and the political (or social, non-epistemic, or whatever) are or should be *separated*, in the practice of science *and* in the practice of historians interpreting the science. If one were to attempt an interpretation that did not presume the epistemic/political dichotomy, one might find, when utilizing relevant documentary sources, that an account of the *practices* of the scientists involved will reveal those practices as incapable of description using the epistemic/political dichotomy. That is, accounts of scientists' practices may express Rouse's (2002a) notions of causal intra-action and real possibility, with their commitments to the resistance of the dichotomies of nature/normativity, epistemic/ political, and material/discursive, in addition to its futural orientation. While the "social policy" considerations found may not be as overt as with the controversy in the 1950s over fallout and atomic testing, if one uses the perspective of the technological infrastructure, one might be able to find them.

Third, Lewontin (1974, ch. 3) suggests an argument for how the classical/balance debate in fact has continued to have social implications beyond the 1950s and 1960s. While his main point there is to survey the foundational problems of evolutionary genetics, Lewontin offers an explanation to account for the theoretical positions in the classical/

balance controversy. Focusing on the uncertainty involved in the controversy, Lewontin argues that the positions held can be reduced, at least in part, to particular worldviews, or ideologies. His argument is incomplete, however, because he does not support it with contextual historical information. His contention rests primarily on the epistemological point that underdetermination was at work in the classical/balance controversy.

For example, Lewontin's (1974) claim that the history of the problem of variation illustrates and confirms the role of "deeply embedded ideological assumptions . . . in determining scientific 'truth' and the direction of scientific inquiry," is not fully supported, but nevertheless may turn out to be accurate. To infer from the fact that underdetermination is at work in the classical/balance controversy, that it is "not the facts but a world-view that is at issue," (p. 157) one must provide specific justification through detailed historical investigation. That underdetermination is at work in itself does not establish that ideology is the controlling factor; it fails to demonstrate *which* ideological (or social or cultural) factors are at work, and what roles are played by the worldviews that correspond to them. Lewontin's (1974) contention that the classical and balance positions correspond to worldviews that represent

a divergence between those who, on the one hand, see the dynamical processes in populations as essentially conservative, purifying and protecting an adapted and rational *status quo* from the nonadaptive, corrupting, and irrational forces of random mutation, and those, on the other, for whom nature is process, and every existing order is unstable in the long run. . . . (p. 157)

is not established by the facts as he sets them out in chapter 3, although it is undoubtedly plausible.

It is possible that Lewontin's (1974) own political views are at work here (see, for example, Levins and Lewontin 1985). Lewontin's neo-Marxism is apparently motivating some of his conclusions, at least in part. Indeed, despite his reluctance to take a firm stance in the classical/balance debate (at least up to this point in his book), Lewontin (1974) implies that a position closer to the balance view is the correct view; organisms do have a fairly large amount of variability, although it is not explained by heterosis. In discussing his (1957) views on the temporal instability of marginal environments, Lewontin favors Carson's (1959)

view, according to which recombination is necessary in such marginal populations so that unique allele combinations are produced, combinations not present in the main, central populations. But, as Lewontin (1974) states,

it is not some particular, specialized, homozygous genotype that is being selected in the marginal environment. In the highly unstable and unpredictable environment of the margin, quite different genotypes are being selected at different times. It is not surprising, then, that genic heterozygosity is high, and remains high, because no particular genotype is favored for very long. The metaphor of the laboratory is sometimes used to describe marginal populations. They are thought of as performing genetic “experiments.” If so, these are frustrating experiments, the very opposite of Hershey’s Heaven. (pp. 151-2)

Lewontin’s (1974) reference to “Hershey’s Heaven” is explained earlier in the chapter, when he claims that A. D. Hershey “is reported to have described heaven as ‘finding an experiment that works and doing it over and over and over’” (p. 116). What is noteworthy in this is that Lewontin favors the view that the “existing order is unstable in the long run,” and this seems commensurate with his Marxist politics, his liberal social views, his anti-eugenics stance, and his view that scientific results cannot adequately explain or answer fully questions regarding human culture (see, e.g., Fracchia and Lewontin 2002). Indeed, in chapter 2, when explaining the classical position, Lewontin (1974) states that this position gives race “a considerable biological importance”:

A basis for racism may . . . flow from the concept of the wild type, since if there is a genetic type of the species, those who fail to correspond to it must be less than perfect. Platonic notions of type are likely to intrude themselves from one domain into another, and Dobzhansky (1955) was clearly conscious of this problem when he attacked the concept of wild type. “The ‘norm’ is, thus, neither a single genotype nor a single phenotype. It is not a transcendental constant standing above or beyond the multiform reality. The ‘norm’ of *Drosophila melanogaster* has as little reality as the ‘Type’ of *Homo sapiens*.” The balance hypothesis, conversely, presumes that a vast amount of hidden genetic diversity exists *within* any population, so that interpopulational differences are less significant. (pp. 25-6)

Moreover, when discussing racial variation in humans (pp. 152-7), Lewontin makes the following claim:

The taxonomic division of the human species into races places a completely disproportionate emphasis on a very small fraction of the total of human diversity. That scientists as well as nonscientists nevertheless continue to emphasize these

genetically minor differences and find new “scientific” justifications for doing so is an indication of the power of socioeconomically based ideology over the supposed objectivity of knowledge. (p. 156)

These comments, again, are commensurate with Lewontin’s Marxist politics and suggest distaste for eugenics. In upholding his (1957) interpretation of the dynamics of marginal populations, and given the evidence he surveyed in his chapter 3 that supports the view that interpopulational differences (i.e., racial differences) are a small proportion of total genetic diversity, Lewontin (1974) is vindicating the liberal, democratic worldview of his teacher, Dobzhansky, while at the same time showing how his own theoretical interpretations are superior to Dobzhansky’s. Also, he is discrediting some of Muller’s uses of the data of evolutionary and population genetics, including Muller’s eugenics, and the use of such data in justifying the dividing of humans into races (based on “obvious” differences, such as skin color or facial characteristics). So, perhaps Lewontin’s own motivations are at least in part political or ideological, and represent his desire to uphold his own liberal, Marxist worldview.

To say that Lewontin has failed to demonstrate which ideological factors are relevant is not to say that worldviews or ideological factors could not be significant factors in evaluating the classical/balance controversy. Paul’s (1987) claim that social factors no longer play any role in the debate seems as suspect as Lewontin’s. Merely because few have been identified, or because the disputants *claim* to base their theories exclusively on empirical considerations, does not establish that social or ideological factors are not at work; cultural norms are not always consciously held or discursively articulated. Lewontin’s own position is apparently motivated in part by his own worldview. Indeed, he believes that the “impact” of his 1974 book *The Genetic Basis of Evolutionary Change* “is a confirmation of the power of dialectic analysis” (Levins and Lewontin 1985, p. viii). As he and Levins state in their book,

Both separately and together we have published scores of essays, applying the dialectical method sometimes explicitly, sometimes implicitly, to scientific and political issues and to the relation of one to the other. Indeed, it is a sign of the Marxist dialectic with which we align ourselves that scientific and political questions are inextricably interconnected—dialectically related. (p. viii)



Similarly, in his analysis, Beatty (1987) employed certain historiographical criteria to interpret the classical/balance controversy. He concluded that social policy considerations played a role in developments in the scientific controversy. Specifically, Dobzhansky and Muller's concerns over human evolution, eugenics, and radiation safety served to prevent a resolution of the controversy. Nevertheless, if other historiographical criteria were chosen, a different interpretation, or at least a modified interpretation, might have resulted. For example, one could argue that developments in the classical/balance controversy had implications outside the narrower context of the technical controversy. This appears to be the case for the controversy over fallout and the genetic effects of radiation. Moreover, it is equally plausible, given historiographical criteria focusing on the epistemic foundations of genetics, to suggest that it was the scientific and epistemological status of the classical/balance controversy that precluded a resolution to the broader debates over fallout and the genetic hazards of radiation. That is, one could argue that the uncertainty involved in assessing the genetic damage produced by radiation is itself adequate to account for the ongoing controversies over fallout and atomic testing. In adopting such a view, however, one risks presenting an overly simplistic interpretation of the controversies. Nevertheless, epistemic factors are important to scientists.

It seems if one were to engage the historical data with an enlarged view of historical context and with alternative historiographical choices, a more complex picture of the classical/balance controversy may arise. Instead of employing historiographical criteria that focus primarily on either epistemological or social issues, I suggest that a perspective that takes into account both—as with the technological infrastructure of science—yet privileges neither, is superior. Such a perspective allows one to construct an historical narrative that answers pertinent questions; it keeps history telling a story—a narrative. Similarly, as this historiographical analysis of the classical/balance controversy suggests, studies of scientific controversies should not only take into account broader cultural and historical contexts, but they should also examine the relevant epistemological questions, focusing on the practice of scientists and their attempts, using technical and theoretical technologies (which are themselves cultural), to separate signal from noise.

In addition, paying more explicit attention to historiographical matters may help suggest particular answers to broad historical questions, answers that may not be apparent from the perspective of alternatives. Moreover, historiographical analysis may play a role in resolving “scientific” questions as well, such as those involving the evaluation of the epistemological status of past scientific evidence. For example, a particular historiographical perspective may suggest that certain scientific questions can be (or could have been) answered, while another perspective may lead to the opposite conclusion (Burian 1996, 1997). Historiographical and meta-historiographical analyses, consistent with the technological infrastructure construct presented in Chapters IV and V above, may thus have a critical bearing on important epistemological views of science, and on the policy questions implicit in them from the perspective of the relevant historical context.

The goal of Chapters VII and VIII is to re-evaluate the classical/balance controversy, using the technological infrastructure with its postmodern naturalism, and connect it to several overlapping narratives in which epistemic and political matters are intertwined. To evaluate the experiments in radiation genetics of Dobzhansky, Wallace, and others, I expand on the interpretation developed in this chapter, derived largely from Lewontin’s analyses, and I attempt to explore some key historical questions. That is, I explore in particular how the futural norm, the *promise for future research*, might contribute to a re-evaluation of stories involving Dobzhansky, Wallace, radiation and population genetics, and the political and policy debates in which they became intertwined. In addition, when considering the signal/noise issue, and the nature of the relationship between theory and experiment in population genetics, a common theme has been to point to the lack of the kind of data needed to answer questions posed by geneticists at the time. This seems to have been the result, at least in part, of a lack of scientific techniques that would do the job. Indeed, the underdetermination of the data produced by experimenters has been mentioned by scientists and historians alike. As Lewontin (2002) states:

Evolution is a loose and complex process, the result of a number of interacting, individually weak forces with many alternative outcomes, and at all times contingent on previous history. The best answer to any question about evolution is the lawyer’s answer to any general question about the law: “It depends on the jurisdiction.” That is why the program of evolutionary investigation *never comes to an end*—and, so

often, never to a conclusion. (p. 17, emphasis added)

How this uncertainty was used by the Atomic Energy Commission (AEC) in influencing radiation protection policy to maintain a program of atmospheric atomic testing, by the Eisenhower Administration and its Federal Radiation Council (FRC) in setting policy on allowable levels of radiation contamination from fallout, and how uncertainty in general, and (sometimes deliberate) confusion, in particular, can contribute to an understanding of not only how scientific research programs can be maintained, but also how governmental policy positions can be justified in practice. This intra-twinning of epistemic and political norms flows from the basic principles of the technological infrastructure; in addition, its primary focus on exploring the technological basis of development in scientific change reflects Pitt's core principle for studying how science (and culture) changes. In both main stories explored—Dobzhansky, Wallace, and heterosis, on the one hand, and the AEC, the FRC, and radiation protection, on the other hand—the notion that preconceived or predetermined positions were chosen, and then the data or experimental evidence adduced to justify the favored position, is explored. This strategy, which ostensibly violates the present epistemic/cultural methodological ethos of good scientific practice, is presented as endemic in both the scientific and policy practices involving population genetics and radiation protection of the 1950s and 1960s. Whether this strategy can be extended to other sciences or to other contexts, remains to be demonstrated, but there is evidence that it is at least one component of contemporary scientific and policy practice.<sup>177</sup>

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<sup>177</sup> The Bush (George W.) Administration has been repeatedly and harshly criticized by scientists and others for its tailoring of interpretations of scientific results in order to fit those interpretations into its predetermined and inflexible policy positions. The Union of Concerned Scientists (2006) obtained the signatures of thousands of scientists, including many Nobel laureates, to protest this policy strategy; see "Evidence of Political Interference: The A to Z Guide to Political Interference in Science," available at [http://www.ucsusa.org/scientific\\_integrity/interference/a-to-z-guide-to-political.html](http://www.ucsusa.org/scientific_integrity/interference/a-to-z-guide-to-political.html), accessed 16 January 2007. See also Andrew C. Revkin, "Bush vs. the Laureates: How Science Became a Partisan Issue," *The New York Times*, 19 October 2004, p. F1; Editorial, "Subverting Science," *The New York Times*, 31 October 2004, section 4, p. 10; Susan F. Wood, Ph.D., "Women's Health and the FDA," *The New England Journal of Medicine* 353: 1650-1, 20 October 2005; Editorial, "Censoring Truth," *The New York Times*, 9 February 2006, p. A26; Editorial, "The Politics of Science," *The Washington Post*, 9 February 2006, p. A22; Editorial, "A Fishy Policy: The Bush Administration's Big Chill on Speech Isn't Limited to Global Warming," *The Washington Post*, 3 June 2006, p. A16; Juliet Eilperin, "USGS Scientists Object To Stricter Review Rules: Pre-Publication Policy Seen as Cumbersome," *The Washington Post*, 14 December 2006, p. A29.

## CHAPTER VII

### Population Genetics and Radiation Standards, 1953-1958

Some men use statistics as a drunkard uses a  
lamp[p]ost—for support rather than illumination.  
—Curt Stern<sup>178</sup>

In this chapter, I present an historical narrative of scientists' efforts in the 1950s to make policy on radiation exposure limits, focusing on the genetic effects of radiation. The Cold War of the 1950s saw the advent of programmatic atmospheric atomic weapons testing, which the U. S. government believed was necessary in order to maintain an arsenal of deterrent and battlefield-ready nuclear weapons, especially after the USSR tested its first atomic weapon in 1949 and the Cold War arms race began (cf. Kennan 1982). In this period, responsibility for national policy on atomic weapons rested with the White House, the Department of Defense (DOD), and the Atomic Energy Commission (AEC), a civilian agency created by the Atomic Energy Act of 1946 that took over responsibility for all Manhattan Project research on 1 January 1947. The Joint Chiefs of Staff recommended weapons requirements to the President and the Secretary of Defense. The AEC's role was advisory only; it advised on the feasibility of weapons development. However, it was the AEC with its national laboratories that was responsible for the design of the atomic weapons that the military would recommend for development (Hewlett and Holl 1989, pp. 8-12), and since this included the testing of weapons, the AEC was responsible for ensuring the public's safety from testing as specified by the Atomic Energy Act of 1946 (Mazuzan and Walker 1984, ch. 1; Hacker 1994; Walker 2000, p. 14).

Although scientists knew as early as the Manhattan Project that radioactivity from fallout resulting from weapons testing would pose potential threats to human health, by at least 1953 many scientists became concerned about radioactive fallout in the environment,

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<sup>178</sup> Curt Stern (1902-1981), "Biology Course Notes," Curt Stern Papers, American Philosophical Society Library, Philadelphia, Pennsylvania; hereafter cited as APS. This phrase appears in Stern's handwriting on his typewritten biology course notes from the University of Rochester (ca. 1940s).

and what that might mean in terms of human health and safety. Geneticists saw the potential dangers of fallout as a threat not only to the exposed population, but also to future generations. Beginning in the late 1940s, geneticists were consulted and included on committees investigating and recommending radiation exposure limits, at the same time that genetics was gaining in credibility and expanding as a respected academic profession centered on evolution and Darwinian natural selection (see, e.g., Cain 1993). As the problem of radioactive fallout increased in the 1950s and attained national and international notoriety by 1955, geneticists increasingly spoke out against the dangers of fallout and radiation, and they were included on the various committees recommending exposure limits.

The purpose of this narrative is to construct an historical case study using the principles of the technological infrastructure of science. In particular, I consider the following themes developed in previous chapters: (1) *separating signal from noise*. The epistemological status of evolutionary theory and population genetics, as developed in Chapter VI, must be analyzed in order to grasp adequately the issues scientists were dealing with in this historical period. (2) *epistemic sovereignty*. Taking seriously the epistemic status of the science investigated means one must not adhere to epistemic sovereignty and prejudge the quality or efficacy of the scientific arguments of the time. (3) *epistemic politics*. The modern bifurcation of the epistemic and the political should be questioned and not prejudged in constructing the narrative. (4) *historiality*. Rheinberger's conception of historiality, with emphasis on recurrence in telling stories and on his and Rouse's view that norms, whether scientific or other, are futural and diachronic in the sense that their emergence and elucidation require future events to be adequately explicated, must be a consciously held and reflexive component of the practice of the historian constructing the narrative. The vagaries of an appropriate nonlinear epistemology of time, reflexively incorporated as a component of historiographical practice guiding narrative construction, should not be ignored or swept under the rug. (5) *the promise for future research*. Related to and in a sense derived from all of the above themes, is the notion that in attempts to separate signal from noise—whether that be discovering an entity or mechanism of nature or discovering a truth regarding the behavior of some component of a natural phenomenon—the

research efforts of the scientist normally must contain, if the scientist is to retain a research program and continue to work productively, the prospects or promise (although research *plan* is perhaps a better term, for there is never an actual *promise* at the time, only with recurrent hindsight) for future research. This “promise” can be the short-term knowledge of where to go next in experimental practice, or it could be a more long-term plan for future research programs. Significantly, the activity of developing such a plan, *in addition* to the historical and historiographical activity of reconstructing such practices, is an activity that has both epistemic and political components that are difficult to separate in practice. Nature and normativity are intratwined; one presupposes the other.

In this chapter, I begin with the advent of the international controversy over fallout from atomic testing. Then, I work back and consider geneticists’ efforts to advise the government on exposure limits. In Chapter VIII below, I take the narrative on policy regarding radiation exposure limits forward to 1965, after atmospheric weapons testing was banned, but also after, I argue, the hazards of increased radiation had become instantiated as part of our modern culture of risk (Beck 1992). By then, even though there was continuing resistance among scientists’ groups and others to many of the dangers of the atomic age—including fallout, radioactivity from nuclear power and the storage of nuclear waste, and other chemicals resulting from the economic expansion of American industry in the post-war period<sup>179</sup>—the debates over the use of these technologies and their potential biological effects were predicated not on their elimination, but on placing limits on them, providing cost/benefit analysis calculations, and establishing permissible dose levels. Indeed, the very

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<sup>179</sup> Popular cultural examples of such opposition can be gleaned from books and movies that had an effect on cultural attitudes in the late 1950s and early 1960s. Three movies that depicted end of the world scenarios (or nearly so) from nuclear war were 1959’s *On the Beach*, which depicted the ultimately apocalyptic effects of fallout on the earth from a nuclear exchange, and two post-Cuban Missile Crisis (1962) movies from 1964 that portrayed the nuclear arms race and nuclear stockpiling as immoral and illogical, the thriller *Fail-Safe* and the black comedy *Dr. Strangelove: Or, How I Learned to Stop Worrying and Love the Bomb*, directed by Stanley Kubrick. For an analysis of the fear generated by the nuclear age, see Weart (1988). For a scholarly view of these and other depictions of scientists in literature, see Roslynn D. Haynes (1994), *From Faust to Strangelove: Representations of the Scientist in Western Literature*, Baltimore: The Johns Hopkins University Press. In addition, Rachel Carson’s book *Silent Spring* (Boston: Houghton Mifflin) appeared in 1962; it depicted the potentially devastating effects of pesticides, and chemicals on the environment and their manifestation as cellular, reproductive, and embryonic disruption. Watkins (2001) argues that the dangers of radioactivity and fallout “set the stage for the reception of Rachel Carson’s urgent message” and “spurred an environmental movement that fallout failed to produce. . .” (p. 304).

concept of a tolerance dose, or maximum permissible dose level, for which it is ostensibly the job of the expert scientists to calculate and then recommend policy, presupposes these risks as acceptable and the corresponding technologies as culturally beneficial and desirable, despite any potential dangers. As I show, however, the activity of formulating such policy is an activity that has both epistemic and political components, and should not be regarded as the result of a process that is apolitical, disinterested, or otherwise epistemically sovereign. Finally, to illustrate a specific example of the intra-twining of the epistemic and the political, the question of Dobzhansky and Wallace's commitment and adherence to heterosis as an interpretation of experimental results on the genetic effects of radiation on *Drosophila*, is explored as an historical and historiographical question from the perspective of the technological infrastructure of science.

### **The Fallout Controversy**

To detail the cultural and political context in which this story takes place, it is useful to start with the advent of the controversy over radioactive fallout. The international debate over the hazards of fallout was triggered by the Atomic Energy Commission's (AEC) test of the first "dry" thermonuclear device.<sup>180</sup> The *Bravo* test, part of the *Castle* series, was conducted on 1 March 1954 near Namu Island, at the northwest perimeter of the Bikini Atoll in the Marshall Islands. *Bravo*'s 15 megatons—the equivalent of 1,000 Hiroshima bombs—far exceeded its expected yield of six megatons and spread lethal fallout over 100 miles to the east. The fallout irradiated the native inhabitants of the islands of Rongelap, Ailinginae, and Utirik, and the U. S. Navy evacuated the entire populations to the island of Kwajalein. In addition, the Navy evacuated 28 military personnel from Rongerik, where a weather station and other equipment were located to monitor the tests. The "fallout problem" made international headlines when the Japanese fishing boat *Fukuryu Maru* (*Lucky Dragon*), which

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<sup>180</sup> The development of the "dry" hydrogen bomb helped make possible the delivery of H-bombs by airplanes. The first H-bomb test, the 10.4 megaton *Mike* shot of 31 October 1952, weighed 164,000 pounds.

had been just outside the AEC exclusion area<sup>181</sup> (about 82 miles from Bikini) at the time of *Bravo* detonation, returned to Japan. All 23 crewmembers were suffering from radiation sickness; one later died (Hewlett and Holl 1989, pp. 172-7; Divine 1978, ch. 1; Lapp 1958). The AEC released no official information on the fallout from atomic testing until 15 February 1955 (Hewlett and Holl 1989, pp. 283-7; see AEC 1955).

Before *Bravo*, concern over fallout from weapons tests was mostly limited to AEC officials and researchers, some scientists, and to residents in the vicinity of the Nevada Proving Ground, the site of U. S. continental atomic testing since 1951. Before the *Upshot-Knothole* test series in 1953, the AEC included a section in its *Thirteenth Semiannual Report* (AEC 1953) that minimized the hazards that would be produced by fallout from the tests. The report reassured the public that

the highest levels of radioactivity released by the fallout particles are well below the very conservative standards fixing the amounts of radiation that can be received externally or internally by the human body without harming the present or later generations. (p. 125)

The AEC's rationale for stating that genetic hazards would be negligible was that while "mutations are [induced] in proportion to the dose, with no repair or recovery process at work," the spontaneous mutation rate for humans

is so low that it is doubtful that a doubling [of the spontaneous mutation rate, estimated to be produced by exposure to 80 roentgens] would be noticeable in one generation since a large proportion of the mutations are recessive. (p. 123)

The report concluded by stating that

[o]n the basis of experiments and observations so far made, it appears that over a number of generations radiation from fall-out from Nevada tests would have no greater effect on the human mutation rate in the United States than would natural radiation in those parts of the Nation where the background [radiation] levels are high. (p. 123)

While a few of the Nevada tests produced local fallout that prompted officials to warn area residents to stay indoors, and while the AEC knew as early as 1951 that fallout could be deposited thousands of miles from the Nevada Test Site (Hacker 1994, pp. 50-2), the fallout

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<sup>181</sup> The AEC set up the exclusion area around the Pacific Proving Ground not as the result of fear of fallout, but because of security concerns (Hewlett and Holl 1989, p. 171).



did not cause public panic or widespread protest. Except for the question of whether fallout was responsible for a number of sheep deaths near the test site,<sup>182</sup> the continental tests and fallout prior to *Bravo* caused little public concern (Hewlett and Holl 1989, pp. 150-6), although government researchers had known of its possible dangers since the Manhattan Project (see, e.g., Szasz 1984, Hacker 1987). Indeed, the potential for fallout from atomic bombs to irradiate large areas was known since the Trinity test in New Mexico of the first atomic bomb on 16 July 1945. Indeed, evacuation plans for the test were devised in case populated areas became exposed to high radioactivity levels, a contingency that was largely dependent on the weather. Fortunately for the Manhattan Project, the regions that showed the highest radioactivity levels, some as high as 20 R/hour, were mostly uninhabited.<sup>183</sup> Since all detailed information on fallout from the Trinity test was classified, the only information available to the public was from government officials. Until the early 1950s, the official line was that *no fallout* had resulted from the Trinity test (Szasz 1984, ch. 6) and that the health hazards of fallout from atomic testing were negligible.<sup>184</sup>

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<sup>182</sup> In May of 1953, fallout from the Nevada test site irradiated herds of sheep grazing nearby in Utah. The AEC responded by investigating the dead sheep and conducting experiments to see the effects on test sheep from exposing them to radiation. Scientists involved in the investigation, and other participants at a meeting concerning the sheep deaths, testified over 25 years later that Gordon Dunning, a health physicist with the AEC's Washington office, "said that he wanted a unanimous statement that fallout was unlikely to have caused the deaths" (R. Jeffrey Smith 1982, "Scientists Implicated in Atom Test Deception," *Science* 218: 545-547, quotation from p. 546). Even in the case of the 1953 sheep deaths, which resulted in a 1955 lawsuit by ranchers seeking compensation (that was dismissed in 1956), the AEC saw it necessary to suppress the truth of the effects of fallout and to seek expert opinion to justify their conclusions that fallout was harmless.

<sup>183</sup> The dose of radiation at which 50% of humans exposed would be expected to die (LD-50) was calculated to be about 450 to 500 R. If the 20R/hour level were to hold for several hours, fatalities would have resulted if the Trinity fallout hit populated areas, and persons in those areas took no precautions.

<sup>184</sup> Following the decision in 1950 to develop the hydrogen bomb, and with the advent of the continental weapons testing program in 1951, fallout from atomic testing became commonplace in the United States. From 1949 to 1958, the U. S. conducted at least 166 atmospheric tests and 22 underground tests; the USSR and U. K. conducted another 111 atmospheric tests (Norris and Ferm 1988). By 1952, the AEC had in place a nationwide system for collecting data on radiation levels from fallout. The AEC's 121 monitoring stations, most of which were located at Weather Bureau Stations, collected daily airborne and dust samples and forwarded them to the AEC's New York Operations Office (NYO), where they were analyzed. Merrill Eisenbud (1915-1997), head of the Health and Safety Division of the NYO office, was in charge of the project. A 1953 article in *Science* (Eisenbud and Harley 1953), describing the AEC's fallout monitoring facilities, represents one of the few early attempts to disseminate fallout data to the public. The findings of the article are based on continental tests conducted in the winter and fall of 1951 and in the spring of 1952. The tone of the article is reassuring; it argues that the radiation levels from fallout are insignificant when compared to levels from naturally occurring radioactivity: "For brief periods immediately following a detonation, the radioactive background can be markedly increased in some areas, but the cumulative dose from such depositions are minute

However, events in early 1953 proved the AEC's calculations wrong. Herbert Clark, a chemist at Rensselaer Polytechnic Institute in Troy, N.Y., observed on 27 April that the Geiger-Mueller counters in the radiochemistry lab were giving readings some three times higher than normal. The previous evening, the city of Troy—some 2300 miles from the Nevada Proving Ground—had experienced a torrential rainstorm. Clark determined that fallout from a test conducted on 25 April had been deposited on Troy, and that “the extent of the rainout was much greater than that detected in the area from any previous nuclear detonation” (Clark 1954, p. 619). While Clark characterized the fallout levels as “exceptionally high, though not hazardous,” he calculated that on the first day after the fallout, “the [radio]activity of drinking water was greater than 1  $\mu\mu\text{c}/\text{ml}$  or about 100 to 1000 times greater than the natural radioactivity generally associated with surface and ground water, namely . . .  $10^{-2}$  to  $10^{-3}$   $\mu\mu\text{c}/\text{ml}$ ” (p. 621).<sup>185</sup> Clark's analysis showed that this level violated the NCRP standard for maximum permissible concentrations in water (Handbook 52, 1953), set at 0.8  $\mu\mu\text{c}/\text{ml}$  for strontium-90, and 0.01  $\mu\mu\text{c}/\text{ml}$  for an unknown mixture of fission products (Clark 1954, p. 621). The levels did not, however, violate the Federal Civil Defense Authority's emergency standard of 5,000  $\mu\mu\text{c}/\text{ml}$ .

The Troy rainout did not receive national attention until after the *Bravo* shot and the beginning of the fallout controversy (Sternglass 1981). In fact, in the spring of 1954, after the *Bravo* test and the controversy that erupted over the Japanese anglers exposed to fallout, the public temporarily lost interest in hydrogen bombs and fallout (Divine 1978, pp. 25-7). While the destructive power of the hydrogen bomb—AEC Chairman Lewis Strauss

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because of the rapid decay of the activity. The long-lived components of the radioactivity are of a low order compared with the natural radioactivity of the earth's surface and atmosphere” (Eisenbud and Harley 1953, p. 147). According to the NYO data reported in the article, the total radiation exposure from fallout, “under the worst conditions observed [0.05 mR], is . . . no greater than the dose received from natural radioactivity in a period of 25 days.” Since the NCRP maximum permissible dose was 0.3 R/week, the worst dose from fallout was only “one sixth that allowed in a week” (pp. 146-7). The article stressed that environmental radioactivity, especially from radon gas, results in a much more significant exposure than does fallout. It argued that the *daily* lung dose from radon gas, calculated to be 0.01 rem (a unit of dose equivalent, equal to the energy of the radiation source, in rads or roentgens, multiplied by other factors, such as the relative biological effectiveness or RBE, specifying the effectiveness of the type of radiation in producing biological damage; see Eisenbud 1984, p. 32), resulted in a much higher exposure than fallout, since the *total* dose from one atomic test was calculated at 0.02 rem (p. 147). Clearly, fallout was seen to be a negligible hazard to human health.

<sup>185</sup> A  $\mu\mu\text{c}$ , or micromicrocurie, is  $10^{-12}$  curie, or one picocurie. The curie is a unit of radioactive decay equal to  $3.7 \times 10^{10}$  nuclear transformations per second (Eisenbud 1984, p. 31).

announced to stunned reporters on 31 March 1954 that an H-bomb could “destroy a city”<sup>186</sup>—first became real to the American people as a result of *Bravo*, other news headlines drew attention away from atomic testing and fallout, including the Army-McCarthy hearings of the House Committee on Un-American Activities, the Oppenheimer security clearance hearings, and the Supreme Court school desegregation decision (Divine 1978, pp. 26-7).

The full magnitude of the fallout from *Bravo* became clear only after the nuclear physicist Ralph E. Lapp began a series of articles on fallout for *Bulletin of the Atomic Scientists* (BAS). Lapp had been Assistant Director of the Metallurgical Laboratory in Chicago during the Manhattan Project. After the war, he was scientific advisor to the War Department General Staff and Acting Head of the Nuclear Physics Branch of the Office of Naval Research (ONR). In 1950, he left the ONR to organize a writing and lecturing service. Lapp had become discouraged over the failure of the U. S. government to institute an adequate plan for civil defense in case of atomic war. Interestingly, in 1950 he saw no need to worry about the radioactivity from atomic bombs:

Radioactivity has become a kind of national villain. As such, radioactivity has been grossly exaggerated as a hazard, and I personally believe that in an atomic war we would pay little attention to it. (Lapp 1950, p. 242)

However, by late 1954, Lapp’s view on fallout had changed markedly. In a series of semi-technical articles published in *Bulletin of the Atomic Scientists* (of which he was guest editor) from November 1954 to June 1955,<sup>187</sup> Lapp dissected the major scientific and policy issues of the emerging problem of fallout.<sup>188</sup> In these articles, Lapp questioned not only the AEC’s candor in releasing information on fallout to the public, he also questioned the scientific basis of the arguments used by the AEC to justify what it perceived to be the nonexistent, minimal, or acceptable health hazards resulting from atomic testing. Lapp’s analyses represent the first important public quantitative assessments of the fallout resulting from atomic explosions, even if they were based on unofficial reports and estimates from the weapons tests (1955a). In fact, his calculations were deemed so accurate by the government

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<sup>186</sup> Quoted in Williams and Cantelon (1984), pp. 112-13; see also Hewlett and Holl (1989), pp. 181-2.

<sup>187</sup> See Lapp (1954a, 1954b, 1955a, 1955b, 1955c, 1955d).

<sup>188</sup> Cf. Hewlett and Holl (1989), pp. 281-2; Divine (1978), pp. 32-3; and Lapp (1979), p. 5.

that they resulted in allegations that information had been leaked to BAS by an AEC insider (Cook 1955, p. 196; Lapp 1955d, p. 339). Lapp's articles were clearly a major reason that the AEC finally issued a report in February 1955 (AEC 1955) stating its official position on fallout hazards.<sup>189</sup>

The most significant aspects of the Lapp articles were that they first revealed (1) the AEC's tactics in dealing with the fallout problem; (2) the AEC position, and how it differed from the views of independent scientists and from the NCRP standards; (3) the AEC's scientific arguments for dealing with the problem; and (4) the fact that it was possible to quantify, at least in terms of a statistical argument, the nature and extent of the fallout and the perceived health hazards that might result. Lapp's initial article (1954a) provided a brief history of how the U. S. government had handled the problem of disseminating to the public information concerning atomic energy since World War II. As he revealed, the release of atomic information was under the direct control of the five Commissioners of the AEC, who were under pressure from the White House and the military to prevent sensitive information from being leaked. Several attempts at "public enlightenment" concerning the new atomic age had been attempted, but they either had been canceled or were impossible to obtain (pp. 312-14). For example, *The Effects of Atomic Weapons* (Glasstone 1950) was provided in 1950 to civil defense authorities who had been requesting official data on atomic weapons effects. With this report, as Lapp (1954a) remarked,

[c]ivil defense got more than it bargained for. What it needed was a brief and understandable description of what damage is created by modern explosives. What it got was a Handbuch der Physik compilation replete with formidable equations and adorned with masses of technical phraseology. In effect, the AEC had handed a book on quantum theory to a youngster in knee britches. (p. 313)

Indeed, an ongoing tactic of the AEC was to make issues involving atomic weapons and

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<sup>189</sup> Ironically, in February of 1954, just before the *Bravo* incident, Eisenhower, who had given his "Atomic Power for Peace" address before the United Nations General Assembly on 8 December 1953, indicated to Congress that he favored changing the Atomic Energy Act of 1946 to end the government's monopoly on fissionable material, and that he favored peaceful and private development of atomic energy (Mazuzan and Walker 1984, p. 24). The Atomic Energy Act of 1954, which took effect on 30 August 1954, gave the AEC increased regulatory power over privately developed nuclear power, and reinforced the AEC's responsibility for health and safety (*ibid.*, p. 30).

fallout as confusing as possible, at least when information was released.<sup>190</sup> Clearly, its rule of thumb was to maintain a policy of secrecy, the main justification of which was national security concerns.<sup>191</sup>

In addition, the withholding of pertinent information was another tactic used by the AEC in dealing with atomic information, especially fallout. In its first official release of fallout information on 15 February 1955 (AEC 1955) the AEC ignored pertinent aspects of fallout effects (Lapp 1955b, 1955c). These included what Lapp identified as the persistence over time of fallout, the internal hazards, and the genetic effects. In its *Sixteenth Semiannual Report* (AEC 1954), the AEC stated that, as a result of the *Bravo* test in which American test personnel, island natives, and Japanese fishermen had been exposed to fallout, none of the test personnel showed any symptoms of radiation sickness and that “medical observations” did not indicate that permanent injury had resulted (pp. 51-2).

In addition, the natives who had been exposed were reported by the AEC (1954) to have experienced radiation burns, but that they were “almost completely healed.” As for internal exposure to fission products, the report stated that “in no case did the body burden for the various radioactive isotopes exceed the permissible limits.” The report concluded that “there is no reason to expect any permanent after effects on the general health of these people” (p. 52). In addition, the report mentioned that the human body is continually being exposed to radiation from cosmic rays and the soil as well as from naturally occurring radioactive isotopes. It concludes with the following:

The level of activity from fall-out, outside the area surrounding the Pacific Proving

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<sup>190</sup> McGeorge Bundy (1919-1996), who was Special Assistant for National Security Affairs to Kennedy and Johnson from 1961-1966, argues that Eisenhower’s “Atoms for Peace” program, initiated in 1953, became, in part, a means to divert public attention away from the dangers of atomic weapons (Bundy 1988, pp. 287-95). Concerning Atoms for Peace and AEC Chairman Lewis Strauss’s (1896-1974) handling of it, Bundy states: “Atoms for Peace, in [Strauss’s] hands, became not only the occasion for a new emphasis on the peaceful promise of the atom, but also the starting point for a conscious and sustained policy of using that promise as a means of reducing attention to the dangers of nuclear weapons. By the autumn of 1955 he felt able to claim success in this effort, proudly announcing to an audience of enthusiasts for nuclear power that at his [1955] Geneva conference [the Atoms for Peace Conference] on peaceful uses, ‘the mesmerism of the bomb’ had been cast off. ‘No other event that has occurred has done so much toward taking the horror—the terror—out of the atom’” (p. 303).

<sup>191</sup> See, for example, Hewlett (1981); Boyer (1985), pp. 187-8; Seidel (1986), p. 145; Forman (1987); Hewlett and Holl (1989), p. 294; and Kevles (1990). President Truman created the National Security Agency (NSA) in 1952.

Ground, has been far less than any required to produce detectable injury either from the radioisotopes within the body or from external radiation, or from a combination of the two. (p. 54)

As Lapp mentioned in his rebuttal to the AEC release of February 1955, the “AEC report is not candid on the persistence of Fall-out.” The report provided no information on the regions hardest hit by the fallout. Lapp’s calculations using AEC data showed that the 110-mile downwind location of the 1954 accident would produce lethal doses of radioactivity (to unprotected persons) for as long as several months after the blast (Lapp 1955b, pp. 170, 200). According to the AEC report, the test “contaminated a cigar-shaped area extending approximately 220 statute miles down-wind and varying in width up to 40 miles” (AEC 1955, p. 13). This suggests that fallout was limited to this area; however, fallout was not limited to this region.<sup>192</sup> The “contaminated area” mentioned in the AEC report included only the region in which deaths were likely to occur:

At a distance of 220 miles or more down-wind, it is unlikely that any deaths would have occurred from radioactivity even if persons there had remained exposed up to 48 hours and had taken no safety measures. (p. 15)

Furthermore, the information on the exposure of the natives was also less than candid. It neglected to mention that many of the islanders suffered from acute radiation poisoning, the symptoms of which included nausea, vomiting, and diarrhea (Eisenbud 1990, pp. 97-103). The prediction that no long-term effects to their health would result was not realized; 19 of the 21 children on the island of Rongelap who were under the age of twelve at the time of the blast eventually developed thyroid tumors, although this cancer did not appear until ten years later (Titus 1986, p. 48). Moreover, the results of actual tests performed on the Rongelapese indicated that the mean internal thyroid levels of Iodine-131 radioactivity were reported to be between 6.4 and 11.2 microcuries at one day. This far exceeded the maximum

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<sup>192</sup> The fallout pattern from *Bravo* was discussed at an AEC meeting on 24 May 1954 (Hewlett and Holl 1989, p. 182). General Kenneth E. Fields presented a diagram with the *Bravo* blast superimposed on Washington, D.C. (on p. 181). The fallout would have resulted in a lifetime dose of 5,000 roentgens to fully exposed persons in the Baltimore-Washington area; over 1,000r to those in Philadelphia; and over 500r to those in New York City. The dose at which half of the exposed population would be expected to die (LD-50) is about 500r. According to Hewlett and Holl (1989), the “diagram was classified secret and received very little attention beyond the [AEC] Commissioners” (p. 182). The spread of fallout from the *Bravo* shot, if detonated over Washington, D.C., would have extended past the Canadian border.

permissible amount, 0.3 microcuries, as set by the NCRP (Eisenbud 1972, p. 405).<sup>193</sup>

Hence, by early 1955, the controversy over fallout and radiation safety had become a persistent problem for the AEC.<sup>194</sup> Furthermore, it became entrenched in political debates concerning atomic testing (see, for example, Divine 1978; Walker 1989, 2000). As Hewlett and Holl (1989) state: “The *Bravo* shot unexpectedly had forged inseparable links between the fallout issue and international demands for a nuclear test ban” (p. 295). One of the most polemical and emotional aspects of the controversy was concern over the genetic effects of radiation. This concern became intertwined with concerns over fallout and atomic testing, since many of the country’s top geneticists were asked to serve on committees (government and independent) that were charged with providing recommendations on exposure to radiation. Moreover, the classical/balance controversy became entwined with these debates. Wallace and Dobzhansky’s position in the controversy hardened in the mid-1950s, and within the genetics community, scientific battle lines were drawn. At the same time, the AEC exploited the classical/balance controversy in order to deflect growing public concern over genetic effects of fallout and low-level radiation, and to create confusion and uncertainty regarding atomic weapons testing and its potential biological hazards.<sup>195</sup>

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<sup>193</sup> As Eisenbud (1972) states in his textbook on radioactivity and its biological effects, the U. S. weapons tests “discharged into the environment . . . amounts of radioactivity that were large in relation to the prohibitions self-imposed by the AEC. . .” (p. 12). And as far as the biological effects of radiation were concerned, Eisenbud concludes that “in one way or another, almost all the major effects of ionizing radiations on man were known prior to World War II. Moreover, the basic techniques for protecting workers were also known and have been utilized ever since with minor basic modifications” (p. 19). In addition, in his oral history, Eisenbud (1995) stated: “I don’t understand why our country could not have been more forthright after *Bravo*.” He recalled that “[t]here are press releases that were made, which were lies. . . .” Regarding AEC Chairman Lewis Strauss and his handling of *Bravo*, Eisenbud stated: “There were a lot of things about L[ewis] Strauss that I did not understand, still don’t understand, and some of those are discussed in my book [Eisenbud 1990]. This whole story of *Bravo* was something that he was mixed up with.”

<sup>194</sup> According to Bundy (1988): “Neither Eisenhower nor any member of his administration was responsible for originating interest in a test ban. Where Atoms for Peace had been essentially an internally developed gimmick designed to give hope, . . . the pressure for limiting or ending tests emerged from a widespread fear of radioactive fallout” (p. 329).

<sup>18</sup> The historical narratives presented in this chapter and the next stand in contrast to many of the interpretations of several of the most visible accounts of atomic testing, radiation safety, and nuclear regulation. J. Samuel Walker, Historian of the Nuclear Regulatory Commission, has written several books and articles on the history of nuclear regulation (Walker 1989, 1992, 1993, 1994, 2000, 2004; Mazuzan and Walker 1984). These works lack the critical engagement needed to account fully for the deception and deliberate confusion created by officials in the U. S. government. They clearly take a pro-AEC, FRC, and NRC stand on these agencies’ roles in history, and only one (Mazuzan and Walker 1984, pp. 44-58) considers the role the genetic

## The Genetic Dangers of Fallout

One component of this emerging debate over fallout and the hazards of radiation was the genetic dangers. In the 1950s, genetics as an organized academic discipline was still young, especially when compared to the medical sciences. Indeed, the genetic effects of fallout became the most polemical and disputed aspect of the fallout controversy, in part because of the perception that genetics was not on strong experimental or epistemic ground. The public and scientific polarization of views on the genetic dangers of fallout began at least as early as June 1954 when Alfred Sturtevant, an internationally respected geneticist and former colleague of Dobzhansky, gave the Presidential Address (Sturtevant 1954) at the Pacific Division of the American Association for the Advancement of Science (AAAS) in Pullman, Washington.<sup>196</sup> After discussing a diversity of subjects such as the genetic basis of the nature/nurture controversy and its misuse in racist viewpoints, Sturtevant devoted the

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effects of radiation played in the debates over radiation safety. This work does, on the one hand, state that the AEC “undermined its own credibility by consistently placing the most benign interpretation on available information”; that it did not “convincingly counter arguments that fallout would present a growing threat in future years if testing continued”; and that this “intensified doubts about its position and damaged its public image” (p. 58). On the other hand, Mazuzan and Walker (1984) do not give an in-depth analysis of the scientific data on genetic or somatic effects, and the overall tone of the book is pro-government. Walker’s other work, while carefully researched, adopts similar positions.

Similarly, Barton C. Hacker—who was originally hired in 1978 (the year before the opening on 16 March 1979 of the anti-nuclear power movie, *The China Syndrome*, itself two weeks before the Three Mile Island nuclear power plant accident on 28 March 1979) by Reynolds Electrical and Engineering Co., Inc. (described in the preface to his 1994 book as a “Department of Energy Nevada Operations . . . prime contractor”) to write what became his 1987 and 1994 books, and then served from 1992-6 as historian at the Lawrence Livermore National Laboratory—adopts a perspective on the history of radiation safety that avoids virtually any criticism of the U. S. government and its agencies, and neglects pertinent issues and events. Hacker (1994) states his book’s thesis: “Those responsible for radiation safety in nuclear weapons testing under the auspices of the Atomic Energy Commission were competent, diligent, and cautious. They understood the hazards and took every precaution within their power to avoid injuring either test participants or bystanders. Testing, of course, meant taking risks, and safety could never be the highest priority. Those in charge sometimes made mistakes, but for the most part they managed to ensure that neither test participants nor bystanders suffered any apparent damage from fallout. Describing how they did so, in terms as neutral as possible and as fully as the sources will allow, defines this book’s ultimate purpose” (p. 9). Two brief examples show that this last claim was not realized. First, Hacker (1994) states that the fallout controversy “vanished when testing went underground after the 1963 limited test ban treaty” (p. 6). The narrative on the Federal Radiation Council (FRC) in Ch. VIII below shows this is false; concerns over rising fallout levels extended into the mid-1960s and prompted Congressional hearings, the protests of scientists’ groups, and public concern. Second, the genetic effects of radiation are given a total of 5 sentences on 3 pages (pp. 184, 185, 227) and only one geneticist, Muller, is ever mentioned. However, Hacker presents him as *Herbert J. Muller* (on p. 184 and in the bibliography, p. 523), whose “path-breaking 1927 article” initiated the “so-called threat to the germ plasm. . .” (p. 184). Despite its voluminous information and breath-taking documentation, the book was almost useless in writing this dissertation.

<sup>196</sup> Cf. Kopp (1979) and Divine (1978), pp. 33-4.



remainder of his address to the genetic hazards of fallout. While he admitted that much uncertainty confounded attempts to quantify the genetic damage from radiation, especially in humans, he stated that “there are certain general qualitative results that have now been so widely confirmed that we may confidently assert that they apply to all higher organisms, including man” (pp. 406-7). Among these results was the notion that there is “no threshold value below which irradiation is ineffective” in producing mutations. He went on to state that the

overwhelming majority of these mutations is deleterious—that is they seriously affect the efficiency of individuals in later generations in which they come to expression. These deleterious genetic effects may lead to early death or to any of a wide variety of defects, often gross ones. (p. 407)

Sturtevant’s address is significant for two reasons. First, he was advocating the classical line, according to which virtually all mutations are to be considered dangerous, as they add to the population’s genetic load. Second, the reference to “gross” genetic defects reinforced the popular misconception that genetic defects are limited to horrible physical deformities or monsters.<sup>197</sup> Such a view, a distortion of Muller’s classical position, had made its way into popular culture and even some professional literature since the bombings of Hiroshima and Nagasaki. When fallout became a public concern after the 1954 *Bravo* shot, the AEC tried to deflect such arguments and prevent public panic. Third, Sturtevant’s exaggerated claims, surely aimed at combatting AEC propaganda on the harmlessness of radiation, called into question the foresight of atomic testing policy. However, while his efforts did help create public awareness of genetic dangers, his overreaction hurt the credibility of geneticists. It is not surprising that the AEC sought its own expert opinions on the hazards of fallout, and strove to reassure the public of the safety of atomic testing, especially when prominent scientists such as Muller and Sturtevant made alarming claims:

There is no possible escape from the conclusion that the bombs already exploded will ultimately result in the production of numerous defective individuals—if the human species survives for many generations. And every new bomb exploded, since its

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<sup>197</sup>See, for example, Scheinfeld (1947) and Mumford (1954), p. 151. In this context, “gross” defects refers to visible or easily recognizable genetic abnormalities. In this period, the term “monsters” was often used to denote such obvious physical deformities. Carlson (1981) characterizes Sturtevant’s views on radiation hazards at this time as “errors” (p. 356).

radioactive products are widely dispersed over the earth, will result in an increase in this ultimate harvest of defective individuals. (Sturtevant 1954, p. 407)

Muller himself believed that his position was misunderstood as consonant with the view that the genetic damage from radiation often produces gross, visible defects, or monsters. As he wrote in 1953:

Sometimes the warnings have been sensationalized and exaggerated so that people got the idea that their children were likely to be monsters, but more usually the warnings were quietly sidetracked by the people dealing with radiation, both medical men and others, because they did not want to have the public think that they might be doing damage. When referring to my warnings, they usually made it appear that I had indulged in the same kind of sensational presentation of them as that of the alarmists. . . . (quoted in Carlson 1981, pp. 354-5)

It may have been such a perception of Muller's views that led the influential chemist and AEC Commissioner Willard F. Libby to ban Muller from reading a paper (Muller 1955c) on the genetic hazards of radiation at the United Nations International Conference on the Peaceful Use of Atomic Energy (Atoms for Peace Conference), held in Geneva in August 1955.<sup>198</sup> After the conference, Muller learned that the AEC had deliberately removed him from the conference schedule (probably from Glass, who was a new member of the ACBM). The incident caused public embarrassment for the AEC, especially since it had initially

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<sup>198</sup> On the "Muller fiasco," see Carlson (1981), ch. 25; Beatty (1987), pp. 303-7; and Hewlett and Holl (1989), pp. 266-9. Elof Carlson, a student and biographer of Muller, interviewed Libby on 8 April 1971 at UCLA. He transcribed his notes immediately after the interview and wrote them in dialogue form ("Interview with Willard F. Libby on H. J. Muller and Radiation Hazards," Curt Stern Papers, APS). Libby insisted that Muller was a communist and a spy and that he tried, with Oppenheimer, to sabotage the development of the hydrogen bomb: "I don't see why a son-of-a-bitch like that should have any support. He's like so many intellectuals of that time that I knew. . . . They wouldn't defend their country if it needed them. Muller didn't care for this country at all. He was a spy." Later Libby stated: "He would have done anything to sabotage our defense program. He's no different from that other traitor, Oppenheimer." Later: "I despise him as a traitor and spy. God knows how many students he converted to communism. You can't undo that you know. But as a scientist he was the best of the lot. His discovery was one of the major biological findings of this century. He was a real genius too, much brighter than his contemporary geneticists." Minutes of an ACBM meeting in January 1956 reveal that an AEC official with the DBM stated that Libby (1908-1980), who later won the 1960 Nobel Prize in chemistry for his development of carbon-14 dating, had "instructed our office to see to it that the paper was not included on the program for oral presentation." The reason given was Muller's "loyalty problem" ("Minutes, Meeting of Advisory Committee for Biology and Medicine [ACBM] held at the New York Operations Office and the Radiological Research Laboratory, Columbia Medical Center, New York, New York, January 13 and 14, 1956," DOE/NV Nuclear Testing Archive, Las Vegas, NV).

blamed the United Nations for rejecting Muller's paper.<sup>199</sup> In an interview after the conference was held, but before the controversy broke, AEC Chairman Lewis Strauss commented on the outcome of the Geneva conference:

I think there is—there was a good deal of discussion of radiation hazards and the meteorological aspects of radiation hazards and some of the irresponsible statements that had been made on the subject were liquidated in the course of that Conference. (quoted in Cook 1955, p. 197)

The aftermath of the controversy caused a stir in the genetics community. Many geneticists believed that the conference had been “packed” to serve the AEC position (Cook 1955, Beadle 1955). Bruce Wallace, whose experiments (e.g., Wallace and King 1951, Wallace 1952, 1956) indicated that populations of irradiated *Drosophila* had produced generations with a higher proportion of viable offspring than control groups, was one of the speakers at the conference (Wallace 1955). In March 1955, prior to the conference, the AEC had presented Wallace's AEC-sponsored research program to the public in *U.S. News and World Report*:

AEC TESTS SHOW: Fruit flies, raised for 128 generations in highly radioactive surroundings, did not degenerate as expected. Instead, they ended up a better race of fruit flies—heartier, more vigorous, more reproductive, with better resistance to disease.<sup>200</sup>

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<sup>199</sup> Muller wrote to Curt Stern after the Geneva conference controversy and admitted he “felt rather bitter about the matter.” He indicated “that on Oct. 3 Admiral [and AEC Chairman Lewis] Strauss issued to the press a statement which has been referred to as an apology. He said that he was sorry about the ‘regrettable snafu’ and that the A.E.C. made a mistake in having simply withdrawn my paper when what they should have done was to ask me to remove the references to Hiroshima in it. It is of course very difficult for me to believe that these references, which after all were minor ones, constituted the real reason for the rejection of the paper, but if his statement is an indication that in the future the A.E.C. will not give preference to the expression of opinions antagonistic to the idea that radiation is genetically damaging, then I shall consider the incident to have had a beneficial result after all. Incidentally, although a statement [of apology] was made to the press, none was conveyed to me personally” (Muller to Stern, 7 October 1955, Curt Stern Papers, APS).

<sup>200</sup> “The Facts About A-Bomb Fallout,” *U. S. News and World Report*, 25 March 1955, pp. 21-6; quotation from p. 25. In May of 1955, *U. S. News and World Report* interviewed Muller on the genetic effects of radiation, indicating he “expresses views independent of those of the Atomic Energy Commission” (“What Will Radioactivity Do to Our Children?” *U. S. News and World Report* 38, 13 May 1955, pp. 72-78). The interview began with the question whether “Communists are behind the statement that continued atomic tests will build up enough radiation to harm the human race?” (p. 72) and continued with the magazine presenting the AEC line in its questioning of Muller. The interview ended with a question concerning Wallace's experiments on heterosis in irradiated populations of *Drosophila*: “But hasn't it been shown that in fruit flies radiation changes actually were helpful—that a stronger, tougher, strain resulted?” Muller answered: “Well, in fruit flies, multiplication can be very rapid. For this reason the very few advantageous mutations produced by the irradiation are able to crowd out the rest of the population in a relatively short time, while the original type and

The article accused certain people of causing unnecessary “fear of A-bomb test[s],” and asserted that fallout from atomic testing was not hazardous. As for the possible genetic dangers, the article claimed that “some experts believe that mutations usually work out in the end to improve species.”

This article represents perhaps the first public presentation of the AEC’s distorted version of the hardened balance view of Dobzhansky, Lerner, and Wallace (see Ch. VI above). Contrary to the explicit positions of these scientists, the article was an overt attempt to demonstrate the harmlessness of atomic testing. Wallace expressed “dismay” at the AEC’s “shallow analysis” of his experiments (Wallace 1991, p. 47), and a number of noted scientists complained to the AEC, including Linus Pauling, Curt Stern, and Muller (Kopp 1979, p. 410). In a letter to John Bugher, a member of the AEC’s Advisory Committee on Biology and Medicine (ACBM), Curt Stern, the geneticist member of the ACBM, cited some of the offensive reporting:

“New experiments show present ‘fall-out’ level cannot harm future generations.”

“There is agreement among geneticists working with [the] AEC that some changes in future generations are likely to result only from heavy or successive doses.”

“. . . there is no evidence that ‘fall-out’ from test explosions now carried out by the U. S. will be hazardous to people now, [or] to future generations. . . .” (Stern to Bugher, 28 March 1955, Curt Stern Papers, APS)

On these assertions, Stern retorted:<sup>201</sup>

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the many weaklings die out. It would be absurd to suppose that anything like this could be done with mankind, because humans do not multiply like flies. Moreover, unlike fruit flies, humans are extremely variable to begin with” (p. 78).

<sup>201</sup> Stern also sent a copy of this letter to the other members of the ACBM and to George Beadle, the former geneticist member of the ACBM, and member of the Genetics Committee of BEAR I. In addition, Stern wrote a separate letter to ACBM chairman Failla, questioning him about another statement in the *U. S. News and World Report* article, namely that “within the U. S., the planners have set a limit of 3.9 roentgens of total exposure for a year as the maximum any community will receive from tests permitted. This is only a fourth of the maximum amount that atomic workers may be subjected to routinely during the year” (Stern to Failla, 28 March 1955, Curt Stern Papers, APS). Stern complained: “I do not know whether this statement is correct and, if so, whether ‘the planners’ refers to AEC personnel. Unless I am greatly mistaken, the Advisory Committee [ACBM] has not been consulted about this limit although the function of the committee should surely include advice on a bio-medical problem of such importance.” Stern requested that Failla and Bugher “place this item on the agenda of the next meeting [of the ACBM] unless the information given by the magazine is wrong.”

The truth of the first two sentences quoted will be denied by all geneticists, I am certain. The third sentence has everything against it although the wording, “there is no evidence” keeps it from being a direct untruth. (*ibid.*)

On Wallace’s heterosis work, the magazine article declared: “The result: a much improved race of fruit flies with more vigor, hardiness, resistance to disease, better reproductive capacity.” In his letter, Stern declared this interpretation of Wallace’s experiments to be

absolutely and intentionally misleading by not mentioning the, in human terms, terrible heavy loss of life due to selective elimination of the majority of offspring. The same is true for the statement, “Some experts believe that mutations work out in the end to improve species.” This principle is valid for animals and plants under the rigor of natural selection but we cannot apply it to radiation induced mutations in man where the certain costs in defectives must be weighed against the possible gains. (*ibid.*)

It is significant to note that Stern supported atomic testing throughout the 1950s. It is clear, however, that he rejected the notion that no genetic damage would result from testing, and that he was categorically against any interpretation of genetics experiments that attempted to argue that increased mutation rates could be good for humans.<sup>202</sup>

It may have been this article that prompted Muller to respond to the AEC’s position on fallout in his lecture at the National Academy of Sciences in Washington, D.C. According to Hewlett and Holl (1989), Muller’s lecture (Muller 1955a, 1955b) “caused alarm in government circles because it explicitly related genetic damage to nuclear testing and

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Stern also sent copies of this letter to the other members of the ACBM (*ibid.*). From the Stern papers at the APS, it is not clear why Stern left the ACBM in 1955, as membership was on a rotating basis for 3 years (see AEC Press Release No. 291, 23 June 1950). It may be that Stern left the committee as a result of this controversy. Bentley Glass replaced Stern.

<sup>202</sup> In response to the 1952 Nobel Peace Prize winner Albert Schweitzer’s (1954) call in *Science* for scientists to speak out about the biological effects of atomic testing—which was clearly motivated by the controversy surrounding the *Bravo* shot (“The Scientists Must Speak Up,” *Science* 120, 10 September 1954, p. 11A)—Stern (1954) wrote in defense of Schweitzer’s stand against nuclear war and the arms race, but did not reject the need for atomic testing. He gave a brief overview of the arguments for the genetic damage produced by increased radiation from testing, and concluded: “Relatively then and in terms of mankind as a whole the biological consequences of controlled test explosions are very small.” As for atomic testing, Stern stated that the “question of whether or not nuclear tests are of ultimate benefit to mankind is one of political opinion and partly one of scientific prophesy. For the political aspect the scientist’s views cannot claim greater attention than those of other citizens. If the tests cause benefits the harm also done must be evaluated in relative terms.” On nuclear war, however, Stern pleaded: “It is to mankind’s shame not to yet have banished even the thought of war. Schweitzer’s ‘anguish in my heart’ must be felt by all of us. The scientists cannot cease to tell humanity the terrible truth about nuclear war, but the [atomic] tests themselves pose much lesser problems” (“One Scientist Speaks Up,” *Science* 120, 31 December 1954, p. 5A).

nuclear warfare and because Muller had already given a copy to the *Bulletin of the Atomic Scientists* for publication” (p. 267). In his lecture, Muller directly attacked Wallace’s experiments on heterosis, the AEC’s position on fallout hazards, and the view that no genetic damage resulted from the atomic bombings of Hiroshima and Nagasaki. However, Muller did not yet call for a test ban;<sup>203</sup> rather, he opposed it and characterized such calls as “defeatist propaganda.” But as far as the genetic damage from testing was concerned, Muller believed society had to “recognize the truth, to admit the damage, and to base [its] case for continuance of the tests on a weighing of the alternative consequences” (Muller 1955a, p. 839). Clearly, by late 1955, the biological effects of fallout, and hence the wisdom of atomic testing, were scientific and political issues that had engendered international controversy.

### **Quantifying the Biological Effects of Radiation**

The narrative to this point indicates clearly that by late 1955 concerns over the genetic effects of radiation from fallout had become intertwined with disputes in the genetics community on how to interpret the experimental data of population genetics. In that year, Dobzhansky (1955) labeled the opposing positions the “classical” and the “balance” positions, thereby suggesting that Muller’s and Crow’s views were outdated, while his own position—supported by the experiments of his graduate students, including Bruce Wallace—was at the cutting edge of genetics. As indicated above and shown in detail in the sections below, while the U. S. government took quite seriously the possibility of hazards from the genetic effects of fallout, after *Bravo* and the eruption of the public debate over fallout, the AEC and the White House sought to downplay any possible genetic (and somatic) dangers by utilizing and exploiting the epistemic uncertainty embodied in the classical/balance controversy. They did this in at least two ways: first, by initiating a public relations

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<sup>203</sup> Carlson (1981) gives an overview of Muller’s view on atomic testing in the period 1953 to 1955 (pp. 354-6). Muller then supported atomic testing; he believed that the “destruction of civilization would be much more likely to occur if such weapons as the atom bomb were *not* developed by scientists in the United States, since they surely would be developed by those in the Soviet Union anyway and in that case would be used, either directly or through the pressure created by the threat of their use, to accomplish the enslavement of all mankind” (quoted on p. 354).

campaign in which top AEC officials used distorted versions of the balance view to argue the harmlessness or even genetic benefits of fallout. Second, they sought to dominate all the relevant committees, government and independent, that were formed to study and/or recommend policy on radiation exposure limits. Their goal in these efforts was to justify scientifically the (policy) position—that atomic testing was safe—through public relations, interaction with scientists, and participation in scientific practice.

I aim to show in this narrative that utilizing the principles of the technological infrastructure of science to construct this historical narrative will result in a narrative in which many of the dichotomies of modernity—epistemic/political, nature/normativity, discursive/material, theory/experiment, and power/knowledge, for example—break down and are revealed as problematic oppositions. To take one of these as an example, the epistemic concerns of scientists, in their practices, are intertwined with the political, when considered from the traditional perspective of their practices as scientists (e.g., designing and interpreting experiments), from the perspective of their practices outside the laboratory (e.g., serving on policy committees or interacting with other scientists), and from a perspective that considers both of these perspectives as relevant to interpreting *practice*. How to design and interpret experiments using *Drosophila* to answer questions related to the genetic structure of populations; which theoretical model to use and why; who advocates that theoretical model and what is their standing within the scientific community; how to plan one's future experiments in the context of developments in genetics; who are one's competitors; what scientific journals should one publish in, who will peer review, and what are the standards of the review process; where and how to get one's funding; where is one employed and what are the requirements for maintaining that employment—these are all questions that relate to the practices of geneticists, and they all point to the technological infrastructure making possible the doing of science. Hence, there is no *a priori* reason why we should not consider them all as related components of practice.

Furthermore, if the historical evidence can reveal these practices as intertwined and interrelated, while the historian maintains a reverence for truth, then the traditional notion of the scientist *qua* scientist as distinct from the scientist as person or citizen, is problematic;

this distinction is traditionally made or assumed just so the epistemic and political are bifurcated, thereby upholding epistemic sovereignty. The distinction is problematic because traditionally epistemic decisions can be made, at least in part, for political or value-laden reasons; conversely, political decisions can be made for traditionally epistemic reasons. Moreover, such practices can be interpreted in this manner, even if the norms operating are not consciously held by the historical actors, for it is their practices, their actions that matter, *including* their discursive articulations, which themselves are practices. And while these discursive articulations ought not be evaluated stripped of their material contexts—hence the problematization of discursive/material—they are nevertheless subject to interpretation using norms and values the historian gleans from the historical record. What the historian should aim to find, then, are how and why the practices of scientists in intervening and interacting with the material (i.e., experimental) world of his/her surroundings, reveal what norms were operating that account for the scientific and cultural changes that one is investigating. In the present story, the main norms (used to mean usual practices) are: the prospects for future research; creating confusion on scientific/epistemic matters to support the argument of a particular scientific/policy position; and selectively searching, fitting, and interpreting scientific evidence in order to support previously held scientific/policy positions, especially those that are not testable (i.e., in the sense that the adaptationist program is not “testable,” as explored in Chapter VI above) in practice. These norms can help to account for the actions of scientists as policy-makers and as scientists, and for how the narratives in which those actors were embedded, changed.

### I. *The UNSCEAR Report*

Shortly after the August 1955 Geneva Conference, which further polarized the genetics community when Muller’s paper was banned, the United Nations established the Scientific Committee on the Effects of Atomic Radiation (UNSCEAR). Shields Warren, a Harvard pathologist and former Director of the AEC’s Division of Biology and Medicine (DBM) and now on the Advisory Committee for Biology and Medicine (ACBM) of the General Advisory Committee (GAC) of the AEC, was the U. S. delegate. Merrill Eisenbud, a



health physicist with the AEC's New York Operations Office, and the physician Austin M. Brues of the Argonne National Laboratory, were the alternate delegates. With the chemist and AEC Commissioner Willard F. Libby and AEC Chairman Lewis Strauss, these men were the AEC's chief spokesmen on fallout hazards; they distrusted geneticists and consistently argued that the biological effects of fallout were negligible.<sup>204</sup> As Beatty (1987) indicates, adherents to both the balance and classical views of genetics were among the consultants to the U. S. delegation; these included Dobzhansky and Crow. The first UNSCEAR session was held in March 1956, but the genetic effects of radiation were not taken up until the third session, held in April 1957. In the committee's deliberations, the balance view was given serious consideration (Beatty 1987, pp. 307-11). Clearly, both Warren and Eisenbud, neither of whom were geneticists, received first-hand exposure to the classical/balance debate. The position of the U. S. Delegation to UNSCEAR was "that while fallout makes a negligible contribution to dose rates, this source of irradiation can be expected to increase with time."<sup>205</sup>

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<sup>204</sup> Warren (1898-1980) was an undeviating champion of atomic energy in all its forms (see, for example, Warren 1953, 1972). As a Naval Officer after World War II, he went to Japan to study the health effects of the atomic bombings (Warren and Draeger 1946); he was also involved with Operation Crossroads, the two atomic tests in the Bikini Islands in 1946 (Draeger and Warren 1947). In his testimony before the JCAE in 1957, Warren stated: "I firmly believe as a physician that it is inexcusable for us to jeopardize our own safety and that of the rest of the free world in order to eliminate a risk of as low an order of magnitude as is constituted by any reasonable program of atomic weapons testing" (JCAE 1957, p. 1418). In 1956, Warren argued that if the rate of atomic testing were continued for thirty years, background levels would not rise appreciably and the genetic hazard would be insignificant (*New York Herald Tribune*, 18 October 1956). Ralph Lapp, in his testimony before the JCAE in 1957, accused AEC spokesmen of making "reckless" statements regarding the health effects of fallout. In particular, he focused on a comment Eisenbud (Director of the AEC's New York Operations Office, in charge of collecting fallout data) made to the *New York Sunday News* on 20 March 1955: "The total fallout to date from all tests would have to be multiplied by a million to produce visible deleterious effects except in areas close to the explosion itself." Using Eisenbud's own calculation of 10 milliroentgens as the dosage from fallout in Troy, NY, Lapp multiplied this by  $10^6$  and got 10,000 roentgens. Senator Anderson correctly noted: "10,000 roentgens would kill everybody in sight" (JCAE 1957, p. 1277).

<sup>205</sup> "Official Report of the United States Delegation to the U. N. Scientific Committee on the Effects of Atomic Radiation [UNSCEAR]," Alexander Hollaender Papers, APS. This report, written after UNSCEAR's second session from 22 October to 2 November 1956 (the first session was 14 to 23 March 1956), stated that "it was generally agreed that the tissue dose due to external gamma irradiation from radioactive fallout created in past weapons tests, does not represent an appreciable hazard to man since it is small by comparison both with the natural radiation background and with the variations in the latter." It did note, however, that "radiological hazards to man may eventually result from the fallout of Sr 90, and first in areas where the natural strontium or calcium content of the soil is low." By this session, the committee had not considered the genetic effects of radiation, and "decided to postpone detailed discussion until its next session in April [1957]. . . . The committee hoped that . . . certain preliminary recommendations relating maximum permissible levels of radiation exposure of populations to genetic considerations would be made." Hence, by the time BEAR I was submitted and made public in 1956, UNSCEAR had not even discussed genetic

After the report was published in 1958 (UNSCEAR 1958), it generated controversy among geneticists, especially those who adhered closer to the classical line. For example, William L. Russell, a mouse geneticist with the Biology Division of the Oak Ridge National Laboratory, wrote U. S. UNSCEAR Delegate Shields Warren (at Warren's request and at the request of George Beadle, Chairman of the Genetics Committee of BEAR II) that his and his colleagues' work was "completely ignored" and that what little *was* considered was "misused" in the report. After listing the major research results that their mouse data<sup>206</sup> indicated regarding radiation hazards in humans, Russell complained:

It has been a disappointing surprise to find that not a single one of these [results] is mentioned in the version of Chapter G [of the UNSCEAR report] prepared by the Secretariat. It is especially surprising in view of the fact that every one of the topics with which these points deal *is* discussed. Some of the conclusions, for example, that the genetic effects will be obtained at low doses and that they are cumulative, would have been strengthened by citing our results. Other statements, such as the treatment of quantitative characters, particularly in so far as life-span is concerned, are in direct conflict with our findings. In view of this, and of the fact that other results are cited, it can hardly be argued that there was no room to include any of these findings in this Chapter. (Russell to Warren, 9 January 1958, Alexander Hollaender Papers, APS)

Russell presented Warren with a point by point critique of the UNSCEAR report, stating: "Many times I have felt the U. S. has not put its best foot forward on the genetic hazard." Clearly, Russell was disappointed not only in how his own work was ignored, but also in

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considerations, and had not deemed the internal exposure of humans to Sr-90 to be a significant hazard. The UNSCEAR report was not completed and made public until 1958.

<sup>206</sup> Among the mouse data Russell cited that he considered relevant to "the human genetic hazard problem" was that the "induced mutation rate in mice is about  $25 \times 10^{-8}$  per roentgen, per locus" and that this "is about 15 times higher than the rate obtained in comparable experiments with *Drosophila*." In addition, there "is no recovery from genetic damage with time after irradiation. Offspring conceived long after exposure of the father to radiation are just as likely to inherit induced mutations as those conceived a few weeks after exposure. This principle is thus established for the first time by direct experimental evidence on the material in which the investigation was badly needed, namely, the spermatogonia of a mammal." He continued: "More than half of the radiation-induced mutations obtained have proved to be recessive lethals"; that "[m]ost of these lethals kill at times that would be considered tragedies in human experience" and that the "deleterious effect of some of these lethals in heterozygous state is large enough to be detectable in the individual"; that "in all three large-scale experiments at different doses the survival to weaning age in the offspring of irradiated males is lower than in the controls"; that there "is a significant shortening of life in the offspring of irradiated males"; that "there is no question in our minds that there will be genetic effects in [spermatogonia] from doses as low as [2 or 3r]"; and that "[o]ur data provide the only extensive information in any organism on mutation in spermatogonia, the cell stage that is important in man" (Russell to Warren, 9 January 1958, and "Specific comments on the UN Scientific Committee's document: Chapter G, Genetic Effects of Radiation (A/AC.82/R.61)," Alexander Hollaender Papers, APS).

how the report made the United States look regarding the genetic dangers of fallout.<sup>207</sup>

Russell's dissatisfaction with Warren's actions reveals several points. First, even though he was employed by the AEC, the epistemic significance of his work mattered to him, despite whatever loyalty he might have had to his employer. It really bothered Russell to see his work ignored, and to see a viewpoint put forward in the UNSCEAR report that conflicted

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<sup>207</sup> Russell's complaints reflect concerns for the significance of his and his colleagues' mouse data on the problem of the genetic effects of radiation, for how his data was used in relation to how data from other (esp. *Drosophila*) geneticists was used (e.g., in the context of the classical/balance controversy), and for the political and policy ramifications of his data. The BEAR I report (Russell was on the Genetics Committee) had already been published in 1956 and Russell had already testified at the 1957 JCAE Fallout Hearings (regarding how geneticists were being criticized for interpreting their data and on the lack of recovery over time after irradiation; see below). Both the BEAR I report and the 1957 JCAE hearings were dominated by the classical position, while the first Atoms for Peace Conference in Geneva in 1955 (where Libby banned Muller's [1955a] paper and Wallace [1955] gave a paper) and the U. S. UNSCEAR delegation (Dobzhansky was a consultant), both controlled in large part by the AEC, were dominated by the emerging hardened balance position. Although Russell was employed by the AEC, he was nevertheless concerned for the future of his mouse work, for his reputation, and for the policy implications of his work. He emphasized that the mouse work was the only significant work done on mutation rate in spermatogonia, which he believed was superior to and more significant than the work done in spermatozoa in *Drosophila*. In his letter to Warren (*op. cit.*, note 29), Russell cited Muller's WHO paper on lack of repair after irradiation, and his position in his critique of the UNSCEAR report is in accord with Muller's classical position: "The idea of heterozygosity per se at many loci is, in the first place, not *necessary* to account for reproductive fitness and flexibility. In the second place, what is the evidence for 'advantages of the heterozygous state' so far as radiation-induced mutations are concerned? Our data . . . , which are not even mentioned, would indicate just the opposite: it is presumably the heterozygotes in our experiments which show the shortened life-span. Furthermore, of the induced mutations obtained at specific loci in mice many, perhaps most, and certainly all of them at the locus that has the highest rates, are deleterious in the heterozygous state" ("Specific comments," *op. cit.*, note 29).

However, in a paper published later that year, Russell again emphasized the significance and originality of his mouse work by differentiating aspects of his work from Muller and his classical line (W. L. Russell, Liane Brauch Russell, and Elizabeth M. Kelly (1958), "Radiation Dose Rate and Mutation Frequency," *Science* 128: 1546-50). Central among these differences was the focus on detecting mutations in mammalian diploid spermatogonia, which are constantly dividing mitotically and presumably subject to greater potential damage from radiation, rather than the classical focus (he cites Muller on p. 1547) on haploid mature sperm in *Drosophila*, or spermatozoa. Russell *et al.* found that when spermatogonia are considered, the classical view that the frequency of induced mutations is independent of dose rate does not hold, and that chronic low-level gamma irradiation is apparently not as great a danger to humans as larger acute exposures of x-rays (although the data confirmed the classical results for spermatozoa). His conclusion, however, was that "it should not be forgotten that even the lower mutation rates obtained with the present intensity levels are still appreciable" (p. 1550).

Regarding Russell's concerns for the policy ramifications of his work, his concern was reflected in his participation in BEAR I and II and his testimony critical of the AEC at the 1957 JCAE Hearings on Fallout. In his letter to Warren regarding the 1958 UNSCEAR report, Russell complained: "It seems to me that the unreasonable propaganda about the genetic dangers of fallout could have been neatly turned against the U.S.S.R. in particular by asking: 'Which Country discovered this danger first, and the danger from other sources of radiation? Which Government first supported experiments on this danger? Where is the largest amount of work on this subject being done? What country has succeeded in obtaining essentially the only results on mammals and has also accumulated over the years the most extensive data on other organisms?'" (Russell to Warren, 9 January 1958, Alexander Hollaender Papers, APS).

with his more classical genetics perspective. In the same letter, however, Russell also expresses his dissatisfaction with how Warren made the U. S. look by presenting an effectively one-sided and distorted view of the genetic effect of radiation (see fn. 30 above). Clearly, Russell's letter reveals concerns that are epistemic and political, scientific and policy-oriented; he makes little effort to separate them. It also shows his concern for the future of his own work, and how that fits in to not only the disputes embodied in the classical/balance controversy, but also the political issues embodied in the controversy over the genetic effects of radiation.

## II. *From the Manhattan Project to the NAS BEAR I Genetics Committee*

In this section, I move back in time and outline efforts to study the genetic effects of radiation prior to the UNSCEAR committee. These efforts started in the Manhattan Project with the experiments of Curt Stern and ultimately made headlines with the efforts of the National Academy of Sciences (NAS) to tackle the problem. The NAS study was commissioned in early 1955 and released its first report in June 1956. Both Dobzhansky and Muller served on the Committee on Genetics of the NAS study on the Biological Effects of Atomic Radiation (BEAR I 1956, BEAR II 1960).<sup>208</sup> By the first meeting of this committee

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<sup>208</sup> The text of the summary report of the Committee on the Genetic Effects of Atomic Radiation of the National Academy of Sciences (1956), one of six reports appearing as the Study of the Biological Effects of Atomic Radiation (BEAR I 1956), was the first committee report of BEAR I published in *Science* ("Genetic Effects of Atomic Radiation," *Science* 123: 1157-1164). Geneticists who signed the report were George W. Beadle (1903-1989), James F. Crow (b. 1916), Milislav Demerec (1895-1966), H. Bentley Glass (1917-2005), Berwind P. Kaufmann (1897-1975), Clarence C. Little (1888-1971), Hermann J. Muller (1890-1967), James V. Neel (1915-2000), William L. Russell (1910-2003), Tracy M. Sonneborn (1905-1981), Alfred H. Sturtevant (1891-1970), and Sewall Wright (1889-1988). Dobzhansky, who was invited to be on the committee, but did not participate in the BEAR I meetings, did not have his name on the report. Others on the committee included Warren Weaver (1894-1978) of the Rockefeller Foundation, who served as chairman; the Columbia University biophysicist Gioacchino Failla (1891-1961); the radiation biologist Alexander Hollaender (1898-1986), Director, Division of Biology, Oak Ridge National Laboratory; and Shields Warren (1898-1980), a pathologist with the New England Deaconess Hospital and Harvard Medical School; other than Weaver, all these men were AEC personnel. The report recommended a "national system of radiation exposure record-keeping"; a "vigorous movement to reduce the radiation exposure from x-rays to the lowest limit consistent with medical necessity"; a 10 roentgen limit for the "general population," above background exposure, "of ionizing radiation as a total accumulated dose to the reproductive cells from conception to age 30"; that "the previous recommendation should be reconsidered periodically"; that individuals should receive not more than "a total accumulated dose to the reproductive cells of 50 roentgens up to age 30" and "not more than 50 roentgens additional up to age 40"; and that "every effort be made to assign to tasks involving higher radiation exposures individuals who, for age or other reasons, are unlikely thereafter to have additional offspring." In addition, the

in late November 1955, *Bravo* had exploded into the international spotlight, Ralph Lapp had begun his series of articles in *Bulletin of the Atomic Scientists* critical of the AEC, and Libby had banned Muller's paper at the first Atoms for Peace Conference. Although not a geneticist, Shields Warren also served on this committee (and was chairman of the pathology committee), as did the biophysicist Gioacchino Failla. In the 1950s, both these men, who served on a variety of organizations and committees regarding the setting of exposure limits from atomic radiation—including the NCRP, BEAR I and II, the AEC's Division of Biology and Medicine (DBM), UNSCEAR, the Advisory Committee for Biology and Medicine (ACBM) of the AEC, and others—consistently strove to minimize the health hazards of radiation (Seltzer 1993). After geneticists were included in the policy-making process in the 1950s, the classical/balance controversy became intertwined with the debates over the health hazards of radiation and fallout. However, well before the National Academy of Sciences became involved in radiation protection matters, the U. S. government sought out the expertise of many scientists on the biological effects of radiation, including expertise on the genetic effects of radiation.

Prior to the formation of the National Academy of Sciences BEAR I Genetics Committee in 1955, much of the effort to study the biological effects of radiation and recommend radiation exposure limits was controlled or sponsored by the U. S. government. Examples of this were the classified research programs of the Manhattan Project and its successor, the AEC, and the NCRP, which was a division of the National Bureau of Standards of the Department of Commerce, but was dominated by AEC personnel (Seltzer 1993, ch. II). There were, however, scientists who labored to educate their peers and the general public regarding the health effects of radiation. Foremost among geneticists who strove to educate the public on the dangers of radiation in the 1940s and 1950s were Curt Stern (1902-1981) and the 1946 Nobel Prize winner Hermann Muller (1890-1967), both *Drosophila* geneticists who had studied under Thomas Hunt Morgan at Columbia University,

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geneticists argued that since “[w]e badly need to know more about genetics—about all kinds and all levels of genetics, from the most fundamental research on various lowly forms of life to human radiation genetics”—it follows that “our society should take prompt steps to see to it that the support of research in genetics is substantially expanded and that it is stabilized” (p. 1164). Indeed, the geneticists saw their participation on this committee as a means to argue for further funding for their science, which was still seen as less well-established than other fields, including the medical and the physical sciences.

and who had longstanding interests in human genetics and its social implications.<sup>209</sup> In 1943, Stern became involved in the Manhattan Project research program investigating the biological effects of radiation at the University of Rochester, where he had been on the faculty since 1933 and was Chair of the Biology Department.<sup>210</sup> His classified work on mutation rates and the doubling dose in *Drosophila* from low levels of radiation suggests that the United States government took seriously—as early as 1943—the prospect that low levels of radiation, as might be produced by the atomic bomb and in laboratories, might be genetically dangerous.<sup>211</sup> Moreover, it was Stern who solicited Muller’s help in his Manhattan Project experiments, thereby bringing Muller and his crusade against radiation exposure into government circles.<sup>212</sup> Indeed, in 1943 Stern recruited Muller, while still at

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<sup>209</sup> Muller’s (1950) paper, “Our Load of Mutations,” published in the second volume of the *American Journal of Human Genetics*, and Stern’s (1949) textbook, *Principles of Human Genetics* (San Francisco: W. H. Freeman), became influential classics. Robert Kohler (1994) has provided an excellent analysis of Morgan’s laboratory and the advent of classical Mendelian genetics.

<sup>210</sup> All the experiments performed by Stern and his collaborators were initially classified and could not be discussed with geneticists who did not have a security clearance. The work was not declassified until after the AEC took over the Manhattan Project programs on 1 January 1947. These included: Ernst W. Caspari and Curt Stern [1947] “The Influence of Chronic Irradiation with Gamma Rays at Low Dosages on the Mutation Rate in *Drosophila melanogaster*,” AEC Report MDDC-1200, published in *Genetics* (1948) 33: 75-79; Warren P. Spencer and Curt Stern [1947] “Experiments to Test the Validity of the Linear r-dose/mutation-frequency Relation in *Drosophila* at Low Dosage,” AEC Report MDDC-765, published in *Genetics* (1948) 33: 43-47; and Delta Uphoff and Curt Stern [1947] “Influence of 24-hour Gamma-ray Irradiation on the Mutation Rate in *Drosophila*,” AEC Report MDDC-1492, declassified 3 December 1947. Stern wrote to Muller in late 1945 regarding the classification of his work, complaining that the “report brought out by the Man[hattan] Dept. does not give any information as to the results obtained in our fly and mice work.” He stated: “Personally, I am very unhappy about the present withholding of information which obviously has no military value, but there is nothing we can do just now” (Stern to Muller, 28 December 1945, Curt Stern Papers, APS).

<sup>211</sup> Ernst W. Caspari (b. 1909), whose previous genetics work focused on the mealmoth *Ephestia* and the mouse, came to Rochester in 1944 to work with Stern on his Manhattan Project work. In a draft of the history of the Biology Department at Rochester, Caspari wrote: “Stern, in collaboration with Warren P. Spencer, Ernst W. Caspari and Delta Uphoff, studied the mutation rate induced in *Drosophila* by low amounts of radiation. These investigations form the basis of our present knowledge of mutagenic radiation effects” (“History of the Biology Dept., Univ. of Rochester, 10-13-79, Draft by Caspari,” Ernst W. Caspari Papers, APS).

<sup>212</sup> In a detailed 5-page letter of 12 October 1943, Muller wrote Stern and Spencer, explaining his experiments on the spontaneous mutation rate in *Drosophila* in relation to what they were previously able to tell Muller about their own experiments. As Muller stated: “Inasmuch as you let me into your confidence about your work I think I should let you into mine too and explain that my primary object here is to test the possible effect of ageing, as occurring under several different conditions, on ‘spontaneous’ mutation rate.” Muller suggested to Stern that “perhaps some type of collaboration could be arranged, such as by my being asked to be an official ‘consultant’ (or whatever term would seem to you most suitable), whereby my results, stocks, and techniques, in so far as they might be useful to you, could be placed freely at your disposal and included in your reports.” Muller also complained about the lack of awareness among radiologists about the effects of radiation

Amherst College in Massachusetts, to be a consultant to his Rochester research program.<sup>213</sup>

In 1946, the National Committee (later Council) on Radiation Protection (NCRP) was formed from its predecessor, the Advisory Committee on X-ray and Radium Protection. This reorganization effectively negated this committee's prior independence from governmental influence by including AEC and military members on its subcommittees (Seltzer 1993, ch. II). Stern and Muller, along with the mouse geneticist Donald R. Charles, served on the subcommittee on permissible external dose, which was chaired by Gioacchino Failla, and which was charged, among other things, with the responsibility of considering the genetic effects of radiation.<sup>214</sup> The first meeting of this subcommittee was in June 1948, after Stern had left for the University of California at Berkeley, and after Muller had gone to Indiana University and won the Nobel Prize. The last meeting was held in December 1952, but

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exposure, a theme that ran throughout his career: "I do hope . . . that your work goes well, and that you finally succeed in waking up radiologists to what we know in advance to be the case—that for mutation it is only the total number of  $r$  (i.e. time times intensity) which count, quite regardless of time *or* intensity alone. The J.A.M.A. in an editorial a few months ago tacitly assumed the contrary, and I had a fight about the matter in Britain, in getting my last report . . . through the editorial board of the Medical Research Council. . . . It is a disgrace, in fact a large-scale crime on the part of the medical radiologists, that after all these years it should not yet be known, but you are I hope in a position now to see that this question is properly settled, even in mammals" (Muller to Stern and Spencer, 12 October 1943, Curt Stern Papers, APS).

<sup>213</sup> On 5 November 1943 Stern wrote to Stafford Warren, asking that Muller be "admitted in some kind of consulting capacity" to the research project, admitting that "[i]n order to conduct our project here we had to get in contact with Professor Muller telling him in some very general way what we were doing without, of course, giving away secret information. As a result of these contacts, Professor Muller has provided us with some extremely valuable stocks of fruit flies which are being used as working tools. It appears that Professor Muller may have information which would save us a very great amount of work. Furthermore, he is handicapped in advising us regarding stocks by the fact that he cannot be fully informed of our problem" (Stern to Stafford Warren, 5 November 1943, Curt Stern Papers, APS). Stern wrote to Muller, informing him of the official request (Stern to Muller, 8 November 1943, Curt Stern Papers, APS), and in December, the Director of the Manhattan Department at Rochester, Andrew H. Dowdy, MD, wrote Stern to give him permission to inform Muller that Stern's "work is being done in part as an O.S.R.D. project, and that any information so discussed is of a restricted nature, [and that] we in turn shall be able to clear Professor Muller in the usual manner." Dowdy also informed Stern that his secretary would have to get a security clearance (Dowdy to Stern, 7 December 1943, Curt Stern Papers, APS). In late December Stern officially informed Muller of his project and asked him to come to Rochester, expenses paid by the government, to give a talk (Stern to Muller, 27 December 1943, Curt Stern Papers, APS), and in late January 1944, Stern wrote Muller to tell him his security clearance had been approved (Stern to Muller, 29 January 1944, Curt Stern Papers, APS).

<sup>214</sup> The subcommittee on permissible internal dose was chaired by Karl Z. Morgan (1908-1999), chief health physicist for the Oak Ridge National Laboratory. Other members included: Gioacchino Failla, of the Radiological Society of North America and chairman of the external dose subcommittee; Hermann Lisco and Austin Brues, Biology Division of the Argonne National Laboratory; Joseph Hamilton, University of California Radiation Laboratory; and Shields Warren, Director, Division of Biology and Medicine, AEC (Handbook 52, 1953).

because of disagreements over how the genetic effects should be incorporated into the subcommittee's recommendations, the report was not released to the public until 24 September 1954, after the *Bravo* shot, but before the National Academy of Sciences had formed BEAR I. Despite heated disagreements and intervention by Lauriston S. Taylor, the Chairman of the NCRP, Failla's relentless dismissal of the geneticists' arguments prevailed, and the final report effectively disregarded the genetic effects of radiation, including population exposure considerations, as legitimate factors in the setting of permissible exposure limits (Seltzer 1993, ch. II).<sup>215</sup>

Stern's participation in the government's attempts to gauge the effects of atomic energy and warfare on the human genome continued into the 1950s. In June 1950, Stern replaced George Beadle on the AEC's Advisory Committee for Biology and Medicine (ACBM), a position he held until 1955. Prior to this, Stern was asked to be on an AEC ad hoc committee investigating the genetic effects of atomic energy. The committee, which included Beadle, Russell, Neel, and Max Zelle, met in March 1950 to answer three questions posed by Shields Warren:<sup>216</sup>

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<sup>215</sup> Testifying at the 1957 JCAE fallout hearings, Muller stated: "The grounds for the reduction in permissible dose that was made by the committee [NCRP] a few years ago, prior to the issuance of the National Academy's report [BEAR I 1956], did not lie in considerations of genetic damage. For the permissible dose handbook [Handbook 59, 1954] specifically stated that this dose (of 0.3 roentgens per week) was set without regard to genetic effects. The geneticist members objected to that but it was carried anyway. In other words, it was known that the dose was considered too high on genetic grounds but, it was adopted in spite of this, although it was acknowledged that it might be reduced again later" (JCAE 1957, p. 1062). Muller added: "[T]he record of this committee's [NCRP] decisions on the permissible dose, which as Dr. [Lauriston] Taylor [NCRP Chair] presented the matter yesterday appeared to show that they were so cautious, in actuality showed that the first dose they set was far too high, so that they had to set it lower. . . . This does not indicate that they have been so cautious. It means they have not been cautious enough. The geneticists would not have set so high a permissible dose in the first place, on the basis of what we knew 30 years ago" (*ibid.*). Taylor (1902-2004) later authored a book on radiation protection standards (Taylor 1971); he also contributed to its history (Taylor 1979, 1980).

<sup>216</sup> "Report of the ad hoc Committee to Evaluate the Effects of Atomic Energy on the Genetics of Human Populations," Curt Stern Papers, APS. Even earlier than this, in June of 1947 while he was still at Rochester, Stern was asked to be on the National Research Council's (of the NAS) Committee on Atomic Casualties, the forerunner of the Atomic Bomb Casualty Commission (ABCC), which studied the medical and genetic effects of the atomic bombings of Hiroshima and Nagasaki (see, for example, Beatty 1991, 1993 and Lindee 1992, 1994). Lindee (1994) shows that while the ABCC was under the control of the NAS and its advisory committee (the Committee on Atomic Casualties), the AEC's Division of Biology and Medicine, and especially Shields Warren, "took an active interest in the day-to-day administrative and scientific business of the ABCC" (pp. 104-7, quotation from p. 107). Stern's first graduate student, James V. Neel, then a Lieutenant in the Army Medical Corps, became one of the leaders, with William J. Schull, of the genetic studies on the



I – Assume a single exposure of 50 r to a group of 500 young males. What would be the probable effect on their first generation offspring and on their own fertility?

II – Assume a group of young males, 100,000 in number, receive 10 r per 8 hour day. For how long could they receive this exposure without causing significant genetic changes?

III – Assume a mixed group of  $10^6$  or larger. How long could they receive a dose of 1 r per 24 hour day without significant genetic effects?

In answering these questions, the geneticists emphasized that “any exposure above background will cause mutations” and that any standards adopted should be “cautious” initially, yet “might later be relaxed in the light of new data. . . .” They concluded that “it is unwise to attempt to formulate quantitative answers to the above three questions.” Nevertheless, “emphasizing again that the following are at best little more than guesses based upon inadequate data,” they answered: On question I, the exposure “may lead to a total of 25 deaths and many more cases of slighter damage in future generations,” but with no permanent effect on fertility. On question II, they answered 2.5 days, and on III, 25 days. Clearly, the geneticists believed the effects of even low doses of radiation were not negligible and would be, on the contrary, quite limiting. The available data, however, they described as meager and inadequate, and they called for more research funding for genetics (“Report of the ad hoc Committee. . .,” *op. cit.*, note 216).

Later in 1950, after Stern was appointed to the ACBM, he was again asked by the AEC to participate in a meeting, this time to estimate the radiation effects on military personnel in a nuclear war. Stern was the only geneticist present, presumably because of his Rochester work and because a security clearance was required; the remainder of the members were mostly radiologists and pathologists, all with ties to the AEC, including Shields Warren, Andrew Dowdy, and Austin Brues, with representatives from the Army, Navy, and Air Force also present. One question the committee was asked was: “Assume that troops are acutely exposed to penetrating ionizing radiation (gamma rays). At what dosage level will they become ineffective as troops?” The committee’s answer was approximately 150r.<sup>217</sup> No mention was made of the possible genetic effects of radiation.

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ABCC. However, Stern declined to participate, citing previous commitments (Stern to Weed, 11 June 1947, Curt Stern Papers, APS).

<sup>217</sup> “Draft Letter—12/18/50 [classified as “CONFIDENTIAL”],” Curt Stern Papers, APS.

Clearly, the efforts of these committees further support the notion that the government and military were aware of the potential biological effects of radiation, including genetic effects. The U. S. government sought the expertise of the best geneticists in the country to answer questions related to the genetic effects of radiation. Nevertheless, with the public controversy over radiation hazards, it became the undeviating goal of the AEC to justify scientifically the position that fallout and low levels of radiation pose no threat to humans, and might even be beneficial genetically.

### III. *The Genetics Committee of BEAR I*

As indicated above, by the time of the first meeting of the BEAR I Genetics Committee in November 1955, the controversy over the hazards of fallout from atmospheric atomic testing had reached international proportions. AEC Chairman Lewis Strauss, believing the AEC's position on radiation hazards was scientifically supported, had requested the NAS study in early 1955 (Mazuzan and Walker 1984, p. 44). By this time, it was known that radioactive strontium-90 from fallout had made its way into the environment. Chemically similar to calcium, and thereby posing a risk to humans by concentrating in certain crops, in the milk supply, and ultimately in human bones, Sr-90 was seen by some scientists as a very dangerous by-product of the atomic age.<sup>218</sup> Moreover, as at least some geneticists, such as Stern, were consulted by the AEC on the genetic effects of radiation, and given that the AEC had research programs on the genetic effects of radiation at many of its national laboratories, it is clear that the U. S. government saw the genetic effects of atomic energy as more than just an unsupported theoretical possibility.

The first meeting of the BEAR I Genetics Committee was held at Princeton University on 20-22 November 1955.<sup>219</sup> One point of contention that arose was the problem

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<sup>218</sup> See, for example, Commoner (1971). The AEC's Project Sunshine (AECU-3488), which was designed in part to assess the problem of Sr-90 in the environment, was started in 1953 (see the section on the FRC below for more on Project Sunshine). Clearly, Libby and others in the AEC knew of the potential problem, and began to take steps to assess its magnitude at least as early as 1953. Moreover, the Troy, NY rainout occurred in early 1953, and the chemist Clark (1954), who detected it, was published quickly.

<sup>219</sup> The geneticists present included Beadle, Crow, Demerec, Glass, Muller, Neel, Russell, Sturtevant, and Wright. Dobzhansky and Stern were invited but could not attend. Nongeneticists included the chairman, Warren Weaver, Detlev Bronk, Failla, Hollaender, and Shields Warren. Glass was the rapporteur ("Genetics

of extrapolating data from animals to humans. First raised by Sturtevant, the problem provoked a curious response from Shields Warren:

**Sturtevant:** We should consider the problem of extrapolation to man from other organisms and the inability of the public to see the relevance of data on the latter.

**Warren:** First, in regard to information in the biomedical field, what secret classification there is is related to dosages, etc. Little would be added by complete declassification. Secondly, ovaries are more protected than testes, so the male is somewhat more exposed to radiation. The somatic recovery of bomb victims seems to be quite complete. Epilation of bomb victims was restored in a year or a year and a half. This striking capacity to recover is why it is so hard for a pathologist to keep in mind that there is no recovery from genetic damage. It is very hard for the A.E.C. to find an adequate supply of interested and qualified personnel to study these problems. (“*First Meeting*,” p. 2, *op. cit.*, note 219)

Warren moved from a specific problem raised in population and radiation genetics, to a statement that implied that future incidences of genetic damage were not as well-established as somatic ones, and that repair of genetic damage was still a viable theoretical possibility. However, he later admitted that for the exposed populations in the Pacific tests, there was “a higher incidence of leukemia and an indication of a higher death rate among the exposed than among the unexposed population” and that physicians had a higher incidence of leukemia than the general population, while “among radiologists it is 5 to 15 times higher than in other physicians” (*ibid.* p. 3).

Later, Muller weighed in when the subject of the public’s reception of the genetics report was discussed:

**Muller:** . . . Of the short-term aims, the most important may be to find the permissible dose. I don’t like setting a limit, but it seems to be a practical necessity. What factors should determine it? Measured in days of life lost, a given dose is likely to cause a greater loss to later generations than to the generation that is exposed. If that is so, the genetic damage must be taken into account in determining our practices, inexactly as we know the genetic effects. Peacetime uses should be especially important in determining the permissible dose. . . . We should beware of reliance on illusory conclusions from human data, such as the Hiroshima-Nagasaki data, especially when they seem to be negative. We need to emphasize the importance of education as to the genetic effects of radiation among those whose words carry

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Panel, National Academy of Sciences Committee to Study the Biological Effects of Atomic Energy, *First Meeting*,” Alexander Hollaender Papers, APS).

authority, such as physicians. For example, I learned that in a four-week course given on various aspects of radioactivity to 32 foreign students, mostly physicians, at Oak Ridge [National Laboratory], not a word was said in regard to any genetic effects, although other health effects were stressed. (*ibid.*, pp. 5-6)

The striking differences in the emphases of the AEC pathologist Warren, as compared to the Indiana University geneticist Muller, underscores the perceived status of genetics as a “soft” science among those in the medical professions. It must have seemed curious to the Nobel laureate Muller, if not humiliating, to have someone serve on a panel of geneticists who was not trained in genetics and who believed the genetic effects of radiation to be negligible, ostensibly because they were not immediately visible, and for him to imply that the science of genetics was somehow inferior to the medical sciences.<sup>220</sup>

In addition, Warren’s presence on the genetics committee and his actions toward undermining any arguments suggesting possibly significant genetic damage from low levels of radiation, underscores again the need to characterize scientific practices in a way that resists the dualism inherent in epistemic/political, for example. The members of the committee were charged with making recommendations based on the best available evidence on the genetic effects of radiation. Even without nongeneticists, it is difficult to see how the geneticists’ actions could be seen as apolitical or purely epistemic, given the uncertainty

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<sup>220</sup> In 1956, Warren argued that if the rate of atomic testing were continued for thirty years, background levels would not rise appreciably and the genetic hazard would be insignificant (*New York Herald Tribune*, 18 October 1956). Lindee (1994) documents how the AEC—and Shields Warren in particular—were intimately involved in the administrative and scientific affairs of the Atomic Bomb Casualty Commission (ABCC), even though the ABCC was under the control of the NAS. According to Lindee (1994), “AEC officials sometimes made direct requests of ABCC scientists to publish results immediately or to ‘refute’ reports of radiation effects in the popular press, and in at least one case, an AEC official asked Neel to report directly to the ACBM rather than the NRC Committee on Atomic Casualties.” (p. 107) Beatty (1993) shows that the ABCC was designed “from its inception to help align Japan against Communism” (p. 230). Late in his career, after the ABCC’s responsibilities for tracking and documenting the health effects of the bombings of Hiroshima and Nagasaki were transferred to the Radiation Effects Research Foundation (RERF) in 1975, Warren (1977) finally admitted the “late radiation effects among survivors” of the atomic bombings (pp. 98-9). These included an “increased incidence of leukemia, particularly myeloid leukemia, which appeared early and apparently reached its peak between 1955 and 1962” (p. 98); an increase in “cancers of the thyroid, female breast, and the lung” (p. 99); and even an “increase in the incidence of chronic leukemia at lower doses” (p. 99). However, Warren did not change his views on the genetic effects; for the children of survivors, Warren stated that the ABCC studies “did not demonstrate . . . any . . . genetic effects from the radiation exposure.” For later generations, “studies are underway . . . to determine the effects, if any, in the second generation. . . . Research has now shifted to the molecular level, and the composition of the blood proteins of children in the second generation is being studied” (p. 99).

inherent in the classical/balance dispute and the underdetermination of any interpretation of the experimental evidence by the available data. Moreover, the geneticists based their recommendations in part on traditionally nonepistemic factors—such as concerns regarding future developments in atomic industries—as Failla would eventually charge at the second meeting of the committee.

Later in the discussion, there was considerable debate over whether to base considerations of genetic hazards on effects to populations, or on effects to individuals, such as the offspring of particular atomic workers. Then, when faced with the question of whether humans might benefit from an increase in the mutation rate, the following exchange occurred, which included mention of Bruce Wallace's work on heterosis:

**Wright:** We ought to distinguish different levels of genetic effects: first, on individuals (somatic mutations); second, on descendants; third, on the general population. . . . Increased selection is good for the population, though hard on individuals. It would be distressing to increase by tenfold the frequency of some particular defect appearing at age twenty, but it would not be serious to the population. But genes of small effect that lower the general viability, intelligence, or fertility might be more serious to the population. There is danger in thinking of particular genes as either favorable or unfavorable. There is actually a need for slightly deleterious genes that may be worked over into a gene complex that as a whole is beneficial. This is probably the explanation of such results as B[ruce] Wallace's. Unfavorable genes have become favorable genes under the drastic selection for the better gene complexes. . . .

**Crow:** But not to the extent that unless you know the effect of a gene in all possible combinations it has just as good a chance of being beneficial as deleterious; or that a lethal might be useful under altered circumstances. And it requires selection to change the environment so that a price must be paid by individuals.

**Wright:** I don't deny that selection is distressing to individuals, but it is the most beneficial thing to the populations.

**Russell:** . . . I think we should be concerned primarily with the practical problems of the near future. Can we not ignore long-term population effects? If we found ourselves, population-wise, heading down a curve in reproductive capacity, etc., the decline takes so long—so many generations—that we could retreat after five or six generations by decreasing the radiation. And suppose that we learn to control fusions. Who knows what the general exposure to radioactivity from this will be? (*ibid.*, pp. 7-8)

Clearly, even though there was no firm proponent of the balance school on the BEAR I genetics panel, Wallace's and Dobzhansky's work was discussed, and Wright argued for the balance view; Glass later summarized their work for the panelists (p. 10). Moreover, the kinds of confusions and mistakes, as seem to be reflected in Russell's lack of understanding of population effects above, that had made their way into the AEC's arguments implying that mutations might be beneficial to humans (for example, the sensational reporting of Wallace's work in the media earlier in 1955), again cropped up in the panel's discussions at the first meeting. However, the perceived intractability of the problem at hand did not prevent the geneticists from reaching agreement, and Weaver was successful in allowing the geneticists to maintain control of the recommendations.

The next day, Weaver broke the geneticists up into two groups, one with Muller as chair, and the other chaired by Sturtevant.<sup>221</sup> They each were to come up with an answer to a question posed by Failla: "What items of biological information, given a particular dose, is it necessary to know in order to estimate the genetic danger to individuals and to the population from the effects of radiation?" (*ibid*, p. 11). By the end of the evening session, they had some tentative numbers for all their questions, including the spontaneous mutation rate in humans, permissible exposures for atomic workers, and exposure limits for individuals and populations. There did not seem to be much, if any, controversy over the recommendations:

**Weaver:** This reminds me of an experience in the War in handling antiaircraft fire control problems. At that time the existing information and the probability of getting hit involved factors of uncertainty much greater than these [of this panel]; and still, before it was over we began to shoot down some airplanes.

**Whitaker:** As a non-geneticist, I find the agreement is what is striking. (*ibid.*, p. 19)

The agreement of the committee, despite the acknowledged uncertainty inherent in the available data, suggests that this process was not apolitical. Indeed, on the one hand, the geneticists were worried about presenting too mild a case for genetic dangers, lest the classical view on genetic load be confirmed and serious dangers arise in the future. On the

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<sup>221</sup> Muller's group included the geneticists Beadle, Crow, Berwind P. Kaufmann, Neel, and Russell; Sturtevant's included Cotterman, Demerec, Glass, Tracy M. Sonneborn, and Wright. The nongeneticists could "sit in wherever they desired" ("Genetics Panel, *First Meeting*," p. 10, *op. cit.*, note 219).

other hand, it is clear that the nongeneticists, especially Warren and Failla, had as their motive just the opposite: they were worried that setting exposure guidelines too low would hamper atomic testing and emerging atomic industries, such as nuclear power.

At the second meeting of the Bear I Genetics Panel, held on 4-5 February 1956,<sup>222</sup> discussion again became centered on the notion that mutations could be beneficial to populations, and whether that consideration should have bearing on the panel's work. Wright presented the balance position on genetic homeostasis, arguing that "a change in the conditions of life may make plasticity through mutations more desirable" (*op. cit.*, note 222, pp. 9-10). When Weaver interjected and raised Muller's genetic load argument, suggesting that it was "rather easy and attractive" to say that the price paid for an increase in the human genetic load is worth it, the following exchange resulted:

**Wright:** I'm not advocating stressing that side of it under present conditions, but there is that added consideration.

**Russell:** Could we say that any *sizeable* increase in the amount of mutation would be harmful?

**Wright:** That gets you into a wholly impossible situation from the standpoint of evaluation.

**Russell:** Do you mean that a small increase might be beneficial?

**Crow:** No. I mean that even the smallest increase might be harmful.

**Beadle:** That is to say, we are at present not at [the] optimum [level of mutability], but already *above* it.

**Wright:** We are probably *below* it because we have had a hundred generations or less than that under our present conditions of life.

**Crow:** Are we at present above or below the optimum level of mutability? I would think we are above it.

**Sturtevant:** Where did you get that ratio of forty and sixty per cent [of beneficial to harmful mutations]? If I had been asked to estimate it, I would have said four instead of forty.

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<sup>222</sup> "Genetics Panel, National Academy of Sciences Committee to Study the Biological Effects of Atomic Energy, *Second Meeting*," Alexander Hollaender Papers, APS.

**Wright:** I pulled it out of the air.

**Sturtevant:** It seems to me to be very high.

**Wright:** It depends upon how big the mutations are you're talking about. . . . You might say that all mutations, perhaps theoretically close to 100 per cent, have a net effect that is injurious. But as the injurious effect becomes lower in amount, you get to a point where there is a balance between forty per cent beneficial and sixty per cent injurious effect among the individuals. . . .

**Crow:** I think, however, it's impossible to conceive of a change in environment in which the majority of the mutations would become beneficial in that environment, which is the assumption that you seem to be making.

**Neel:** No, it doesn't call for the assumption that all mutations can be beneficial, or even a majority of them. But there is no doubt of the fact that the selective pressures on man are now being completely redefined within a short period of time.

**Muller:** It seems to me that the important assumption in the preceding considerations is that human evolution is proceeding in a desirable direction at the present time. Maybe the pressure of selection as it exists now is in an undesirable direction. If so, an increase in desirable mutations would not necessarily have a desirable long-term effect. (*ibid.*, pp. 10-11)

Clearly, the balance viewpoint did receive consideration when Wright presented it in the context of the homeostasis argument. Nevertheless, at no time did any of the geneticists suggest that an increase in mutation rate from exposure to radiation would be a good thing for the human race. Yet there was much confusion, partly because of differences in opinion on the interpretation of scientific matters (i.e., epistemological matters), partly because of differences in training and emphasis (e.g., choice of experimental organism), and partly because of more traditionally ethical and political matters (e.g., whether to plan for the far future or not). But as George Beadle indicated, no geneticist was willing to support the contention, advocated publicly by the AEC, that increases in radiation would be good for the human race:

**Beadle:** I want to make one comment on Bruce Wallace's experiment, namely, that 98.8 per cent of the flies are eliminated in his population in every generation and that makes it an entirely different proposition from a human



population. I know everybody here knows this. (*ibid.*, p. 28)

The remainder of the discussion involved the problem of setting a cumulative dose limit for radiation exposure, including the proposal that all atomic workers carry radiation badges to measure this dose. The biophysicist Failla persistently argued against setting too low a permissible level in the light of genetic data, suggesting that it would hamper atomic industries (pp. 18-19, 25-26) and that it would cause “a division of opinion between geneticists and other groups. . .” (p. 24). Then Failla attacked the very idea that the geneticists were setting the cumulative exposure level on the basis of their own scientific expertise as geneticists:

**Failla:** Yes. My feeling is that this question of setting a total dose, that is, a cumulative dose, involves more than the genetic aspect. . . . Actually, the panel is not setting this figure on genetical grounds, for I have been told for the last 25 years that from the point of view of the geneticists, what matters is what the average dose for the population is, and that what one individual may get has nothing to do with the future of the race. You were worrying about the psychological impact on a man who may have abnormal children because of overexposure to radiation. For as far as the race is concerned, the few individuals who have received large doses are not going to change the picture. Certainly it isn't purely a genetic matter. You are getting a little beyond your field in recommending that the total dose should be not more than 50 rem in the occupational group. (*ibid.*, pp. 24-25)

Not surprisingly, the geneticists objected, with Beadle the most vocal. Weaver supported the geneticists, suggesting that the panel “pay attention to the importance and validity of the practical points that Dr. Failla has raised, but let's not feel they must stymie us” (*ibid.*, p. 26). Failla retorted, followed by Beadle:

**Failla:** Dr. Weaver, you are perfectly right that this question should be settled on scientific grounds irrespective of whether it can be administered or not. The administration should then be changed so that it performs. But the figure proposed has not been arrived at on scientific grounds. Other considerations have entered into the establishment of the figure.

**Beadle:** I was not impressed with the argument that the geneticists should not set a limit because it isn't exclusively their business. The pathologists may say, we don't have any scientific grounds to set a limit. . . . We're going to have to investigate them for ten years. So they will say we can not do it either, and then we end up by doing nothing. Yet I think there is an impressive body of evidence that says there is a substantial amount of danger from this level of radiation. We can't

positively prove it by any particular set of facts, but I don't want to sit here and say I'm not going to worry about it for the next ten years, because after that ten years it's going to be too late to worry about it. We will have an industry built up and based on the exposure of a substantial and increasing number of people to 500 r a year, and we've got to do something about it right now. (*ibid.*, p. 26)

All of the main points in the previous discussion—including the views that genetic damage is proportional to the mutation rate, that the total dose of radiation is cumulative, that the total cumulative dose is what is important from a genetic standpoint, that a “national system of radiation exposure record-keeping” be implemented, and the proposed total cumulative dose limit of 50 roentgens to age 30—were adopted by the panel and appeared in the final BEAR I report.<sup>223</sup> Thus was born the first radiation exposure limits that were set on the basis of the genetic effects of radiation, including population exposures.

Again, it is significant to note that the 1956 BEAR I report appeared less than a year after the fallout controversy became an international phenomenon. Despite the uncertainties in population genetics, illustrated in the polarization embodied in the classical/balance controversy and in the geneticists' disagreements over how to make interpretations on even basic matters in genetics, the panelists came to fairly quick agreement on exposure levels. It is difficult to characterize this agreement as a practice that is not political and value-laden, in addition to epistemic. In particular, it seems that we cannot separate geneticists' experimental practices from their other practices (e.g., ethical, political, etc.) and still tell a coherent story. Beadle's worry about future exposures to populations reflected a *real possibility* that something might happen (Rouse 2002a). That real possibility could not be reduced unproblematically to any experimental data in genetics, as they were contestable and contested; its orientation is futural, awaiting further developments and recurrence. In addition, Beadle's concern is thoroughly infused with value judgments that are inseparable from epistemic judgments. To separate them and evaluate them in isolation of each other, would be to distort the story in a way that privileges one over the other. Instead, they ought to be taken as inseparable; one does not make sense without the other. If it turns out in the story that more traditionally epistemic factors were *more important* than nonepistemic ones

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<sup>223</sup> Committee on the Genetic Effects of Atomic Radiation, National Academy of Sciences (1956), “Genetic Effects of Atomic Radiation,” *Science* 123: 1157-1164, quotation from p. 1164.

in explaining what happened, then the narrative should reflect that. Nevertheless, human practices that involve the investigation of natural/biological reality are always already in an historical/cultural context infused with values and politics of one sort or another. It is not that scientists are deceiving themselves and do not make contact with reality; they sometimes do, but the efficacy of such claims is *itself* futural, awaiting further developments. We should tell such stories by reconstructing narrative contexts that reveal *practices* expressing the irreducibly normative character of those practices—and this includes practices that aim to reveal physical and/or biological nature, i.e., those performed by scientists.

#### IV. *The Reception of BEAR I and the Genetics Committee of BEAR II*

After the appearance of the BEAR I report in June 1956, more controversy erupted when the report, which received much public attention in the press,<sup>224</sup> was interpreted by the Eisenhower Administration in a way that made it appear as if the National Academy of Sciences had minimized the biological effects of fallout.<sup>225</sup> An article in *The Washington Post* on 15 October 1956,<sup>226</sup> which suggested that the BEAR I committees had come to the conclusion that if fallout was increased by a factor of 10 it would still be safe, was blasted by

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<sup>224</sup> BEAR I panelist Milislav Demerec, Director of the Cold Spring Harbor Biological Laboratory (where Bruce Wallace worked until 1958), wrote to Warren Weaver after the report appeared and stated he “was very happy to see that the report of the Genetics Section received so much attention in the press. This amply proves the correctness of your judgment that effort should be spent on the preparation of a statement that could be read by the general public. I am very glad that you received the well-deserved credit, particularly after the many hard experiences you had with the members of the committee.” Demerec also indicated that criticisms of BEAR I by some geneticists for its not adequately considering the balance position, were not justified: “Recent laboratory experiments . . . [by] Bruce Wallace . . . strongly suggest that the effect of a mutant gene is considerably influenced by the genetic make-up of the organism which carries the gene, and that whether a mutation will be good or bad depends upon the particular genotype in which it happens to appear. This question was mentioned on several occasions during the discussions at the meetings of our committee. However, since this work is still very recent, and a number of technical problems have not been entirely solved, a majority of the members of the committee took a justly conservative view, which would be acceptable to most geneticists” (Demerec to Weaver, 21 June 1956, Milislav Demerec Papers, APS).

<sup>225</sup> A press release by the Federation of American Scientists (FAS) documented the controversy that erupted. It quoted extensively the administration’s release, which appeared in *The New York Times* on 24 October 1956, right before the presidential election, and indicated that it quoted “only those points which contain some new information or interpretation, and/or which raise technical arguments basic to the test-ban discussion.” The FAS release also provided a copy of Sturtevant’s Letter to the Editor of *The Washington Post* of 26 October, as well as an Editorial by the *Post* that discussed both Sturtevant’s letter and the White House’s release on the need for continued atomic testing and its supposedly negligible biological effects (Information Bulletin No. 82, Federation of American Scientists, 28 October 1956, Alexander Hollaender Papers, APS).

<sup>226</sup> “Ten-fold Rise in A-Tests Seen as Safe,” *The Washington Post*, 15 October 1956.

Sturtevant in a Letter-to-the-Editor on 26 October 1956:

I have just seen the news item in your issue of Oct. 15. . . . This account implies that the National Academy of Sciences Committee on the Genetic Effects of Radiation concluded that a tenfold increase in fallout would not be serious. As a member of that committee I wish to state that the report of the committee reaches no such conclusion, and that I, for one, would have been unwilling to sign a report that could have reasonably have been so interpreted.

Further, since the committee reported, [AEC] Commissioner Libby has indicated (Oct. 12) that the danger from radioactive strontium in fallout is greater than the information available to the committee led us to suppose. For this reason, our conclusions about the danger from fallout need revision upward. (*The Washington Post*, 26 October 1956)

The *Post* editors backed Sturtevant, and in an editorial in the same issue, the editors made it clear that the President's statement of 24 October was based in part on the BEAR I report, and that the *Post* news story of 15 October "seems to have been based upon misinterpretation of the words of an NAS staff member." The editorial also mentioned the statement of the Radiation Hazards Committee of the Federation of American Scientists, which indicated that strontium-90 posed a dangerous risk because of its unequal distribution in the environment, and that 19 scientists at the University of Rochester had "voiced their disagreement with the President's position." The *Post* editorial concluded with the following:

It is impossible to say with finality who is right in this dispute, but the growing protest of scientists indicates that the Administration is taking grave risks in seeking to sweep the fallout issue under the rug. Why, really, should persons who fear the peril to world health in the continuation of large nuclear tests be placed in the negative position of having to prove that more radiation would be harmful? On an issue which may affect all humanity and on which the limits of tolerance themselves are in question, should it not, indeed, be the responsibility of officials who advocate continuation of such tests to demonstrate beyond reasonable doubt that more tests will be safe? (*The Washington Post*, 26 October 1956)

Clearly, the politics of fallout and nuclear testing had generated considerable controversy just before the 1956 presidential election. Moreover, a growing number of scientists were now publicly speaking out against the AEC and the Eisenhower Administration's positions on atomic testing and the hazards of fallout.

Another incident further shows how politicized and controversial the issue of fallout and atomic testing had become in the wake of the publication of BEAR I. In a letter to

George Beadle—the new Chairman of the Genetics Committee—Warren Weaver, who had resigned after the report was submitted, indicated that he had been asked by Senator Hubert Humphrey, Chairman of the Subcommittee on Disarmament, to testify regarding the dangers of radioactivity. Weaver indicated he was asked a number of questions, including whether the AEC had tried to “bring any pressure to bear” on the committee, and “whether any members of the Genetics Committee had signed the report without seeing it.” To both questions, Weaver answered “no.” However, Weaver then indicated to Beadle that in a separate report that was published at the same time as BEAR I—entitled “A Report to the Public”—a statement on fallout appeared “which I realize does not represent the conviction of some of us.” This report was only seen by the BEAR I committee chairmen, and not by the individual members of the committees. The statement in question referred to the biological damage from atomic testing thus far as “essentially negligible.” Weaver seemed embarrassed by this admission, and he stated in his letter: “I do not want to duck, to the slightest degree, my own personal responsibility in this matter.”<sup>227</sup> Indeed, the aftermath of the BEAR I report indicates that the Eisenhower Administration continued its public relations efforts to downplay the significance of the genetic effects of radiation. It is clear that the science of population genetics was now politicized (in the juridical, as well as Foucauldian sense) in a way that put its practices in national headlines, in Congress, and in the White House.

The makeup of the BEAR II Genetics Committee was roughly the same as BEAR I, but with Dobzhansky now taking an active role and Beadle serving as chairman.<sup>228</sup> Again, the nongeneticists Failla, Hollaender, and Shields Warren were on the committee; Failla and Warren were two of the AEC’s main spokesmen who consistently downplayed the biological hazards of atomic testing. What was significant about the BEAR II committee was not so much the deliberations of the members, as the topics were much the same as those debated by BEAR I, with the balance view receiving even more consideration through Dobzhansky

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<sup>227</sup> Weaver to Beadle, 24 January 1957, Alexander Hollaender Papers, APS.

<sup>228</sup> See, for example, “Minutes of the National Academy of Sciences Committee on the Genetic Effects of Atomic Radiations [BEAR II] Held in New York City,” 1-2 December 1956, Alexander Hollaender Papers, APS. In 1958, during his tenure as Chair of the Genetics Committee, Beadle won the Nobel Prize for his genetics work.

and Wright. More interesting for the present analysis were the debates over what to recommend for future research in genetics, and Dobzhansky's efforts to get Bruce Wallace on the genetics committee. These efforts further illustrate the intertwining of the epistemic and the political, as well as the concern for the future of genetics, that is, its technological infrastructure. At stake were not only the disputes inherent in the classical/balance controversy; now the future of genetics was at stake. Indeed, since the BEAR committee was to be a perpetually continuing reevaluation of the biological effects of radiation (it still exists today), some geneticists saw as a *real possibility* the national implications for future federal funding engendered by a committee that would regularly recommend policy on what kinds of future research needed to be performed.

Prior to a BEAR II meeting scheduled for 15 August 1957—but after the 1957 JCAE Fallout Hearings in May and June, which further brought the problem of fallout into the headlines (see below for a detailed analysis)—Dobzhansky wrote to Beadle to inform him he could not attend, as it would interfere with his fieldwork in Arizona. Instead, he recommended that “the committee should co-opt as one of its members Dr. Bruce Wallace,” who “has done some of the most significant recent work on the genetic effects of radiations on living populations. . . .” Dobzhansky indicated Wallace's participation “would be extremely helpful, especially when the matters of further research which the Committee may decide is necessary for elucidation of the problems which interest it. . . .” Dobzhansky sent a copy of the letter to Demerec (Wallace's superior at Cold Spring Harbor), and wrote at the bottom:

I hope that you will press for inclusion of Bruce. It is a shame to have the Committee include several duds and not include Bruce. And when future research is discussed it is important to have the man present who has done almost the only relevant work anyway! I have sent a copy of this to Campbell and to Glass. Do write in support! (Dobzhansky to Beadle, 8 July 1957, Milislav Demerec Papers, APS)

Apparently, Demerec did not respond directly to Dobzhansky's letter, but instead wrote to Beadle, outlining his proposal for future research in genetics that would shed light on the genetic effects of radiation on human populations; he sent Dobzhansky a copy. Demerec's letter was partly in response to Muller's letter to Beadle of 20 July 1957, in which

Muller outlined his recommendations for future research (Muller did not send Dobzhansky a copy of this letter). In his letter, Demerec, who had switched from studying *Drosophila* to bacteria, called for more “basic research” on

the structure and the operation of hereditary mechanisms, about the way in which genetic changes occur spontaneously or are induced by mutagens, and about the dynamic forces that are instrumental in incorporating the genetic potentialities of individuals into a population. (Demerec to Beadle, 1 August 1957, Milislav Demerec Papers, APS)

In order to accomplish this, Demerec indicated that

at the present state of our knowledge, research in human genetics and research with mammals could not make a significant contribution toward the solution of the problem of genetic effects of radiations, because of the tremendous complexity of the mechanisms involved. We will be in a position to interpret what happens in humans only after the components of these mechanisms have been analyzed in experimental materials that are most convenient and suitable for study. (*ibid.*)

Demerec also proposed a funding system, which “could be accomplished by setting aside a fund (let’s say, one hundred million dollars) to be administered by some competent organization (such as the National Academy of Sciences) and used during a period of twenty or twenty-five years” to fund “already functioning research centers” so as to “attract and train potentially first-rate scientists.” Demerec was concerned that an unregulated appropriation of funds for genetic research “would probably do more harm than good, since, for lack of enough first-rate scientists, second-class workers would assume responsibilities beyond their capacities.” (*ibid.*)

Dobzhansky immediately responded to this proposal, addressing his letter to both Demerec and Wallace, and complaining to them that they had not responded to his letters.

Regarding Demerec’s proposal for future genetics research, Dobzhansky indicated he

would, needless to say, be all in favor [of] \$100,000,000 for research in general genetics. I fear however that your proposal is so extreme as to be in danger of being self-defeating. I would, no doubt, like to know more than I do about things like “the nature of the forces that link genes together, the properties of the materials that are aggregated around them, functioning of genes and other genetic components of a cell in metabolism and development. . .” and [the] like. But I would find it hard to keep a straight face arguing that they must be studied to evaluate the genetic effects of radiations on human populations. We are not a committee on general genetics or

general physiology but a committee to study a narrow problem. Muller would have only himself, Russell and Neel do the research; Milislav would include all the geneticists, plus most bacteriologists and biochemists. Could there be some midway point between the two extremes? (Dobzhansky to Demerec and Wallace, 3 August 1957, Milislav Demerec Papers, APS)

Demerec finally responded to Dobzhansky's letters, arguing that

our Committee would be in a much better position, and would do better service to genetics, if we take a broad stand and come out with a statement that basic research is essential before we can give an opinion about the effect of radiations on human populations. I am convinced that such an opinion cannot be given now, and also that research on human populations would be just a waste of time. . . . I, myself, have a hard time keeping a straight face when there is talk about genetic deaths and the tremendous dangers of irradiation. I know that a number of very prominent geneticists, and people whose opinions you value highly, agree with me. (Demerec to Dobzhansky, 9 August 1957, Milislav Demerec Papers, APS)

On the issue of including Wallace on the committee, Demerec responded:

The question of getting Bruce on the Committee is a little more complex than you think. This has been discussed at some of the meetings, but there was a feeling that it wouldn't be advisable to have a person who is directly connected with [the] AEC. Also, he would be a third person from Cold Spring Harbor, which is another drawback. However, I will bring the matter up again at the forthcoming meeting. . . . (*ibid.*)

Dobzhansky's response was direct and harsh:

Let us be honest with ourselves—we are both interested in genetics research, and for the sake of it we are willing to stretch a point when necessary. But let us not stretch it to the breaking point! Overstatements are sometimes dangerous, since they result in their opposites when they approach the levels of absurdity.

Now, the business of genetic effects of atomic energy has produced a public scare, and a consequent interest in and recognition of [the] importance of genetics. This is to the good, since it will make some people read up on genetics who would not have done so otherwise, and it may lead to the powers-that-be giving money for genetic research which they would not give otherwise. . . . To say, as you do, that “research on human populations would be just [a] waste of time” is plainly absurd. It is research on human populations that is by far the most important task. We would make ourselves plainly ridiculous if we recommend research only on bacteria and bacteriophages instead. (Dobzhansky to Demerec, 13 August 1957, Milislav Demerec Papers, APS)

On the question of including Bruce Wallace on the committee, Dobzhansky replied:



Several members of the Committee are connected with [the] AEC—Hollaender, Russell, and several are getting support of their research from [the] AEC—Muller, Crow, myself, Neel, and I believe Wright. But I feel it revolting to have on the Committee several members who are totally ignorant of the problem which they are supposed to discuss, and not have [the] one man who had done by far the best work on the subject and is going on with more such work! This is the sort of thing [that] makes me despair of human honesty, or at any rate of [the] integrity of that variety of humans which often prides itself of its alleged integrity—the scientists. (*ibid.*)

After the meeting of 15 August 1957, Demerec wrote to Dobzhansky, giving him a summary of the meeting, and then responding to his previous letter:

Our committee has bravely tried to answer the questions posed, but has ended with a few general statements and warnings just because the information available to us at present is inadequate for anything else. And I feel it is ridiculous to think that we could get that information within, say, five years by studies of humans and mice and monkeys. I think the problem is bigger than that, and that we should have the courage to state this and not give false hopes and make commitments that we will not be able to fulfill. . . . We are now in a position to plan experiments, with a reasonable expectation of success, which should tell us what the chemical structure of a gene is, what its biological structure is, what happens when a mutation occurs in a gene spontaneously or when it is induced by radiation or by various chemicals, and also how a gene or a group of genes behaves in a population. . . . Consequently, I think that work that is done by Watson and Crick, Benzer, us, and many others is more pertinent and important for final solution of the problem than work that is going on now with mice. (Demerec to Dobzhansky, 25 August 1957, Milislav Demerec Papers, APS)

Dobzhansky again responded harshly, essentially accusing Demerec of tooting his own horn, and suggesting to him that

when you say “that work that is done by Watson and Crick, Benzer, us, and many others is more pertinent and important for final solution of the problem than work that is going on now on mice” you are talking nonsense. This is in no way a lack of respect for your or their work. But it is completely irrelevant to the problem, just as irrelevant as say, Wallace’s work, which is also very “basic” or “fundamental” research (if you want to use this very objectionable language), is for the elucidation of, say, the problem of crossing over. You may as well recommend studies on the physiology of digestion because it might in some fashion have some bearing on the problem of genetic effects of radiation! . . . The “work that is going on now on mice” is not what it should be, but that does not mean that we should do Watson and Crick instead. One should do better work on mice. To you, “basic information” is a sort of fetish, which means things related to what you are doing. Basic for what??? . . . [T]here exist some basic *unsolved* problems in this field [population genetics] which

are in no way related to Watson and Crick and Demerec and Benzer. This is nothing against Watson and Crick and Demerec and Benzer, for this simply means that there are many basic problems, That's all! (Dobzhansky to Demerec, 1 September 1957, Milislav Demerec Papers, APS)

Dobzhansky wrote in the postscript to his letter that Wallace “has never written to me at all; judging by lack of mention in your letter he has not been put on the Committee. But has the question at least been discussed? Shame!” (*ibid.*).

Dobzhansky and Demerec differed in their perspectives on future research in genetics and on the functioning of the BEAR II genetics committee for a number of reasons. First, their conflict reflected a difference in choice of experimental organisms and the experimental systems that would likely be successful in conjunction with that organism; that is, it reflected a difference in the practice of science. Dobzhansky studied *Drosophila* in order to demonstrate natural selection at work in natural populations, and fieldwork was more important to him than a policy meeting among geneticists that would be dominated by Muller's genetic load viewpoint (and hence too mathematical for Dobzhansky). Demerec, a geneticist and the Director of the Cold Spring Harbor Laboratory, had moved from *Drosophila* to bacteria and a more biochemical and then “molecular” approach to genetics, seemed to be worrying more about the future of laboratory funding for genetics experiments than quibbling over the theoretical arguments of Muller, Crow, and the other classical Mendelian geneticists on the committee.<sup>229</sup>

In addition, the Demerec-Dobzhansky exchange shows that the geneticists were clearly concerned about the future of their work, and they saw their moment in the government and public spotlight as something to take advantage of, especially if it might lead to increased funding. Moreover, it shows a difference in outlook on the issue of separating signal from noise, on getting results that would answer the questions that needed to be answered. Dobzhansky, on the one hand, claimed he wanted the committee to fulfill its mandate to answer questions regarding the genetic effects of radiation, and he saw Wallace's experiments on heterosis as the means to do this. That is, heterosis and homeostasis

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<sup>229</sup> Burian (1993a, b) shows the significance of how the choice of experimental organism matters in biology, and he provides a case study on the transition from genetics to molecular genetics.

arguments—derived from experiments involving fruit flies in cages subject to radiation, followed by statistical analyses to determine average fitness levels—could provide the answers to the questions. This was Dobzhansky’s kind of work—theoretical population genetics.

Demerec, on the other hand, did not see the efficacy of such arguments, and his focus on a biochemical and emerging molecular approach, in conjunction with the confusion and fundamental theoretical disagreements among geneticists on the committee, apparently led him to conclude that such arguments did not have merit, even if this meant that the committee could not properly fulfill its function in recommending policy. Clearly, Demerec aligned himself with the kind of work he saw as generating future experimental work that could answer the basic questions of genetics from an emerging and different tradition than Dobzhansky’s. These concerns are central to the notion of the technological infrastructure of science; that is, they bear on the support systems of future science and on *how* and *why* science will be practiced one way rather than another. On the one hand, Dobzhansky and Demerec had differing ideas on what that infrastructure should be; they disagreed on what the future of genetics should look like. On the other hand, their efforts on the committee illustrate one component of the technological infrastructure of genetics outside of the laboratory: the increasing significance of large-scale laboratories, federal funding agencies, policy-making committees, and government regulatory bodies as critical components of the technological infrastructure of science. Clearly, how the science of genetics was to “advance” into the future would have much to do with traditionally non-epistemic factors, in addition to epistemic ones.

Finally, in considering all these themes together, it is difficult to conclude that there is any sharp separation between the practice of science and the practice of politics (in the Foucauldian sense of power/knowledge). Rouse’s view of the “intra-twining” of epistemology and power, his view of “epistemic politics,” is pertinent here. The practice of science *was* at times the playing of politically epistemic games, whether at the level of argumentation in the contestable theoretical disputes of population genetics, at the level of science policy-making, as with the various organizations and committees responding to the

scientific and political controversies surrounding the efforts to establish exposure guidelines in the light of concerns over fallout from atomic testing, or with the planning of the future infrastructure of experimentation based on funding opportunities.

By starting with a perspective, if not an historiography, that eschews a commitment to epistemic sovereignty or any preconceived notion of the global or universal nature or essence of what “science” is, one can describe the efforts of scientists, using a properly postmodern model of historical change, as involving a set of interrelated experimental, theoretical, epistemological, ethical, political, and personal practices. And this means that the research and experimental efforts of scientists—despite our prevailing strongly-held cultural view of the “disinterestedness” or “value-free” or “apolitical” nature of good or proper science—should be characterized in a way that intratwines the epistemic with the political. All knowledge claims are contested, some more so, or more interestingly so, than others. Nature and normativity are intra-twined. Sometimes, indeed, they even become intra-twined in a way that makes it seem that the political and power components of knowledge generation have dominated the epistemic components. One example of this, in addition to the policy-setting committee work described above, is when scientists are called to give publicly their opinions and expertise to Congressional committees, as with the 1957 hearings on fallout before the Joint Committee on Atomic Energy. These hearings further explicate the political and cultural contexts in which population genetics was practiced, and they serve to set the stage for the analysis in Chapter VIII below, in which I present some broad cultural conclusions for these interrelated narratives.

### **The 1957 Fallout Hearings**

In order to assess further the public positions of the opposing parties in the fallout/atomic testing debate, the 1957 fallout hearings (JCAE 1957) before the U. S. Congress’ Joint Committee on Atomic Energy (JCAE) is perhaps the best available public record. The testimony and record of these hearings can be used to gauge the state of the debate over the genetic effects of fallout. As indicated, in the 1950s there was no official or legally binding

statute or policy statement on radiation exposures that the AEC was bound to follow. However, the AEC was compelled to give its official views on radiation hazards before the Joint Committee. Additionally, in order to cope with its “public relations problem,” AEC officials made numerous public pronouncements on fallout hazards in order to allay public fear and to defend their positions on atomic weapons testing and its hazards.

The 1957 JCAE Fallout Hearings reveal a number of significant points. First, on the question of the genetic effects of fallout, there was surprising agreement among the geneticists who testified, including William Russell, the one AEC geneticist to appear. The classical viewpoint dominated the discussion, with only non-geneticists arguing a position closer to the balance view. In addition, the geneticists called for some kind of limitation or ban on testing, for more geneticists in policy-making positions within the AEC, and for more funding for genetics research. How decisions were made on which geneticists to call to testify is a question awaiting further research.<sup>230</sup>

Second, the AEC scientists and policy-makers who testified were in virtual agreement that atomic testing must continue for national security reasons, that fallout caused negligible or acceptable health hazards, and that the genetic effects of fallout were not a cause for alarm. Moreover, only AEC officials (non-geneticists) presented arguments to the effect that low levels of radiation might be beneficial to the future generations from an evolutionary standpoint. AEC officials, in their testimony before the JCAE, used these arguments, which were a distortion of the balance viewpoint. Much stronger and more distorted versions, however, were offered to the public via lectures, addresses, the news media, and popular books. The purpose of these arguments, stating that fallout might actually be genetically beneficial, was to calm public fears of fallout and to counter the arguments of geneticists like Muller and Sturtevant, who publicly warned about the genetic dangers of fallout and called

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<sup>230</sup> Muller indicated in a letter to the biophysicist Alexander Hollaender—who was Director of the Division of Biology, Oak Ridge National Laboratory from 1946 to 1966—right before the 1957 JCAE hearings, that he was “asked to be present at the Congressional hearings on fallout on June 4.” Muller told Hollaender that he “ought to be asked for this” also, and that a “representative of the Washington branch of the Federation of American Scientists [Dr. Mortimer M. Elkind] is in touch with the [Joint] Committee [on Atomic Energy] and if you wrote to him he would probably be glad to know that you were available for that and would arrange it with the Committee.” Muller indicated that Elkind was “the one who first communicated with me about this matter” and he provided Hollaender with Elkind’s address (Muller to Hollaender, 23 May 1957, Alexander Hollaender Papers, APS).

for a halt to weapons tests. The view that the AEC, with instructions from Eisenhower, deliberately made issues concerning atomic testing confusing to the public, is borne out by this analysis.<sup>231</sup>

### I. *Geneticists at the 1957 Fallout Hearings*

There was general agreement among geneticists who testified at the 1957 Fallout Hearings (JCAE 1957) on the nature of the genetic effects of radiation. Those testifying were James Crow, Bentley Glass, Hermann Muller, Alfred Sturtevant, and William Russell, principal geneticist for the AEC's Oak Ridge National Laboratory. As Senator Clinton P. Anderson remarked:

I was just wondering if geneticists had a union, guild or gang, or something that teaches you to hang together? This is not only the most agreeable group of seven scientists [2 non-geneticists participated in this discussion], but certainly the most agreed group I have seen. (p. 1144)

Sturtevant replied:

I would like to say that I think it would have been very difficult to get together a group that would have disagreed with most of what has been said here among practicing geneticists. (p. 1144)

In his testimony, Crow gave a summary of information on the genetic hazards of radiation. He stated four points he deemed “well established principles that are necessary background for any discussion of possible genetic hazards to man” (p. 1009). These principles were:

1. All high energy radiations increase the rate of mutation. . . .
2. Almost all mutations that have been studied have been harmful.

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<sup>231</sup> A 1979 *The New York Times* article recorded the furor that erupted when declassified documents suggested that Eisenhower had told the AEC in 1953 to “keep the public ‘confused’ with its explanations of the kind of explosions that were causing radioactive fallout. . . .” Senator Edward M. Kennedy, Chairman of the Subcommittee on Health, “argued that the commission’s [AEC] interest in its bomb-testing program had led it into a ‘clear pattern of distortion’ of the health implications of the atmospheric testing.” The article states that the “records released by Mr. Kennedy showed many instances of commission [AEC] concern that public fears might soon force them to give up the Nevada test site, along with discussions of ways to allay those fears” (Adam Clymer, “A-Test ‘Confusion’ Laid to Eisenhower: Records Indicate That He Advised Vagueness on Explosions,” *The New York Times*, 20 April 1979, p. A1).

3. My third main point is that one might perhaps think that mutations that cause only a minor impairment are unimportant, but this is not so for the following reason: Deleterious mutant genes are eventually eliminated from the population since they generally increase the death rate or lower the fertility of the person carrying them. A mutant [gene] that causes a great deal of harm is eliminated in a few generations. But one which causes only a small amount of harm will persist much longer, and thus affect a correspondingly larger number of persons. On the average the larger number affected by a mild mutation roughly compensates for the lesser effect on the individual.

. . . . .

4. Evidence from experimental animals, principally *Drosophila*, indicates that the number of mutations produced is strictly proportional to the amount of radiation received. (pp. 1009-10, 1012-13)

The testimony of the other geneticists did not depart appreciably from Crow's position, a good summary of the classical view on radiation genetics. Some members of the JCAE, however, pressed Crow on his view that most mutations are harmful. For example, Senator John W. Bricker asked Crow to explain how, if most mutations are harmful, do we square this view with the fact that background radiation must be producing deleterious mutations constantly. As Bricker stated, "There must be some that are beneficial or there would not be the evolutionary process taking place in life" (p. 1011). Crow explained:

The reason for this is natural selection. The mutant genes that have occurred in the past have been weeded out by the process of natural selection so that the genes which are now part of the normal population are those which have been retained by this process of natural selection. Therefore, even though the great majority of mutants at the time they occur are such as to cause harmful effects to the descendants, the ones which cause the most harmful effects are eliminated by natural selection. The genes left in the population are the beneficial ones. (p. 1012)

One point of ongoing confusion between the geneticists and the committee members was the issue of the genetic effects to individuals versus effects to the population as a whole. As the BEAR I participants had discussed the year before, what might be considered "beneficial" to a few individuals may not be advantageous to the population as a whole. Similarly, what might be considered "beneficial" in terms of average population fitness to one population as compared with a second population, may still not mean that most individuals in that first population are better off, or were more fit. It depends on how fitness

is defined and measured, and whether one is talking about individual or population fitness. Therefore, as in Wallace's experiments on heterosis, even though a population exposed to radiation might produce subsequent generations with a higher percentage of individuals with increased fitness (or higher "average" fitness), as compared to a non-irradiated or control population, this does not entail that most, or even many, of the *individuals* in the progeny of the irradiated population are better off. Indeed, as Beadle indicated above, in Wallace's experiment, 98.8% of the flies were eliminated in each generation because of the experimental laboratory conditions. Consider the following exchange in Crow's testimony between Crow and Senator Bourke B. Hickenlooper:

**HICKENLOOPER:** I am sorry to keep interrupting you, Doctor, but, may I ask, do some mutations have a beneficial effect occasionally?

**CROW:** I think a very small minority of mutations probably have a beneficial effect.

**HICKENLOOPER:** We have had experience in mutations in grain where exposed to radiation, and I think the record shows that while in the overwhelming number of those mutations the progeny is less desirable than the ancestor, yet in a certain small percentage the mutations produced are in many ways far superior to the ancestry. Is that your understanding?

**CROW:** I think that is very likely true in man as well, Senator, but neither you or I would suggest—

**HICKENLOOPER:** I am sorry; I understand that you talked about that on the record before I came in, and I do not want to go over the same ground.

**CROW:** I have a sentence I want to say, anyhow. I think neither you or I are willing to suggest that we sacrifice 999 persons with inferior mutations in order to get one beneficial one.

**HICKENLOOPER:** I think we agree that is not even a 50-50, one horse and one rabbit, but I only wanted to be assured on the point that, in effect, not all mutations are bad and that if we have to have mutations there might be some modicum of benefit that would come out of it.

**CROW:** A very small amount. (pp. 1019-20)

Neither Dobzhansky nor Wallace, who accepted extreme versions of the balance view, argued that fallout or other forms of low-level radiation would be genetically beneficial to subsequent generations of *humans*. In fact, they both publicly called for a test ban (Wallace and Dobzhansky 1959, p. 188).<sup>232</sup> Wallace ([1956] 1957), whose paper for the

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<sup>232</sup> Wallace and Dobzhansky (1959) stated that they "are among those who believe that tests of nuclear weapons should stop. But the genetic damage resulting from these tests is not the sole reason for this opinion.



1956 World Health Organization conference on the genetic effects of radiation was submitted for the record of the 1957 JCAE Fallout Hearings, made the point quite clearly. He characterized the classical position as one which “postulates that individuals of the highest possible fitness can be completely homozygous,” and the balance view as one which “assumes that even under constant environmental conditions the individual with the highest fitness is genetically heterozygous rather than homozygous” (p. 1758). In the former, natural selection eliminates deleterious alleles from the population. In the latter, natural selection balances various allelic combinations depending upon environmental conditions and serves to maintain diversity of genotype. With respect to which view of natural selection is correct, however, Wallace offered the following:

If it should develop that selection is more effective in man than we have suspected, we must nevertheless be wary of those who claim that radiation will do no harm to the human species. The rate at which mutant genes enter the gene pool of a population must equal the rate at which they leave. Mutant genes leave the gene pool by the effective elimination of individuals whether through death, sterility, failure to reproduce, or a tendency to reproduce at a reduced rate. Effective elimination of individuals means, for human beings, that one individual is placed at a disadvantage relative to another; in many instances the “elimination” is accompanied by mental or physical suffering. Therefore, regardless of the ability or inability of “natural” selection within human populations to forestall extinction or to maintain the “fitness” of the population as a whole, we are still forced to the conclusion that every exposure of individuals to irradiation must be justifiable in terms of the beneficial effects that exposure confers either to the exposed individual or the population as a whole. In the light of known effects of radiation it is impossible to defend unnecessary or unnecessarily high exposures. (p. 1761)

Concerning AEC policy and atomic testing, the geneticists at the fallout hearings were again in agreement.<sup>233</sup> Glass warned that “unless some international agreement to limit

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As citizens, we consider that the armament race of which these tests are a part is a terrible folly because it leads to war. To our knowledge, there has never been an arms race that ended otherwise; disarmament programs follow wars, they are not undertaken spontaneously. If the next war is to be avoided, suspension of bomb tests seems a necessary first step toward relaxation of political tensions and toward a more general disarmament” (p. 188).

<sup>233</sup> In May of 1957, the scientists Linus Pauling, Barry Commoner, and Edward Condon drafted a petition (Pauling 1957) calling for “an international agreement to stop testing of all nuclear weapons.” By June, 2,173 scientists, including many working in government laboratories, had signed the petition. The petition stated: “Each nuclear bomb test spreads an added burden of radioactive elements over every part of the world. Each added amount of radiation causes damage to the health of human beings all over the world and causes damage to the pool of human germ plasm such as to lead to an increase in the number of seriously defective

weapons-testing, or to eliminate it, is reached, it will only be a matter of a few years until other nations will be testing weapons, too” (p. 1037). Sturtevant argued that “every bomb test adds to the biological hazard. It follows that the most effective way to reduce future hazards would be not to test any more bombs” (p. 1047). Muller, who only a few years earlier had supported atomic testing and had characterized calls for a test ban as “defeatist propaganda,” (Muller 1955a, p. 839) now was strongly in favor of a cessation of testing:<sup>234</sup>

[T]he consequences of a full-fledged war, with its heavy irradiation of large numbers of people on both sides, would be inordinately more serious [than testing itself] in its effects on the human genetic heritage as well as in its more direct effects. It is this consideration which, in any opinion, makes a continuation of test explosions a monstrous mistake of policy for both sides. Of course, it would be absurd to expect one side to stop without the other. But a continuance by both sides would tend to lead the world nearer to a war that even with present techniques would result in the cataclysmic ruination of humanity in general. (p. 1059)

Concerning the way the AEC handled policy matters regarding testing and the genetic effects of fallout, the geneticists argued that the AEC was not doing all it could. Glass, a member of the Advisory Committee for Biology and Medicine (ACBM) of the AEC, called for a geneticist to serve as an AEC Commissioner, or at least have “the present General Advisory Committee [GAC] revised to include geneticists and biologists” (p. 1040). Glass believed that the AEC program of research demonstrated an

imbalance between emphasis on the physical aspects of atomic energy and on the

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children that will be born in future generations” (p. 264). Among the scientists who signed the petition were Ralph Lapp and the geneticists Milislav Demerec, Bentley Glass, Richard Goldschmidt, G. Ledyard Stebbins, Alfred Sturtevant, Sewall Wright, and Dobzhansky and Muller (pp. 265-6). Pauling wrote to Eisenhower that “it is especially significant that the geneticists, who have, of all scientists, the best basis for judgment about the genetic effects of the fallout radiation from the atomic bomb tests, and biologists in general, who have the best basis for judgment about the somatic effects of the fallout, have joined so vigorously in the appeal to stop the bomb tests” (Pauling to Eisenhower, 4 June 1957, Linus Pauling Papers, National Library of Medicine). Curt Stern wrote to Pauling, telling him he could not sign the petition, as its “absolute statement concerning the damage produced by testing seems misleading. . . .” However, Stern also said that he “would rather sign a no more unrealistic document which would declare that sovereignty is an outdated, deadly concept.” The specter of nuclear war disturbed Stern much more than atomic testing: “Even without further testing the nuclear weapons now available—and ‘conventional’ ones too which have killed hundreds of thousands of people in Dresden, Tokyo and elsewhere—will continue to be the terrible threats they are now” (Stern to Pauling, 11 March 1958, Curt Stern Papers, APS).

<sup>234</sup> As Muller explained in his testimony: “I might add in this connection that in my talk to the National Academy [of Sciences] 2 years ago I stated it to be my belief at that time that a continuation of the nuclear tests was necessary. I still think that this was the case at that time but I think the situation has changed since then” (JCAE 1957, p. 1060).

biological counterpart, its effects on living beings. . . . I think it is safe to predict that this imbalance is likely to continue until the genetic and other biomedical problems and points of view are appropriately represented on the Commission itself. (p. 1038)

Sturtevant criticized the AEC position for minimizing the potential genetic damage from test fallout, and for comparing such risks to those “that we voluntarily take repeatedly—such as those involved in riding in an automobile or going for a swim at the beach” (pp. 1046-7). While he noted that there were areas of agreement between the positions of the AEC and the geneticists, Sturtevant stated that

there remain areas in which some geneticists are still disturbed by the AEC position. This, as has often been pointed out, arises in part from a difference in attitude concerning very small percentages. Various methods in calculating damage from fallout result in estimated numbers of affected individuals ranging from a few hundred to perhaps tens of thousands or even millions, depending in part on what assumptions one makes about the rate of future bomb testing. The highest of these numbers remains a very small proportion of the total population, and to some people this means that it is relatively unimportant. It is probably unimportant for the survival of the race, and it is relatively unimportant as an economic burden to society, though these could become serious matters if the rate of fallout should increase by a large amount. But hundreds or thousands or tens of thousands or more of individual human beings are involved, and to me it is not acceptable to say that they are unimportant, no matter how small a percentage of the total they make up. (p. 1046)

Muller devoted part of his testimony to criticizing the NCRP and the make up of its subcommittees. He argued that while the NCRP has had “a geneticist here and there,” it has official representation from about 15 different organizations, mostly of a medical or governmental nature. Yet they do not have an official representative from any of the professional genetic organizations, such as the Genetics Society of America, the American Society of Human Genetics, the American Genetic Association, or the Society for the Study of Evolution, all of which are in my opinion as closely concerned with this matter as for example the Radiological Society. Consequently, there is not sufficient representation among them of the genetic point of view of the mechanism which leads us to expect no threshold and to take the matter more seriously at small doses. (pp. 1061-2)

Even William Russell, the AEC geneticist who was also a member of the National Academy of Sciences genetics committee (BEAR I 1956), complained to the Joint Committee that the geneticists on the NAS committee were criticized for reaching conclusions on “inadequate evidence,” even when they were requested to do so (pp. 1142-3).

As he stated, “I don’t think anyone should be reprimanded for drawing a conclusion when a conclusion was requested” (p. 1143). As for the source of the criticism: “I believe geneticists have been blamed for making too definitive statements based on the evidence, perhaps mostly by medical specialists” (p. 1143).

As the testimony reveals, on the important issues relating to the genetic effects of radiation and to policy regarding atomic testing, the geneticists at the 1957 Fallout Hearings were in close agreement. The consensus was that the number of affected individuals from fallout was a small percentage of the population, but in absolute numbers the number affected was by no means negligible.<sup>235</sup> If the rate of testing were to increase, the genetic damage to humans would increase. In addition, most called for a limitation or ban on atomic testing; none unequivocally supported continued testing. The geneticists were critical of the AEC’s position on the genetic dangers of fallout, and were critical of non-geneticist spokesmen promulgating the viewpoint that the genetic hazards of fallout were negligible.

Furthermore, the testimony shows the confusion that resulted from the distorted version of the balance theory that was prevalent among nongeneticists. The view that low levels of radiation might be genetically beneficial because they would produce favorable mutations in subsequent generations, became part of the standard argument of the AEC. In part, this reflected a lack of geneticists in important positions in the administrative structure of the AEC. Physicists and medical specialists dominated the scientific policy-making positions in the AEC, such as the GAC and the ACBM. The science of human population genetics, “a relatively new science” according to the pathologist Shields Warren (p. 1414), was perceived by many non-geneticists to be speculative, inconclusive, and not on firm experimental ground. Physicians and other medical specialists even attacked the very concept of attempting to quantify the biological effects of low levels of radiation. As Austin Brues, a physician with the Argonne National Laboratory, remarked:

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<sup>235</sup> Crow estimated the total genetic damage to the world population assuming atomic testing produced fallout that exposed each individual to an average dose of 0.1 roentgen. In the first generation, he estimated 8,000 gross physical or mental defects; 20,000 stillbirths and childhood deaths; and 40,000 embryonic and neonatal deaths. The total for future generations would be 80,000 gross defects; 300,000 stillbirths and childhood deaths; and 700,000 embryonic and neonatal deaths. However, the “fraction by which existing abnormalities would be increased,” he calculated as 0.0001 (JCAE 1957, p. 1021). The percentage increase in genetic damage is small, yet the absolute numbers are relatively large.

As one who sits on various committees to discuss and, we hope, solve these problems, I am . . . impressed with the danger that more and more of the best talent and time for the imaginative approach to these questions may be drawn away from the work and thought that they ought to be producing, into more and more debate over the same scanty knowledge. (pp. 933-4)

## II. *The AEC Position*

The official position of the AEC on atomic testing was summarized by the chemist Willard Libby, an AEC Commissioner and one of the AEC's chief researchers and spokespersons on fallout from atomic testing.<sup>236</sup> When asked by the Joint Committee to give "the scientific reasons for the continued testing of weapons," Libby stressed that "the survival of the Nation and that of the free world" depended on the "United States' defensive and deterrent capabilities. These capabilities are inextricably bound to our nuclear warheads and to the weapons systems which would carry these warheads" (p. 1373). Clearly, Libby did not go into detail on the "scientific" reasons for atomic testing; doing so would have required releasing classified information:

As you know, one cannot enumerate in an unclassified statement and without divulging to the world the status of our nuclear armaments the known defense and deterrent weapon systems which we would forego by stopping tests. From the past test series, we have found, sometimes unexpectedly, means of increasing the efficient use of materials, reducing the size and complexity of warheads, increasing their deliverability and yield, and reducing the radioactive contamination from our larger yield devices. The committee is familiar with these past developments and with the fact that additional systems now scheduled could not be brought into being without

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<sup>236</sup> Charles Dunham, Director of the Division of Biology and Medicine, AEC, read the text of Libby's official statement into the record of the 1957 JCAE Fallout Hearings (pp. 1373-74). Libby's scientific contributions to the study of the biological effects of fallout include "Radioactive Fallout and Radioactive Strontium" (1956b), *Science* 123: 657-660; "Radioactive Strontium Fallout" (1956c), *Proceedings of the National Academy of Sciences of the United States* 42: 365-390; "Current Research Findings on Radioactive Fallout" (1956a), *Proceedings of the National Academy of Sciences of the United States* 42: 945-962. In his scientific papers on fallout, Libby consistently downplayed the extent and biological hazards of fallout, and he repeatedly maintained the position that "the worldwide health hazards from the present rate of [atmospheric atomic] testing are insignificant" (Libby 1956b, p. 660). Further AEC-sponsored studies presented similar arguments. A 1957 article in *Science*, drawing on worldwide autopsy samples of human bone and other AEC data—including Libby's (1956c) data on human stillbirths and animal bones—drew attention to the result that "children have 3 to 4 times more strontium-90 per gram of calcium, on the average, than adults." It stated that while strontium-90 levels were increasing, they were still 1/10,000 of the NCRP limit (Handbook 52, 1953) in 1955; see J. Laurence Kulp, Walter R. Eckelmann, and Arthur R. Schulert (1957), "Strontium-90 in Man," *Science* 125: 219-225, quotation from p. 222.

further tests. In a public statement, I can only emphasize that our weapons development would be crippled by a cessation of tests. (p. 1374)

On the subject of the genetic effects of fallout, Libby had much to say publicly, yet before the Joint Committee he deferred to the authority of geneticists, thus sidestepping the issue:

**REPRESENTATIVE VAN ZANDT:** Dr. Libby, let me ask you this question: In your opinion, as a commissioner, do you think that the subject of genetics has been adequately covered?

**DR. LIBBY:** I am not a geneticist, and I know so little about the subject of genetics, Mr. Van Zandt, that I hate to answer that question. It seems to me that I do not know the answer. (p. 1207)

In numerous cases, however, Libby did discuss the genetic hazards of fallout when lecturing on fallout effects. His position was that since fallout produced levels of radiation that were small in comparison to background radiation levels, the genetic effects must be small, if not negligible.<sup>237</sup> In a speech given at Northwestern University on 19 January 1956 and subsequently published in *Science*,<sup>238</sup> Libby stated the following:

On the basis of the information so obtained [AEC fallout analyses], it is possible to say unequivocally that nuclear weapons tests as carried out at the present time do not constitute a health hazard to the human population insofar as radiostrontium is concerned, and it is with good reason that radiostrontium is likely to be the most important of the radioactivities produced. It is well to note that since radiostrontium is assimilated in the bones it constitutes essentially no genetic hazard, for its radiations do not reach the reproductive organs.

Later, he stated the following in a lecture at the University of New Hampshire in April 1957:

By the study of animals and plants we know that radiation does produce genetic

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<sup>237</sup> A 27 April 1957 editorial in *The Washington Post* sharply criticized Libby and the AEC for their position on the health hazards of fallout. The editorial stated that “a wall of secrecy shuts out the public from full knowledge of the facts which the Atomic Energy Commission bases its determinations.” As for Libby’s views on the genetic effects, the editorial noted that Libby’s assumptions, “apparently based on averages,” “must inevitably be qualified for individual cases.” Chastising the AEC for “not even acknowledg[ing] the risk” from atomic testing, the editorial argued that the AEC “position would be much more convincing if it would meet this issue head on—and its position would be stronger still if it would seek to limit the risk, if not through a general moratorium on large tests, at least through some effort to control ‘dirty’ tests. Aunty-knows-best is scarcely a satisfactory answer when the consequences of error may be irremediable” (“Is Fallout Good?” *The Washington Post*, 27 April 1957, p. A12).

<sup>238</sup> Willard Libby (1956b), “Radioactive Fallout and Radioactive Strontium,” *Science* 123: 657-660, quotation from p. 659.

changes—we even irradiate plant seeds in order to speed up the rate at which new forms appear so superior new plants can be produced by selection of the few desirable ones. . . . However, most of the forms from the irradiated seeds have inferior properties and it is only a rare one that is a definite improvement on the original plant. Similar results are found with animals so we guess that human beings probably are subject to [the] same type of effects. Therefore, we believe that there must be *some* genetic effects of test fallout radiation but, again, from our normal experience in which no effects of high altitudes versus low, or brick versus wooden houses, etc., have been observed, we know that the effect must be very small. . . . [T]here must be some very small effect, although it will be entirely undetectable from test fallout. (JCAE 1957, p. 1519)

Later that month, at a meeting of the American Physical Society in Washington, D.C., Libby again discussed the genetic effects of test fallout. Again, he stated that there were effects from fallout, but that “these are extremely difficult to evaluate, since there is so little known about human genetics” (JCAE 1957, p. 1524). Then, citing Muller’s estimate that perhaps ten percent of the human spontaneous mutation rate is caused by irradiation, Libby stated that the genetic effects produced by fallout are

comparable to moving to a slightly different locality and [are] much less serious than changing from one house to another or doing any of a dozen things. The only important point is that the genetic effects show only if large numbers of people are subjected to them. (p. 1524)

The implication here is that the change in background radiation one would receive from moving to a higher altitude or from moving into a house subject to higher terrestrial radioactivity, is much greater than the slight increase one would receive from exposure to test fallout. Moreover, since humans have been continuously subject to background radiation, then the effects of fallout must be small and not different from those due to natural radioactivity. As Libby stated at an address at the University of Chicago in 1955, “if the genetic damages from the tests are real we have always had them in much larger measure from nature” (Libby 1955, p. 131).

One of the viewpoints which the AEC fostered and used to defend atomic testing, was that such a small increase in mutation rate as would be caused by fallout might actually be *beneficial*. In a published version of the 1955 address by Libby just cited (Libby 1955), the editor printed the following comment along with the text of Libby’s address:

EDITOR'S QUERY—It seems inherent in the above discussion that all additional mutations from increased radioactive levels will be damaging, yet it also seems to be an accepted theory that mutations are necessary for progress, in the sense that they cause some fitter-than-average personalities to appear—in other words, that mutations brought us up out of the primordial slime. Is it not possible that along with a few “less-than-normal” humans, a Lincoln, a Newton or a Beethoven might also be produced? (p. 131)

Similar arguments were offered by an AEC spokesperson at the 1957 Fallout Hearings. In a paper submitted for the record,<sup>239</sup> Hardin Jones, a physiologist with the University of California Radiation Laboratory, argued that the “Darwin principle of evolution” made

it possible that some increase in the mutation rate might be to human advantage in the long run by providing a greater pool of variance from which selection could take place, to our final advantage some thousands of years from now. (p. 1120)

According to Jones:

At low radiation levels, such as 10 percent or 1 percent above the natural radiation background (the range of fallout effect), it seems unlikely that long-range genetic disturbances can become an appreciable problem, since the natural radiation background appears to account for only 10 percent of the change in genetic structure per generation. One may speculate further that, in the long run, man may be beneficially affected by good genes yet to be found, so that increasing radiation exposure and the mutation rate may operate to human advantage. (p. 1121)

Additional examples of statements arguing that low levels of radiation might be genetically beneficial to humans are available from the public statements of AEC officials. At a symposium in 1957 on low-level irradiation organized by the AAAS and co-sponsored by the AEC and the Argonne National Laboratory, the AEC geneticist Earl Green (who had invited Muller to give the paper at the 1955 Atoms for Peace Conference) presented a paper on the genetic effects of low-level radiation (Green 1959). Green's paper focused on the uncertainties involved in quantifying the genetic damage resulting from low-level radiation, and it left many questions unanswered. Among those questions he believed could be “answered only by means of experimental or observational data,” were whether

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<sup>239</sup> Hardin B. Jones, “A Summary and Evaluation of the Problem with Reference to Humans of Radioactive Fallout from Nuclear Detonations,” January 14, 1957, in JCAE 1957, pp. 1116-1136.



“radiation-induced mutant genes, if detrimental,” would “persist in the population,” and the following version of the classical/balance quandary:

Many, if not all, species of animals, including man, living in nature are genetically heterogeneous. Nature appears to favor, in some cases, a persisting polymorphism. What will be the effect of adding new mutant genes to the gene pool of the population? Will they come to expression in unfavorable or deleterious ways? Or will they be sorted over, so to speak, and in combination with genes already existing in the gene pool give rise to new adaptive combinations? (pp. 60, 61)

Another example of distorted versions of the issues in the classical/balance controversy comes from Merrill Eisenbud’s chapter in *America Faces the Nuclear Age* (Fairchild and Landman 1961, ch. 6). Eisenbud, a health physicist and former manager of the AEC’s New York Operations Office, characterized “public fear of radiation hazard[s]” as “probably the greatest single impediment to the development of atomic energy” (pp. 94-5). In his attempt to “induce people to be less panicky about the hazard,” Eisenbud took up the “genetics problem” (pp. 91, 102). Referring to Muller’s early experiments demonstrating radiation-induced chromosomal changes, Eisenbud stated that such mutations, for example changes in the “size of the ears,” might produce a hereditary change. But, according to Eisenbud, “[w]hether the change is a beneficial one or not depends on the size of ears in this family in the first place, on a person’s point of view and on the nature of the change” (p. 103). He stated that

there is a basis for believing that at least part of the evolutionary changes that have taken place throughout history have been due in part to the ionizing radiations from nature. Believing this, a person might say, “Look at us! Aren’t we a good thing, and aren’t we this way because of radioactivity? Otherwise we might still be a slime in a pond somewhere! Why is radioactivity bad?” (p. 104)

Eisenbud eventually launched an attack aimed directly at Muller’s classical view:<sup>240</sup>

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<sup>240</sup> Eisenbud, who had no advanced degree or training in genetics, was trained as an industrial hygienist and had a degree in electrical engineering. The AEC hired Eisenbud in 1947 and he worked for the NYO Office until 1959. In his oral history, Eisenbud (1995) displayed a degree of contrition for his role in the AEC’s deception of the American people regarding the effects of fallout. However, his views on the genetic effects of radiation continued to reflect a gross misunderstanding of even basic issues. For example, regarding genetic effects, he stated: “There were two authoritative reports in 1956, one from the Medical Research Council in the United Kingdom, and our own National Research Council, summarizing what was known about biological effects of radiation, and all they talked about was genetics. But genetic problems are not acute problems. You would have to expose generation after generation, contaminate the whole pool. That’s not

Many geneticists argue that there are a limited number of permutations and combinations that can take place in chromosomes. Evolutionary doctrine says that when a good change takes place, it survives because of the principle of survival of the fittest. If a deleterious change takes place, it ultimately dies out. Thus, most of the beneficial combinations may have already been tried out, and some scientists—egoists that they are—say that we have become about as perfect as we can be and that from here on, any more mutations would produce only defects. In the minds of many mothers . . . there is fear today about medical X ray and exposure to radioactivity in any form. Many of these mothers would be comforted to know that the possible genetic injury that is being talked about is not an injury to children or grandchildren, but an injury to the offspring's offspring's offspring many generations hence. What will this injury be? Again, nobody knows. (p. 104)

### **Futural Norms**

This narrative on radiation genetics in the Cold War, constructed in conjunction with the prescriptions of the technological infrastructure of science and postmodern naturalism, suggests and reflects several issues. First, given the presupposition that the epistemic foundations of science are not set or fixed, but are at stake and molded and changed in practice, the activities of the geneticists involved in this story should *not* be seen as irrational or extra-scientific. On the contrary, the geneticists were doing what scientists do in their practices: they interact with their surroundings in attempts to account for the physical and biological phenomena they encounter, but these practices reveal efforts and norms that are not describable as purely epistemic. Their practices reveal what were the norms at work in

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something you do: genetic effects are not seen in a few people, because they were irradiated.” In addition, Eisenbud (1995) again mentions the notion of the possible beneficial effects of low levels of radiation, this time mistakenly calling it “hormesis.” He stated that “there is some evidence that low doses of radiation are beneficial.” His conclusion was: “They've looked at cancer, and they've looked at genetics, longevity, and everything. The people in the high-background area are better off than the people in the low-background area.” Eisenbud's statements on the genetic effects of radiation are in direct contradistinction to what geneticists argued in the 1950s and 1960s, in addition to being in opposition to the current state of knowledge as defined by the National Academies. What Eisenbud (1995) was attempting to describe in his oral history interview was probably *heterosis*, not *hormesis*; both concepts are currently believed to have only very limited roles in explaining data on radiation exposure or exposure to toxic chemicals. *Hormesis* is defined as the beneficial effects from exposure to low doses of a toxin (or radiation) that would be hazardous at higher doses, while *heterosis* is hybrid vigor, or the notion that populations largely heterozygous for genes would have an intrinsically superior fitness over those populations that were largely homozygous. That is, *hormesis* is a somatic effect on *individuals* who are exposed to the toxins or the radiation, while *heterosis* is a genetic effect on the fitness of *populations*. Those interviewing Eisenbud did not recognize the distinction and apparently did not realize the difference.

their interactions with the material world, with other scientists, and with the other discursive and non-discursive aspects of the culture in which their practices took place, and that those practices partially constituted.

In this narrative, and as reflected in this dissertation's focus on the technological infrastructure of science, three of the main norms were: (1) the prospects for future research in genetics, as revealed, for example, in Dobzhansky's and Demerec's concerns for how the NAS and government funding might reconfigure the practice of genetics in a significant way; (2) the confusion that existed, whether deliberate or not, on many of the foundational issues in genetics and the hazards of atomic testing, as revealed, for example, in the practices of AEC and administration officials when using a distorted version of the balance model of population genetics to argue the harmlessness or benefit of fallout from atomic testing; and (3) the notion that experimental data were generated and used to support positions, scientific and political, that were held prior to the experiments being done or in spite of them, as revealed, for example, by the efforts of the U. S. government to force scientific results into their policy positions, *and* by the practices of geneticists themselves in devising and interpreting experiments to fit pre-conceived theoretical positions. These positions were not only severely underdetermined by the experimental data, but were also, at least for some of the experiments of Dobzhansky and Wallace, held prior to the conducting of the experimentation. As Lewontin argues for Dobzhansky (see Chapter VI above), many of Dobzhansky's experiments were devised not to test for natural selection, but were instead demonstrations of selection in action for particular traits. Natural selection and adaptationism were assumed to be operating, but those principles were not themselves what was subject to experimental testing. In the final section of Chapter VIII below, I argue a similar point for Wallace regarding heterosis. If this argument is successful, it supports the notion that scientific experimentation itself is not only embedded in a value-laden context, its future direction is influenced by factors that are not epistemic in the traditional sense (cf. the story on plutonium hazards and exposure standards told by Whitemore 2001). Moreover, it turns the traditional and widely-held Baconian inductive model for experimental testing on its head, and shows that the conduct of science and the conduct of policy are not so far

removed from each other.

Finally, in order to bring this story to the events analyzed in the next chapter, it is important to note the changing power situation, beginning at least with the 1957 Congressional hearings on fallout (JCAE 1957), regarding scientists' political power, *and* regarding cultural attitudes towards radiation hazards. As indicated above, these hearings had an influence on Eisenhower's decision to push for a test-ban treaty. Along with the successful development of the technology to conduct tests underground, and with the chaotic policy situation concerning radiation exposure guidelines, the 1957 fallout hearings, which gained national and international attention, served to push the administration to accept the reality that a ban on atmospheric testing must be attained. On the one hand, the rise of the political power and activities of scientists, as revealed by the efforts of the Federation of American Scientists (FAS) to help solicit testimony from geneticists who held the classical view, such as Muller, is another changing component of the story. On the other hand, in the period from 1958 to 1965, radiation hazards were instantiated and accepted as cultural risks, in the same way that automobile accidents and pollution are accepted; rather than eliminating the risks, the new problematic was to reduce them or make them acceptable or tolerable. This "normalizing" tendency, as Foucault put it, was met with resistance, including from scientists organizing for political power. In the next chapter, I show that scientists' groups flourished in the late 1950s and early 1960s, and they began to attain political power by organizing their activities; indeed, they were included in Congressional hearings on radiation and fallout in 1959, 1960, and 1965, at the same time that radiation risks were being encultured as acceptable and normal. This type of political organizing activity among scientists, which continues to this day, illustrates another component of the technological infrastructure of science—the support system of science—and in addition supports the notion that the epistemic and the political are not sharply separated in practice.

## CHAPTER VIII

### **Atomic Fallout and its Epistemic/Political Fallout, 1959-1965**

In order to find out how and why history is both—factual and fictional, empirical and meaningful—one has systematically to take into account its narrative character. As a narration[,] history is a part of the cultural orientation that human activity and suffering require. Historical narration is a part of social communication within which it gains and unfolds its mental power. “Historical culture” is the very field of human life where history is a part of social reality and not only a reflection on it.

—Jörn Rüsen<sup>241</sup>

In this chapter, I continue the narrative of the previous chapter, a narrative designed using the principles of the technological infrastructure of science as developed in previous chapters, and a narrative designed to illustrate the technological infrastructure of science in the history of radiation genetics. First, I focus on the Federal Radiation Council (FRC), an advisory committee created by President Eisenhower in 1959 in response not only to the chaos created by the lack of coherent policy on radiation hazards, but also in response to the mounting scientific, political, and public pressure put on the AEC and the Administration to stop atmospheric atomic testing and to protect the public from fallout. I argue that the narrative illustrates that, on the one hand, scientists and their emerging organizations gained a degree of political power in their efforts to use their expertise to influence national policy. On the other hand, the narrative shows that the risks from radiation presupposed by the technologies, industries, and politics of the rapidly changing atomic age—including atomic testing, planned nuclear-powered air and spacecraft, planned nuclear excavation technologies, privately-owned commercial nuclear power plants, and medical techniques using radiation (e.g., x-rays) or radionuclides—became encultured in American society in the period from 1957 to 1965 in much the same way as did the health effects of cigarette

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<sup>241</sup> Rüsen (2005), p. 4. Rüsen (2005) states that history “is an interpretation of the threatening experience of time. It overcomes uncertainty by seeing a meaningful pattern in the course of time, a pattern responding to human hopes and intentions. This pattern gives a sense to history. Narration therefore is the process of making sense of the experience of time” (p. 10).

smoking.<sup>242</sup>

While part of this reconfiguration of cultural risks clearly resulted from the Atomic Energy Act of 1954, marking the U. S.'s relinquishment of its monopoly on fissionable material and thus paving the way for commercial nuclear power, it is nevertheless too easy to use hindsight and argue that this discontinuity—this change in what one's culture considers to be "normal"—has only to do with the emergence of nuclear power, which might have reached widespread development in the U. S. were it not for technological problems, some political opposition, and finally the partial meltdown of the Three Mile Island nuclear powerplant in 1979. There were many other technologies involved, yet none of them should be seen as determining factors. Indeed, nuclear power is set to make a possible resurgence in the U. S. in the wake of concerns over energy dependence on foreign oil and its fluctuating costs,<sup>243</sup> thus suggesting that cultural and political contexts are just as important as scientific or technological possibilities. What is significant is that *both* material conditions and discursive articulations matter in any given context. The technological infrastructure specifies that they cannot be isolated from each other without abstracting and distorting the practices that interact with them; indeed, these practices cannot be understood coherently without those conditions.

Second, I undertake a re-evaluation of Dobzhansky's and Wallace's experiments on heterosis in the light of the historical context constructed, and in the light of the principles of the technological infrastructure of science. In particular, I argue that their commitment to heterosis can be characterized as an effectively unfalsifiable, if not untestable, metanarrative that provided avenues for future experiments, but that the main purposes of these

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<sup>242</sup> The story of the scientific data on cigarette smoking parallels the story of fallout guidelines in this dissertation. Significant data linking smoking to lung cancer dates back to 1950; the Public Health Service (PHS) acknowledged this as early as 1957, but did not recommend any action until 1964. However, no significant change in scientific data motivated this decision; the change came from pressure originating outside the PHS; see Mark Parascandola, Ph.D. (2001), "Cigarettes and the U. S. Public Health Service in the 1950s," *American Journal of Public Health* 91: 196-205.

<sup>243</sup> See, for example, Matthew L. Wald, "Uncertainty Surrounds Plans for New Nuclear Reactors," *The New York Times*, 4 June 2006, p. A25. The possible resurgence of nuclear power in the U. S. already exists, however, in a context of opposition and doubt; see, for example, Blaine Harden, "In the Northwest, Nuclear Power Takes a Hit," *The Washington Post*, 22 May 2006, p. A2; and United States Government Accountability Office, "Nuclear Regulatory Commission: Preliminary Observations on its Process to Oversee the Safe Operation of Nuclear Power Plants," GAO-06-888T, 19 June 2006.

experiments were not to test whether heterosis was in fact justified. Instead, heterosis was a preconceived position used to structure various experiments. That is, these experiments were designed to demonstrate heterosis and natural selection at work, not to test (i.e., to submit to potential falsification) the efficacy of heterosis as an explanation of the experimental results. Part of the accounting for this lies in the need to secure the prospects for future research. Heterosis provided these researchers with a form of Hershey's Heaven: future varieties of experiments that could be conducted under the rubric of heterosis, that would be "acceptable" to a component of the genetics community (e.g., would pass peer review), and that would receive funding, even in the face of the serious underdetermination of the competing versions of the classical/balance controversy by the experimental evidence. Moreover, in the case of Wallace, adhering to heterosis also provided him with two advantages: (1) the active support of his advisor, Dobzhansky, who was committed to it as an explanation of the genetic structure of populations, and (2) the financial support of the Atomic Energy Commission, who saw its political implications in the form of a scientific argument supporting the harmlessness or even benefit of an increase in low-level radiation, and thereby as support for the testing of atomic weapons and the development of other atomic technologies.

### **Fallout and the Federal Radiation Council**

After the 1957 Fallout Hearings, when confronted with mounting national and international pressure to ban permanently the atmospheric testing of atomic weapons, not to mention mounting pressure from scientists, including those who served on the committees charged with setting radiation exposure policy, the Eisenhower Administration saw itself on the defensive regarding protecting the public from the dangers of fallout. The Administration's response, after first implementing a test moratorium in late 1958, was to create in 1959 the Federal Radiation Council (FRC). Established by Executive Order of President Eisenhower and subsequent Congressional approval, the FRC was designed to advise the President with respect to radiation matters directly or indirectly affecting health, including matters pertinent to the general guidance of executive agencies by the President with respect to the development by such agencies of criteria for the

protection of humans against ionizing radiation applicable to the affairs of the respective agencies. The Council shall take steps designed to further the interagency coordination of measures for protecting humans against ionizing radiation. (Eisenhower 1959)

The FRC was initially composed of the Secretaries of Defense; Commerce; Health, Education, and Welfare (HEW); and the Chairman of the Atomic Energy Commission (AEC). Later, Congress added the Secretary of Labor. The FRC was to advise the President on matters concerning radiation standards and protection, and to coordinate government agencies in a coherent effort to provide an authoritative voice on radiation hazards.

At the time of the creation of the FRC, negotiations concerning a limited test-ban treaty were under way with the USSR (Seaborg 1981). The three nuclear powers (USA, USSR, and UK) had temporarily stopped atmospheric testing in late 1958.<sup>244</sup> Eisenhower's call for a test moratorium was in response to mounting international pressure against atomic testing and the health threat of radioactive fallout. However, the test-ban negotiations eventually failed (temporarily), and the USSR resumed testing in 1961, the U. S. in 1962. The period from the moratorium failure in 1961 to the implementation of the Partial Test-Ban Treaty (PTBT)<sup>245</sup> in late 1963, marks the most intense period of atmospheric atomic weapons

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<sup>244</sup> The moratorium on atmospheric testing was in direct response to increasing opposition to testing from the public and concerned scientists (Bundy 1988, p. 329; Divine 1978, ch. 10). After the Joint Committee on Atomic Energy (JCAE) hearings on radioactive fallout in late May and early June of 1957, President Eisenhower became convinced of the need for a verifiable test-ban. However, AEC Chairman Lewis Strauss and several government physicists, including Edward Teller and Ernest Lawrence, vigorously lobbied the President against this position, claiming that they could produce "clean" H-bombs—bombs with little or no radioactive fallout (Hewlett and Holl 1989, pp. 397-402). During the test-ban negotiations that proceeded while the test moratorium was observed, a number of issues caused the process to bog down, including the issue of verification (Gilpin 1962, chs. VI-IX; Dyson 1979, ch. 12; Seaborg 1981). Eventually, the USSR resumed atmospheric testing in September of 1961. In March of 1962, President Kennedy announced the U. S.'s intention to resume testing. On 25 April, the U. S. resumed testing with an H-bomb explosion in the Pacific (Mazuzan and Walker 1984, p. 261).

<sup>245</sup> Signed by over 100 nations on 5 August 1963, including the USA, USSR, and UK, the PTBT banned all nuclear explosions in the atmosphere, in outer space, and under water. Since 1963, two countries that did not sign the PTBT, France and China, have conducted atmospheric nuclear explosions. In addition, India, one of the original signatory nations, conducted one atmospheric test in 1974. France announced its intention to stop atmospheric testing in 1975; China in 1986. The last atmospheric test was China's test on 16 October 1980 (Norris and Ferm 1988). Since 1992, an international moratorium on all nuclear testing has been in place. China violated the moratorium with an underground explosion on 5 October 1993, prompting the Clinton Administration to threaten a resumption of tests (Lena H. Sun, "Atomic Test Carried Out by China: U. S. 'Regrets' Move, Prepares to Do Same," *The Washington Post*, 6 October 1993, p. A1). Nevertheless, negotiations for a comprehensive test-ban treaty began. When China again conducted a nuclear test (underground) on 10 June 1994, the U. S. reiterated its desire for a ban on all testing, yet the moratorium was scheduled to end in September of



testing in history (Norris and Ferm 1988).

More importantly, this period marks the institutionalization of the notion that radiation exposures resulting from the hazards of modernization (nuclear testing, commercial nuclear power, nuclear weapons stockpiling) were just as much a part of daily life as other risks societies normally tolerate, such as automobile accidents, plane crashes, and air pollution. Ulrich Beck's (1992) analysis of the hazards of modernization in our transnational, global reality provides the starting point for this particular component of the present narrative. Indeed, many of the tactics used by the federal government in general, and AEC spokesmen and scientists in particular, in order to argue the harmlessness or benefit of radioactive fallout, are used today in a variety of similar contexts: ozone depletion, global warming, cigarette smoking, pesticides in foods, nuclear power plants, mass-marketed pharmaceuticals, and so on. Beck (1992) warns that the risks of modernization represent a "globalizing tendency." As he states: "These risks and dangers pose a potential global threat which is supra-national and not class-specific" (pp. 200, 201).

### I. *Why the FRC?*

From a survey of Congressional hearings (JCAE 1957, 1959, 1960b, 1965) and other sources (Mazuzan and Walker 1984; JCAE 1960a, as examples), I argue that the FRC was, in effect, designed to prevent further public relations problems for the Eisenhower

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1995 (Daniel Williams, "Administration Criticizes Nuclear Test by Chinese: Fracture of Informal Moratorium is Feared," *The Washington Post*, 11 June 1994, p. A17). However, the Comprehensive Test Ban Treaty (CTBT) was ready for signatures in 1996, and on 24 September 1996, President Clinton signed the treaty while at the United Nations in New York (Alison Mitchell, "Clinton, at U.N., Signs Treaty Banning All Nuclear Testing," *The New York Times*, 25 September 1996, p. A1). The U. S. Senate rejected the CTBT in 1999; although the Bush (George W.) Administration has honored the test moratorium to this date, it has stated its opposition to it, its right to resume underground testing at any time, and its intention to introduce a new model of nuclear warhead, which may necessitate a new series of underground tests (United States Government Accountability Office, "Nuclear Weapons: Annual Assessment of the Safety, Performance, and Reliability of the Nation's Stockpile," GAO-07-243R, 2 February 2007). The CTBT cannot enter into force until all nuclear or potentially nuclear states have ratified it; India, Pakistan, and North Korea have not signed the CTBT, and several other nations, including China, Iran, and Israel, have signed but not ratified the treaty. Complicating matters recently has been North Korea's repudiation of the Nuclear Non-Proliferation Treaty and its refusal to give up its nuclear program, and Iran's refusal of inspections by the International Atomic Energy Agency (IAEA) and its stated intention to develop a nuclear enrichment program, while at the same time disavowing Israel's right to exist (David E. Sanger, "U. S. Said to Weigh a New Approach on North Korea," *The New York Times*, 18 May 2006, p. A1). On 9 October 2006, North Korea claimed it conducted its first nuclear test underground (David E. Sanger, "North Korea Says It Tested a Nuclear Device Underground," *The New York Times*, 9 October 2006, p. A1).

Administration, problems which had begun in 1954 when the *Bravo* H-bomb test in the Pacific irradiated American test personnel, native islanders, and the crew of a Japanese fishing boat, resulting in casualties and one death (Hewlett and Holl 1989, pp. 175-7; Lapp 1958). In the period from 1954 to the creation of the FRC, the AEC and the Eisenhower Administration battled to justify its atomic testing program, arguing that testing weapons designs was crucial to combating Communism and to maintaining the security of the free world (cf. Seltzer 1993). Intense national and international pressure, however, had developed in opposition to atomic testing, and this opposition had helped set the stage for politically realistic test-ban negotiations.<sup>246</sup> Scientists in the U. S. and around the world spearheaded

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<sup>246</sup> The call for a test-ban first emerged on the national political landscape during the election of 1956. Democratic presidential candidate Adlai Stevenson made it part of his platform, while Eisenhower dismissed it as a threat to national security (Hewlett and Holl 1989, p. 339). As indicated above, after the 1957 JCAE Fallout Hearings, Eisenhower became convinced of the political necessity of ending atmospheric atomic testing. Subsequent to the 1957 hearings, the record of the hearings (JCAE 1957), which includes detailed testimony and numerous reprints of scientific papers on fallout, became perhaps the definitive public document on radioactive fallout in the late 1950s. By the time of the 1959 JCAE Fallout Hearings, about 20,000 copies of the 1957 hearings had been distributed internationally (testimony of Dunham, JCAE 1959, pp. 15-16). In addition, international opposition to atmospheric testing was increasing. By 1958, the U. S. was conducting negotiations with the USSR on the possibility of detecting violations of a possible test-ban. In the summer of 1958, a technical conference was held on the thorny issue of verification (Humphrey 1959, p. 109; Rosenfeld 1960, pp. 138, 145-8). This conference led to test-ban negotiations in Geneva in the fall of 1958. Soviet and American scientists had agreed that detection of simple surface tests (down to 1 kiloton yield) was possible, but that tests in outerspace were harder to detect, and underground tests could be difficult to distinguish from earthquakes (Rosenfeld 1960, pp. 145-8). With appropriate inspections by both sides, it was deemed that violations were not likely to go undetected.

However, by early 1959 the situation had changed. An American committee of scientists was appointed by the President's Science Advisory Committee to evaluate the feasibility of the proposed inspection system. They concluded that it was "more difficult to identify underground explosions than had previously been believed" (quoted in Rosenfeld 1960, p. 149). On 5 January 1959, the White House issued a press release that effectively called into question the wisdom of the test-ban negotiations. On 6 January, *The New York Times* published an article based on the release, entitled: "U. S. Sees Loophole in Atom Ban Plan." Some suspected that government scientists, such as Edward Teller, were behind the apparent attempt to sabotage the negotiations (Humphrey 1959, p. 109). Senator Hubert Humphrey (1959), Chairman of the Senate Foreign Relations Subcommittee on Disarmament, stated in a speech on the Senate Floor on 20 January 1959, that the press release does not "[reflect] an accurate picture of what has happened" (p. 110). Humphrey's speech was an impassioned plea for test-ban negotiations to continue. His view was that "we must look at the advantages of a controlled suppression of nuclear tests in addition to assessing the risks. If a test ban agreement is reached it will be the first break in the arms race since it began several years ago. It could pave the way to further progress in reducing and controlling the weapons of war. I believe it would help reduce the terrible tension that now exists between the free world and the Soviet bloc. It may make possible a more rational discussion of other problems which divide the world into hostile camps. It would be a major political breakthrough, a major change in the concept of an Iron Curtain that is drawn between us and the Soviet people" (p. 113).

This, in part, was the political situation in which responsibility for radiation protection was debated during the test moratorium. The necessity of removing total control from the AEC was a given. However, there

this opposition, including many biologists and geneticists concerned about the health hazards of radioactive fallout. The FRC was, at least in part, an effort by the Eisenhower Administration to centralize and assume more control over radiation exposure policy-making by bringing it directly into the President's cabinet.

## II. *Pre-1959 Radiations Standards*

Prior to 1959, radiation policy recommendations were the responsibility of a plethora of national and international committees, with varying connections to the U. S. government. In the U. S., foremost was the National Committee on Radiation Protection (NCRP), which dates back to 1929; its predecessor, the Advisory Committee on X-Ray and Radium Protection, was created largely in response to deaths resulting from internal ingestion of radium by female workers who painted the luminous dials on watches (Martland *et al.*, 1925).<sup>247</sup> Created in 1946, the NCRP was a quasi-independent committee of scientists that recommended exposure limits for radioactive substances.<sup>248</sup> However, the NCRP itself had

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was disagreement on exactly where responsibility should lie. The FRC was born out of these considerations. A missing factor (or factors) in these developments can be found in trying to answer some related questions: why did the White House seemingly call into question the test ban negotiations, citing technical problems with verification and inspection? Why was the FRC created? Answers lie (in part) in considering other nuclear programs then under consideration, including nuclear missiles, airplanes, and spacecraft, factors not normally considered by historians in this context. This interpretation (on which more below) reinforces the conclusion that nuclear risks were becoming salient in American culture (and to some extent, at least, internationally) in this period, and it supports Beck's (1992) major conclusions.

<sup>247</sup> On the history of the "radium watch-dial painters" and the recognition by Harrison S. Martland, the medical examiner of Essex County, New Jersey, of the internal dangers of radioactivity, see Maria Rentetzi (2004), "From Technological Design to Political Technology of the Body: The Case of Radium Dial Painters," *Women's History Magazine* 48: 4-12; Claudia Clark (1997), *Radium Girls: Women and Industrial Health Reform, 1910-1935*, Chapel Hill: University of North Carolina Press; William D. Sharpe (1978), "The New Jersey Radium Dial Painters: A Classic in Occupational Carcinogenesis," *Bulletin of the History of Medicine* 52: 560-70; and Martland's (1929) follow-up studies, "Occupational Poisoning in Manufacture of Luminous Watch Dials," *The Journal of the American Medical Association* 92: 466-73, 552-59.

<sup>248</sup> Before the end of World War II, the NCRP's predecessor, a committee of the National Bureau of Standards, and hence the Department of Commerce, was a relatively independent body of scientists, medical specialists, and other experts who set guidelines for a wide array of situations involving radiation exposures and radioactive materials in general. As described in Chapter VII above, over the course of the 1950s, some scientists—including NCRP members, such as the physicist Herbert Parker and the geneticist Hermann J. Muller—criticized the NCRP for being dominated by AEC officials and for not taking genetic and population considerations seriously (Seltzer 1993, ch. II). For example, at the 1959 JCAE Fallout Hearings, Jack Schubert, Senior Chemist at the Argonne National Laboratory, questioned the makeup of the NCRP committee and its subcommittees. He accurately charged the NCRP with being dominated by medical specialists and physicists. His view was that more fields needed to be represented when considering standards for maximum permissible levels of

no legal jurisdiction or statutory power to impose its recommendations, short of specifically targeted state or federal legislation or regulation, even though most government agencies, including the AEC, normally adopted its findings (Seltzer 1993, ch. II).

This policy scenario created problems after the 1954 *Bravo* test engendered national and international concern over the health hazards of radioactive fallout. That is, while public safety from fallout was ostensibly the responsibility of the AEC, and perhaps the Public Health Service (PHS), no legal mechanism existed whereby the AEC's atomic testing program could be regulated with respect to fallout contamination. After *Bravo*, this policy situation, combined with the AEC Commissioners' obsession with secrecy, served to fuel the controversy over fallout hazards and to create what AEC Chairman Lewis Strauss called a "public relations problem" (Hewlett and Holl 1989, pp. 449-51).

Other radiation policy committees included the National Academy of Sciences (NAS)-National Research Council (NRC) Committee on the Biological Effects of Atomic Radiation (BEAR), which released its first report in 1956 (BEAR I 1956). This report lowered some maximum permissible dose (MPD) levels and consequently served to fuel the fallout controversy, even though most of its recommendations were consonant with those of the NCRP.

The International Commission on Radiological Protection (ICRP), the international counterpart to the NCRP, had little impact on the fallout debate until April 1959, when the NCRP *raised* its population MPD by a factor of 3 over the ICRP level (see testimony of Dunham, JCAE 1959, p. 30). More significantly, the NCRP raised *all* the salient strontium-90 (Sr-90) MPDs.<sup>249</sup> the MPD for atomic workers was doubled, and the maximum

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exposure to radioactive substances, including more from statistics, health physics, genetics, and radiology (JCAE 1959, pp. 1606-12).

<sup>249</sup> Strontium-90, a radioactive fission product chemically similar to calcium, has a radioactive half-life of about 30 years. Even before the 1954 *Bravo* test, when in 1953 talk of Sr-90 made its way into non-government scientific circles, the potentially global hazards of Sr-90 fallout contamination worried scientists, as ingestion of Sr-90 could result from its becoming concentrated in milk and ultimately in human bone (Commoner 1971, p. 47). This undoubtedly explains, at least in part, the secrecy that shrouded the AEC's Project Sunshine, an effort to ascertain the worldwide effects of atomic explosions, including the global health hazards of Sr-90 (AECU-3488, 1953). According to Ralph Lapp, a former government physicist and critic of AEC policy in the 1950s and 60s: "There is little question that it was worry over the unprecedented size of the *Castle* series of tests [which included the *Bravo* shot] to be carried out in the Spring of 1954 that led to the conference of experts at Santa Monica, California [held at the RAND Corporation] in July of 1953 [where

permissible level for milk was raised by 25% (Divine 1978, pp. 270-1).

The timing of this change in recommendations caused significant embarrassment for the Eisenhower Administration, for a number of reasons. First, Eisenhower had just announced (3 April 1959) the formation of a committee to study the possibility of transferring radiation protection responsibilities away from the AEC to another agency, possibly the Public Health Service (PHS).<sup>250</sup> This committee consisted of the heads of the AEC, HEW, and the Bureau of the Budget (Divine 1978, p. 270). Since one possibility was to have given the NCRP statutory status, the sudden change in NCRP policy—that is, its raising of maximum permissible dose levels (MPDs)—generated suspicion. These developments were almost certainly part of the motivation for the JCAE’s call for more

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Sunshine was created]” (Lapp 1959, p. 27). The policy of the AEC thereafter was to maintain secrecy regarding Sunshine and the atomic testing program in general (Commoner 1958, Hewlett 1981).

Project Sunshine, in large part orchestrated by the chemist and later AEC Commissioner and government spokesperson on fallout Willard Libby, was one of the research programs that made headlines in the mid-1990s after the Clinton administration’s extensive declassification of data on government-sponsored experimentation on humans and the creation in 1994 of the Advisory Committee on Human Radiation Experiments. The final ACHRE report was published as *Advisory Committee on Human Radiation Experiments—Final Report* (October 1995), Washington, D.C.: U. S. Government Printing Office, and available at <http://www.hss.energy.gov/healthsafety/ohre/roadmap/achre/report.html>; see also Charles C. Mann (1994), “Radiation: Balancing the Record,” *Science* 263: 470-3. Research involving human radiation experimentation was initially sponsored by the AEC’s Division of Biology and Medicine (DBM) and carried out at the University of Chicago and elsewhere. Most Sunshine documents were classified secret, and the research included the assay of the Sr-90 content of human stillbirths; see, for example, Libby (1956c), “Radioactive Strontium Fallout,” *Proceedings of the National Academy of Sciences of the United States* 42: 365-390; see also Lapp 1959, p. 28. In the openness of the Clinton era, this research came under fire as the Department of Energy (DOE, successor agency to the AEC) reported that the parents of the stillborn children were “probably not notified or asked permission for the use of their children in the experiments. . .” (Gary Lee, “Stillborn Babies Used In ’50s Radiation Test: Energy Dept. Widens Disclosure of Experiments,” *The Washington Post*, 3 May 1994, p. A3). AECU-3488, the Project Sunshine report, was declassified (although amended to exclude still classified data) on 25 May 1957, two days before the 1957 JCAE Fallout Hearings began (Lapp 1959, p. 27).

<sup>250</sup> Eisenhower’s decision to consider stripping the AEC of radiation protection responsibilities was clearly the result of pressure from widespread criticism of the AEC and from mounting revelations concerning fallout levels. Prior to this decision, the Surgeon General had formed a committee to study the federal government’s role in radiological health concerns, the National Advisory Committee on Radiation (NACR). The NACR recommended transferring authority for control of radiation hazards to another agency, such as the Public Health Service, which was then under the Department of Health, Education, and Welfare, HEW (*Science*, 1 May 1959; Mazuzan and Walker 1984, pp. 250-3). The PHS had just started monitoring the U. S. milk supply for Sr-90 levels in July of 1958, and the frenzy of tests in late 1958, before the test moratorium went into effect, produced marked increases in fallout levels in early 1959 (see testimony of Charles Dunham [Director of DBM, AEC], JCAE 1959, pp. 10-49; Francis J. Weber [Chief of Division of Radiological Health, PHS], *ibid.*, pp. 155-166; Luther Lockhart, Jr., “Current Information on the Radioactivity of Air,” U.S. Naval Research Laboratory, Washington, D.C., *ibid.*, pp. 483-9).

hearings on fallout in early May of 1959.

Second, fallout levels had increased dramatically in early 1959 due to the flurry of atomic testing by both the U. S. and the USSR before the test moratorium went into effect in late 1958 (Divine 1978, ch. 10). Sr-90 levels in milk had risen, and several scientists' groups criticized the AEC's atomic testing policy and called for more information on the hazards to fetuses, infants, and pregnant women.<sup>251</sup> For example, the Greater St. Louis Citizen's Committee for Nuclear Information (CCNI) began collecting baby teeth in order to measure accurately Sr-90 intake by infants (Divine 1978, p. 268). Studies by Alice Stewart and her colleagues (1956, 1958) at Oxford University suggested that low levels of ionizing radiation—from fetal x-raying<sup>252</sup>—could lead to an increased incidence of cancer in children,

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<sup>251</sup> These scientists' groups included the Greater St. Louis Citizen's Committee for Nuclear Information (CCNI); the Radiation Hazards Committee of the Federation of American Scientists (RHC-FAS); the National Committee for a Sane Nuclear Policy (SANE); the Committee on Environmental Hazards of the American Academy of Pediatrics (CEH-AAP); and the Scientists' Committee for Radiation Information (SCRI), of New York City. None of these groups testified at the 1959 JCAE Fallout Hearings, but articles and reports were included for the record. Barry Commoner (1958), Professor of Botany at Washington University, St. Louis, and a member of the CCNI, gave perhaps the most forthcoming analysis of the potential hazards of global radioactive fallout, along with the scientific and political problems they presented. According to Commoner: "Anyone who attempts to determine whether or not the biological hazards of world-wide fallout can be justified by necessity must somehow weigh a number of human lives against deliberate action to achieve a desired military advantage. Such decisions have been made before—for example, by military commanders—but never in the history of humanity has such a judgment involved literally every individual now living and expected for some generations to live on the earth" (JCAE 1959, p. 1024). Moreover, the first Pugwash Conference, where scientists debated the hazards of atomic energy and atomic war, had been held in Nova Scotia in 1957 ("Pugwash Statement," *Science* 126: 199-200), and scientists' groups, organizing to influence policy on weapons development, radiation hazards, nuclear war, civil defense, and nuclear energy, flourished in the late 1950s and 1960s. In addition to those mentioned above, these groups included Physicians for Social Responsibility (PSR), which formed in 1960 and grew rapidly. PSR succeeded in widely publicizing the potential scientific and medical aspects of nuclear war, and they published an important series of articles in the *New England Journal of Medicine* in 1962 (Day and Waitzkin 1985, p. 647). Starting with the 1960 JCAE Hearings on Radiation Protection Standards (JCAE 1960b), members of scientists' groups were given a voice on Capitol Hill, although their treatment by some members of Congress was harsh and hostile.

<sup>252</sup> By the mid-1980s, fetuses were no longer routinely x-rayed, as the potential harm was judged to outweigh any advantage the image might confer; see Elizabeth B. Harvey, *et al.* (1985), "Prenatal X-ray Exposure and Childhood Cancer in Twins," *New England Journal of Medicine* 312: 541-5; see also Kevles 1997, p. 247. In addition, ultrasound technology often provides doctors with a satisfactory image of the developing fetus. In the period here investigated, x-raying of pregnant women was common, although there was emerging opposition to it. At the 1959 JCAE Fallout Hearings, the physician Austin Brues, Senior Biologist and Director of the Division of Biology and Medical Research, Argonne National Laboratory, when questioned about Stewart's (1956, 1958) research on fetal irradiation, responded by claiming that the x-raying of fetuses saves lives: "It is a safe guess that it probably saves a larger number of cases in labor than it might cost" (JCAE 1959, pp. 1389-91). Contrast this with the physicist Ralph Lapp's contention a year later that the practice is dangerous. According to Lapp, the medical profession have "not policed themselves well" in that

including leukemia, which was then incurable. Further leukemia studies by Edward B. Lewis (1957) of the California Institute of Technology, and a critical editorial praising Lewis's work by *Science* editor Graham DuShane (1957), provoked prompt responses from AEC-sponsored researchers, including Austin Brues (1958) of the Argonne National Laboratory, A. W. Kimball (1958) of the Oak Ridge National Laboratory, and Miriam Finkel (1958) at Argonne. These latter studies attempted to downplay the link between low levels of radiation and cancer. By January of 1959, Linus Pauling of the California Institute of Technology, whose book *No More War!* (1958) had already branded him as an anti-government activist, had joined the infant mortality controversy with a rejoinder to Finkel's (1958) paper (Kamb and Pauling 1959).<sup>253</sup> Hence, by the spring of 1959 the childhood leukemia debate, which lasted into the 1970s, was in full swing (see, e.g., Sternglass 1981). The NCRP's increase in allowable levels of Sr-90 in milk, as well as other MPDs, fanned the flames of the rising controversy and panic over increasing fallout levels.<sup>254</sup>

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women are not even asked if they are pregnant when x-rayed, and pregnant women are x-rayed even when in the third trimester (JCAE 1960b, p. 308). Later studies, for example, M. D. Becker and G. A. Hyman (1965), "Management of Hodgkin's Disease Coexistent with Pregnancy," *Radiology* 85: 725-28, showed more conclusively that fetal irradiation above about 10 rads can cause somatic abnormalities, including childhood cancer, although the issue remained contestable even into the 1980s; see Giulio J. D'Angio and Lourdes Z. Nisce (1973), "Problems With the Irradiation of Children and Pregnant Patients," *Journal of the American Medical Association* 223: 171-3, 177-8. On the history of medical imaging technologies, including ultrasound, see Bettyann Holtzmann Kevles ([1997] 1998), *Naked to the Bone: Medical Imaging in the Twentieth Century*, Reading, MA: Addison-Wesley; and Edward Yoxen (1987), "Seeing with Sound: A Study of the Development of Medical Images," in Bijker, Hughes and Pinch 1987, pp. 281-303.

<sup>253</sup> Lewis (1957) argued that even low levels of ionizing radiation can cause cancer, especially if the dose-response curve is linear and the measurable biological effects are extrapolated to low dose levels. Graham DuShane (1957), the editor of *Science*, praised Lewis' study for making it "possible to calculate the probability of death from leukemia as a result of any particular dose of radiation." His somber assessment was that "the atomic dice are loaded" (p. 963). Lewis won the Nobel Prize for unrelated work in 1959.

<sup>254</sup> Dobzhansky himself entered this debate with a letter he and his close friend Leslie C. Dunn (1958) wrote to the editor of *Science* in response to Finkel's (1958a) article (*Science* 128: 1534). Finkel's article, which made headlines in the media, studied the effects on relatively small numbers of mice, cats, and dogs of intravenous injections of Sr-90 at different dose levels. The smallest group size was 150 mice, and the three largest groups, which corresponded to the smallest injected doses, did not result in statistically significant decreases in life span. Although the article did analyze the problems of extrapolating mice data to humans, it indicated that the smallest dose level that gave a statistically significant decrease in life span corresponded to a retained dose of about 5  $\mu\text{C}/\text{kg}$ , which corresponded to about 350  $\mu\text{C}$  in a 70 kg man, or "to 350 times the maximum permissible body burden for people engaged in atomic energy work and to 3500 times the level set for the general population" (p. 639). After estimating the threshold dose level in humans that would begin to have pathological effects—between 5 and 15  $\mu\text{C}$ —the article concluded that "the present contamination with strontium-90 from fallout is so very much lower than any of these levels that it is extremely unlikely to induce even one bone tumor or one case of leukemia" (p. 641).

Third, the declassification and subsequent public availability (in 1958) of the original Project Sunshine report (AECU-3488)—detailing the AEC’s research efforts on measuring global fallout—aroused suspicion concerning the administration of the research program and the extent to which the AEC was withholding pertinent data on fallout contamination and biological effects. In a scathing article in *Bulletin of the Atomic Scientists*, Ralph Lapp (1959) blasted the AEC project, designed to forecast global Sr-90 levels and its intake by humans. Lapp described the birth of the secret project as follows:

Consider the situation in the summer of 1953. Scientists were alarmed by preliminary measurements which indicated strontium-90 as a global culprit. Some worried about the prospect of a tenfold increase in the world-wide Sr-90 levels which the 1954 test series would bring about. They met in secret; they made secret recommendations; there is no record that they urged that the *Castle* series of tests be restricted, or that this unique world health problem be treated on an international basis. It was assumed that the global retention in humans of one microcurie of Sr-90 was harmless. The majority of the conferring experts were not qualified in the field of biological science, and only one was recognized as competent in radiation health. (pp. 27-8)

In addition, an article in *The New York Times*<sup>255</sup> on 29 March 1959, just 5 days before Eisenhower’s call for a reevaluation of the AEC’s radiation responsibilities, ridiculed Project Sunshine, arguing that the AEC “has no one directly in charge of its program for measuring the extent and hazards of atomic fallout.” The article characterized the situation as follows:

The lack of clear authority and direction over the Commission’s [AEC] multi-million-dollar study of fallout is looming as one of the major problems facing the

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In response, Dunn and Dobzhansky (*Science* 128: 1534, 1958) wrote that Finkel “propounds very sweeping conclusions on the lack of danger from small doses of ionizing radiations, and particularly from strontium-90 fallout. An examination of the assumptions upon which these conclusions rest is called for. The chief of these is that the main danger of radiations in man’s environment lies in their effects on the individuals exposed.” They argued that a “neglect of elementary methods of critical examination of evidence leads us to doubt not only Finkel’s main conclusion” that no tumors or cases of leukemia could be caused by fallout, “but also the rationale on which the work was based. Surely understanding of the effects of radiations on populations of organisms, including man, is not likely to be advanced by willful neglect of one of the well-established effects of radiation [the genetic effects of radiation].” Finkel (1958b) responded to the several letters written (*Science* 128: 1534, 1580-82) and said of Dunn and Dobzhansky: “It is difficult to understand how two distinguished scientists could so misread my paper that they should accuse me not only of ignorance of the distinction between somatic and germinal radiation damage but also of ‘neglect of elementary methods of critical examination of my evidence’” (p. 1581). She concluded that “the fallout problem elicits such an emotional response that many otherwise sagacious and objective scientists lose their ability to read accurately and think clearly” (p. 1582).

<sup>255</sup> John W. Finney, “Fallout Survey Held Makeshift—AEC Spurs ‘Urgent Review of Split Authority and Confusion’ in Research,” *The New York Times*, 29 March 1959.



Commission as it seeks to defend its radiation research program against criticism from the administration and Congress.

Fourth, the NCRP revisions were clearly out of line with those of the ICRP, the NCRP's respected international counterpart, and these differences had generated much publicity (JCAE 1959, p. 30).<sup>256</sup> The significance of this difference lay in the fact that the ICRP took into account population exposures, while the NCRP effectively did not.

Gioacchino Failla, a prominent biophysicist at Columbia University and member of the ICRP, NCRP, BEAR I and II, and the ACBM, revealed the major policy reason the NCRP radiation limits were not lowered along with the ICRP values:

Designers of radiation installations have taken [radiation protection] seriously and have used substantial factors of safety, at least during the last 20 years. For this reason, compliance with the lower permissible limits in general has not necessitated marked changes in protective barriers or operative procedures. This refers to occupational exposure. The stringent limits for non-occupational exposure are more difficult to meet. In my opinion a further lowering of permissible limits would impose a heavy burden on the operation of radiation installations. (JCAE 1959, p. 1582)

Clearly, the political climate in which Eisenhower proposed that the AEC's radiation protection responsibilities be transferred was nothing short of hostile and chaotic. The Administration was on the defensive, and Congressional hearings on fallout were immanent. The 1959 JCAE Fallout Hearings brought out much of the underlying decision-making involved, as well as the various controversies and disagreements among policymakers and scientists. It is not surprising, then, that Eisenhower subsequently opted not to transfer radiation protection responsibilities to the PHS or the NCRP, but instead created the FRC, a committee consisting of his own cabinet members. Nevertheless, the decision to consolidate

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<sup>256</sup> To make matters worse, an article appeared in the *Washington Post* while the 1959 hearings were underway, stating that Sr-90 levels in New York City bread had exceeded permissible levels (Edward Gamarekian, "AEC Reveals New York Bread Exceeded Strontium-90 Limit," *Washington Post and Times Herald*, 7 May 1959). The reporter was called to testify at the hearings, and defended his research (JCAE 1959, pp. 1560-62). During subsequent testimony by other NCRP and ICRP members, there was disagreement on whether the numbers used by Gamarekian were accurate (Roundtable Discussion, JCAE 1959, pp. 1600-13). Karl Morgan, Chief Health Physicist of the Oak Ridge National Laboratory, calculated that the bread levels had reached 66% of the NCRP limit, but had certainly exceeded the ICRP limit (pp. 1600-1). Walter Selove, past Chairman of the Radiation Hazards Committee of the Federation of American Scientists, objected, arguing that Morgan's calculations assumed bread was the *only* source of Sr-90 (pp. 1601-2).

power over radiation standards and effectively take them out of the control of scientific experts, eventually caused more problems for the administration.

### III. *Problems with the FRC*

After the 1959 fallout hearings and the creation of the FRC in August of 1959, the “spring scare” over fallout subsided. Sr-90 levels were falling, and the test moratorium continued, despite a stalemate in test-ban negotiations with the USSR (Divine 1978, pp. 277-8, 281). In 1960, public concern over fallout continued to wane, even if the test-ban negotiations made little progress. Meanwhile, in February of 1960, the FRC *lowered* the maximum permissible level of Sr-90 in milk. As Divine (1978) points out, “the government tacitly admitted that the problem was more serious than it had previously acknowledged” (p. 319).

However, other developments caused hopes for a test ban eventually to fade. On 1 May 1960, the “U-2 incident” occurred, in which the USSR shot down a U. S. spy plane over USSR territory, capturing the pilot alive. At the Paris summit meeting in mid-May, Khrushchev walked out, effectively ending chances of a test-ban treaty during the Eisenhower administration (Divine 1978, pp. 313-14). Moreover, there was still sizeable opposition to a test-ban within the government. Opponents included the secretaries of state and defense, AEC Chairman John McCone, and a number of prominent scientists, including the influential physicists Edward Teller and Freeman Dyson<sup>257</sup> (ch. 11).

The FRC came under fire as well, particularly from scientists’ groups and members of the radiation policy committees, such as the NCRP and ICRP. Concern focused on whether the FRC guidelines applied to fallout at all, and on the dissemination of fallout information to the public. Such was the climate when on 5 May 1960, the JCAE announced its intention to

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<sup>257</sup> Dyson initially opposed a test-ban. In an article in *Foreign Affairs* in April of 1960, Dyson implicitly argued for an end to the test moratorium, claiming that the U. S. must develop the neutron bomb (a supposedly “clean” tactical nuclear weapon that would produce little fallout), especially since the USSR was already developing the technology (Freeman J. Dyson, “The Future Development of Nuclear Weapons,” *Foreign Affairs* 38: 457-64). Dyson’s article caused a stir, and was met with prompt responses by test-ban advocates (Divine 1978, pp. 304-6). Later, as a physicist with the Arms Control and Disarmament Agency, Dyson supported the test-ban and was actively involved in the test-ban efforts (Dyson 1978, ch. 12; Dyson 1984, ch. 14).

hold hearings on radiation protection criteria and standards (JCAE 1960b, pp. 3-5). The record of these hearings provides a wealth of information on the FRC and the debates surrounding its initial recommendations.

One major issue regarding FRC recommendations was whether they actually applied to radioactive fallout at all. The FRC was created, at least in part, to deal with the thorny issue of fallout. Yet, as Walter Selove of the FAS noted at the 1960 hearings, the ICRP and NCRP “have been aware of the problem of dealing with fallout levels, but have felt that this particular problem lay outside their competence. . .” (JCAE 1960b, p. 46). Selove presented the overall problem as follows:

I think this is an extremely important point and one which has caused a great deal of confusion in the public, namely, that there has really been no body which considers itself to be competent or authoritative in recommending what is an acceptable level of fallout. (p. 46)

Ralph Lapp held a similar position and gave the following recommendation to the FRC:

The Federal Radiation Council should prepare and publish an expository statement on fallout, complete with technical appendixes, surveying all aspects of the problem. The American people need such a document and it is long overdue. The technical data are now out in the open, due almost entirely to the investigations of the Joint Committee on Atomic Energy, but they have not been integrated into a simple and understandable exposition. (JCAE 1960b, p. 330)

A second criticism of the FRC involved its statutory status. Although Congress amended the original Presidential Executive Order of 14 August 1959 in order to give the FRC statutory status,<sup>258</sup> the result was that the FRC still had no legally binding authority to impose *any* radiation standards. Consider the following exchange between Senator Bourke Hickenlooper and Elmer Staats, Deputy Director, Bureau of the Budget:

**Senator Hickenlooper:** Mr. Staats, I am not quite clear on your answers to the questions a moment ago about the regulations of the Council. I would like to ask one or two questions to clear the matter up.

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<sup>258</sup> For the original executive order, see Eisenhower (1959). Later, President Eisenhower signed Public Law 86-373, introduced as S. 2568 by Senator Clinton Anderson, Chairman of the JCAE, on 23 September 1959. This made the status of the FRC statutory, added the Secretary of Labor to the Council, and authorized the Council to consult outside experts, including those from the NAS, NCRP, and from the fields of biology, medicine, and health physics (see statement of Arthur S. Flemming, HEW Secretary and Chairman of the FRC, JCAE 1960b, p. 108).

Does the Council have any authority to issue regulations?

**Mr. Staats:** Not the Council itself. They are really issued by the President. The Council could not take any action on its own with any force of directive. I think this has to be very clear.

**Senator Hickenlooper:** If the Council makes a finding or a recommendation, or whatever action the Council takes and whatever you want to call it, and no other governmental official, such as the President or someone else, issues a regulation or an order based upon that—that is, if no action is taken by any other governmental agency or official on any recommendation of the Council, does that regulation of the Council or action of the Council have any force or effect?

**Mr. Staats:** Only such force and effect as people who participate in the Council want to agree on.

**Senator Hickenlooper:** I am asking about the official effect. Maybe I could make it a little more clear. The Council meets. It arrives at certain conclusions so far as the Council itself is concerned as an entity. It recommends that certain actions be taken. Nothing is done by any agency. Nothing is done by any Federal official pursuant to that action of the Council. Does that have any legal force or effect?

**Mr. Staats:** No, sir.

. . . . .

**Senator Hickenlooper:** . . . All I am trying to do is to just define clearly whether this Council has any right to come to any conclusions on its own which, without implementation by any other official in the Government or any other Governmental agency, has the force and effect of law.

**Mr. Staats:** The answer is “No.” (JCAE 1960b, pp. 104-5)

The statutory status of the FRC and its radiation standards raises pertinent questions regarding the purpose and mission of the FRC. Why was it created? Why did it not have the force of law, and why did it continue to balk at dealing with the problem of radioactive fallout? First, previous embarrassment caused by the uncertainties involved in predicting fallout levels contributed to the administrative makeup of the FRC. Guidelines could be changed and revised in the light of new developments, and there were still no legal limits set that might cause trouble or embarrassment for the Administration or its agencies. Additionally, a mechanism was built in to the FRC law that provided for “exemptions” to any future radiation guidelines set (see testimony of Paul Tompkins, U. S. Naval Radiological Defense Laboratory, JCAE 1960b, p. 409). For a particular agency to justify violating a FRC Protective Action Guide (PAG)—the FRC’s radiation standard reports—it need only notify the FRC of its intention to do so.

Second, since the formal members of the FRC were all members of the President's cabinet, the FRC could claim executive privilege with respect to any documents requested by Congress or the general public. Representative Chet Holifield, Chairman of the JCAE Special Subcommittee on Radiation, complained several times at the 1960 hearings that the FRC denied him documents, citing executive privilege (JCAE 1960b, pp. 131, 143-4, 532-3). Holifield stated that the JCAE

wonder what position we are going to be in in the event that there is action by the Council of an important nature, setting bracket guides of some kind, and if we would like to have information as to how those guides were arrived at. (pp. 532-3)

Clearly, the FRC continued the U.S. government's obsession with secrecy concerning weapons testing and the extent and hazards of radioactive fallout, and amounted to an effort at damage control.

Third, the FRC provided no institutionalized mechanism for protesting any of its Protective Action Guides. As Charles Williams of the Harvard School of Public Health stated, one "would assume that one must go to the President to get redress. I don't see how we are going to get around the particular problem" (JCAE 1960b, p. 130). FRC Chairman Flemming confirmed for the Joint Committee that the Council has "not formalized lines of protest" (p. 531). Indeed, the issues of executive privilege and legal recourse served to insulate politically and legally the FRC and the Administration should atomic testing resume or should other nuclear projects require political justification.<sup>259</sup>

#### IV. *Nuclear Missiles, Planes, and Rockets*

One factor not normally considered by historians in connection to radiation protection standards is other nuclear projects, including those for proposed nuclear missiles, planes, and

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<sup>259</sup> The FRC was legislated out of existence when, on 2 December 1970, President Nixon's newly-created Environmental Protection Agency (EPA) took over most FRC responsibilities. However, many other governmental agencies continue to share a wide range of radiation protection responsibilities, including the DOE, NRC, DOD, DHS, DHHS, DOC, DOL, DOT, and FEMA. A 2000 EPA report concluded that "new reviews of radiation risks by the NAS found that radiation risks were significantly higher than had been assumed by the FRC in 1960. By 1986, it had become apparent that the old FRC limits were anachronisms that should be addressed" (U. S. EPA, *Radiation Protection at EPA: The First 30 Years*, Office of Radiation and Indoor Air, EPA 402-B-00-001, August 2000; quotation from p. 11). All of the FRC Reports (or PAGs) are available online at <http://www.epa.gov/radiation/pubs.htm>.

spacecraft.<sup>260</sup> Moreover, efforts to save the existence of these projects and to justify their inevitable radiation releases may have been at the heart of the motivations to move radiation policy to the FRC, rather than another federal agency. That is, even if a ban on atmospheric testing of nuclear weapons was considered inevitable in 1960, there still remained the problem of justifying the radiation releases of other nuclear planned programs, in addition to the radioactive fallout levels that would still be present as a result of further deposition of fallout from the atmosphere and that already on the ground or in the food, water, and milk humans ingest.

At the 1960 JCAE hearings, some discussion was devoted to the Department of Defense's nuclear programs. An AEC representative submitted an overview of these programs in the context of the NCRP and ICRP standards.<sup>261</sup> Following this, Dr. Paul C. Tompkins, Scientific Director of the NRDL, gave a defense of the nuclear projects in the light of the FRC (JCAE 1960b, pp. 399-410). These projects included Aircraft Nuclear Propulsion (ANP), or the manned nuclear aircraft; Project Pluto, the ceramic nuclear reactor developed at the Lawrence Livermore National Laboratory, which was to be used in these projects; SLAM, the supersonic low-altitude missile; Project Rover, the nuclear rocket; Project Orion, the nuclear spacecraft; and Project Plowshare, the project to use nuclear explosions for excavation and other "peaceful" purposes.<sup>262</sup> According to Tompkins,

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<sup>260</sup> When considering the history of the AEC and the biological effects of radiation, historians routinely point to the research and development of commercial nuclear power plants as the main forum for public and internal opposition to policy (see, for example, Najarian 1978; Walker 1989, 1993, 1994, 2000, 2004; Hacker 1992). While this may account for many of the more public debates, the brief and not so public histories of other nuclear projects may turn out to have played a major role in the PTBT negotiations and, in particular, the FRC and its efforts to control radiation policy (cf. Dyson 1978, chs. 10, 12; see Hacker 1994, chs. 8, 10 for some discussion of the AEC's other nuclear programs).

<sup>261</sup> Lt. Col. Joseph A. Connor, Jr., Chairman, Aerospace Nuclear Safety Board, AEC, "Relationships Between Aerospace Nuclear Safety and NCRP and ICRP Standards," 1 June 1960, JCAE 1960b, pp. 395-8.

<sup>262</sup> Freeman Dyson (1984), who worked on Project Orion from 1956-59, described these nuclear programs as "welfare programs for scientists and engineers." (p. 62) On the nuclear aircraft, Dyson recalled that "nobody could think of anything useful to do with it" (*ibid.*). At \$50 million apiece, the nuclear ramjet SLAM was in development at the Livermore National Laboratory from 1957-1964 (Gregg Herken, "The Flying Crowbar," *Air and Space Magazine* 5:1, April/May 1990, p. 28). Developed by Chance Vought Aircraft, one version of SLAM was 5 ft. in diameter, 65 ft. long, and 45,000 lbs.; had a payload of 16 1-megaton hydrogen bombs; a top speed of mach 3 (about as fast as a rifle bullet, 2280 mph) and as loud as 150 db; was powered by a 600 megawatt nuclear reactor and would emit fission fragments as it flew; had a final cruising altitude of 100 ft.; had a surface temperature of 1,000° F; and was deemed ready for testing in the Pacific Ocean when it was cancelled in 1964 ([www.globalsecurity.org/wmd/systems/slam.htm](http://www.globalsecurity.org/wmd/systems/slam.htm), accessed 5 February 2007). Dyson (1984)

the adoption of the [FRC's] "Radiation Protection Guide" has established the basis by which the design and operation of nuclear devices important to the DOD program can be guided. The eventual judgment must be made on the basis of the way these operations contribute to the annual population dose which is limited by the provisions of the guide. Considerations of total quantities and frequency associated with the loss of radioactive materials will have to be added to the existing safety criteria. (p. 402)

Tompkins had been a scientific consultant to the JCAE at its hearings in 1957 and 1959, while serving as Scientific Director of the NRDL since 1952. From 1960 to 1961, Tompkins was Chief of the Research Branch of the Division of Radiological Health of the PHS; he then served as the AEC's Deputy Director of the Office of Radiation Standards, from 1961 to 1962. In 1962, Tompkins was appointed the first Executive Director of the FRC. Tompkins' insider knowledge of various agencies, and particularly his extensive experience with the aerospace nuclear programs, suggest that his appointment to the FRC was, at least in part, designed to maintain these programs in the face of resumed atmospheric testing *and* the possibility of a test ban. In a letter to AEC Commissioner Leland Haworth, dated 25 September 1962—after both the USSR and U. S. had resumed atmospheric testing after the moratorium—Tompkins described his policy strategy as follows:

If any reasonable agreement on this subject [of radiation standards] can be reached among the Agencies, the basic approach to the [FRC] report would be to start with a simple, straightforward statement of conclusions. It would then be a straightforward matter to select the key scientific consultants whose opinions should be sought in order to substantiate the validity of the conclusions or recommend appropriate modifications. (quoted in Morgan 1987, p. 138)

What this indicates, beyond a possible partial interpretation for the creation and early operation of the FRC, is that in this period risks from radiation exposures from nuclear

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described SLAM as a nuclear missile that "was supposed to scream along at two or three times the speed of sound, a few hundred feet above the ground, generating shock waves strong enough to flatten buildings on either side of its track. It would do so much damage while cruising around over an enemy country that it hardly mattered whether it carried a warhead or not" (p. 62). But, Dyson recalled: "Where could it be based and tested? What specific missions would it be good for?" (p. 63). On these nuclear projects, see George Dyson (2002), *Project Orion: The True Story of the Atomic Spaceship*, New York: Henry Holt and Co.; Scott Hirsch (2005), *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving*, New Brunswick: Rutgers University Press; Kenneth Franklin Gantz (1960), *Nuclear Flight: The United States Air Force Programs for Atomic Jets, Missiles, and Rockets*, New York: Duell, Sloan, and Pearce; and H. M. (1960), "The Atomic Airplane: 'This Program Has Had a Very Irregular History,'" *Science* 132: 658-9. According to Freeman Dyson (1965), "the cancellation of Project Orion was "the first time in modern history that a major expansion of human technology has been suppressed for political reasons" (p. 144).

technologies had become part of American culture. While some technologies were doomed to eventual failure, such as all the aerospace nuclear programs, Project Plowshare, and widespread commercial nuclear power plants (at least in the U. S.; see Walker 1992), nuclear technology in general and the risks they posed were compared to other common risks—they had become incorporated into our culture of risk.<sup>263</sup>

In addition, Tompkins' statement highlights the epistemological status of the data on fallout hazards, and the strategy of finding and fitting evidence into pre-existing theories or policy positions. In Chapter VI above, the epistemological status of adaptationism was explored, and a strategy similar to Tompkins' was presented in the context of Lewontin's interpretation of Dobzhansky's work. What is significant about these themes is that they seem to permeate the historical narratives here and in the previous chapter. The underdetermination of population genetics theories by the experimental data is ubiquitous, while the searching for evidence to support various scientific or policy positions held—whether that was heterosis as an interpretation of the genetic structure of populations, or the harmlessness of fallout—also is a common theme, in quite contradistinction to our currently held ideals of the “scientific method” or of proper or ethical behavior in conducting scientific experimentation.<sup>264</sup> This dissertation suggests that these often-promulgated ideals do not

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<sup>263</sup> The BEAR reports of the National Academy of Sciences were later named BEIR, Biological Effects of Ionizing Radiations. BEIR VII, Phase 2 (2006) was released on 29 June 2005 and the panel confirmed that “current scientific evidence is consistent with the hypothesis that there is a linear dose-response relationship between exposure to ionizing radiation and the development of radiation-induced solid cancers in humans. The committee further judges it unlikely that a threshold exists for the induction of cancers but notes that the occurrence of radiation-induced cancers at low doses will be small. The committee maintains that other health effects (such as heart disease and stroke) occur at high radiation doses, but additional data must be gathered before an assessment can be made of any possible connection between low doses of radiation and noncancer health effects. Additionally, the committee concludes that although adverse health effects in children of exposed parents (attributable to radiation-induced mutations) have not been found, there are extensive data on radiation-induced transmissible mutations in mice and other organisms. Thus, there is no reason to believe that humans would be immune to this sort of harm” (Committee to Assess Health Risks From Exposure to Low Levels of Ionizing Radiation, Board on Radiation Effects Research, Division of Earth and Life Studies, National Research Council, *Health Risks From Exposure to Low Levels of Ionizing Radiation: BEIR VII, Phase 2*, Washington, D.C.: National Academies Press, p. 10, available at <http://books.nap.edu/openbook.php?isbn=030909156X>).

<sup>264</sup> For recent examples of arguments that invoke the ideal of interpretations or decisions being made on evidence alone, as opposed to predetermined scientific or policy positions, but which ironically show that this ideal does not hold in practice, see Juliet Eilperin, “USGS Scientists Object To Stricter Review Rules: Pre-Publication Policy Seen as Cumbersome,” *The Washington Post*, 14 December 2006, p. A29; Jerry Avorn, M.D., “Dangerous Deception—Hiding the Evidence of Adverse Drug Effects,” *The New England Journal of*



hold in practice, whether that be scientific practice or science policy practice. These themes will be explored below in the concluding section of this chapter, as a way to provide a more or less unifying interpretation of the intersecting narratives involving the genetic effects of fallout, in general, and of Wallace and Dobzhansky's efforts, in particular. First, however, the narrative concerning the enculturation of nuclear risks into modern culture must be completed.

#### V. *The Risks of Modernization*

Following the resumption of atmospheric atomic testing by the USSR in late 1961 and by the United States in early 1962, fallout levels again began to rise. Some of the levels did not peak until 1963 or 1964, well after the PTBT went into effect.<sup>265</sup> The FRC again came under fire in 1965, when the JCAE called for hearings on the FRC's Protective Action Guides (JCAE 1965). New problems had emerged concerning fallout, including contamination of the Arctic food webs, and concern over another critical radionuclide present in fallout and seen as a hazard to infants, Iodine-131 (I-131), which accumulates in milk from bovine grazing and concentrates in the thyroid gland.<sup>266</sup> Several representatives from

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*Medicine* 355: 2169-71, 23 November 2006; David J. Graham, M.D., M.P.H., "Cox-2 Inhibitors, Other NSAIDs, and Cardiovascular Risk: The Seduction of Common Sense," *Journal of the American Medical Association*, published online 12 September 2006, downloaded 14 September 2006 from [www.jama.com](http://www.jama.com); and Susan F. Wood, Ph.D., "Women's Health and the FDA," *The New England Journal of Medicine* 353: 1650-51, 20 October 2005.

<sup>265</sup> James Terrill, Jr., "Dietary Levels of Fallout Nuclides in Foods," JCAE 1965, pp. 383-456.

<sup>266</sup> The National Academies (1999) conducted a study, released in 1999, to review the National Cancer Institute's (NCI) 1997 report on exposure of Americans to I-131 from atomic testing in Nevada. The study noted that "[n]ot until 1961, near the end of the period of weapons testing did the Federal Radiation Council set as a goal that the annual limit of I-131 doses to the thyroid for a population group not exceed 0.5 rem and that the individual annual limit not exceed 1.5 rem" (p. ix). The NCI study had concluded that the exposure "will produce between 11,300 and 212,000 excess lifetime cases of thyroid cancer with a point or central estimate of 49,000 cases" (p. 6). Those most susceptible to the cancer—papillary carcinoma—were young children at the time of the exposure, especially those who drank milk directly from backyard cows. The review upheld the NCI's analysis, but indicated the actual number of cancers is "probably in the lower part of the range" (p. 6). Data from the 1986 Chernobyl accident provided much of the hard data on cancer from I-131 (Committee on Thyroid Screening Related to I-131 Exposure, Board on Health Care Services, Institute of Medicine; and Committee on Exposure of the American People to I-131 from the Nevada Atomic Bomb Tests, Board on Radiation Effects Research, Commission on Life Sciences, National Research Council, *Exposure of the American People to Iodine-131 from Nevada Nuclear-Bomb Tests: Review of the National Cancer Institute Report and Public Health Implications*, Washington, D.C.: National Academies Press; executive summary available at <http://books.nap.edu/html/iodine/>). The chair of the NRC committee was the geneticist William J.

scientists' groups were called to testify, yet all were treated harshly by some members of the JCAE, who attempted to undermine their testimony. At these hearings, outsiders were again treated as outsiders, not as the independent experts they were taken to be at previous hearings on fallout and radiation in the 1950s and early 1960s, indicating both the political power scientists' groups had attained to be invited to testify, and also the notion that nuclear risks had become enculturated.

For example, Dr. Lee E. Farr, a pediatrician and Chairman of the Committee on Environmental Hazards of the American Academy of Pediatrics, presented his concern that the FRC was not doing enough to protect young children from milk contaminated by fallout. Farr advocated the decontamination of milk by ion exchange, even if the costs were to prove large. Representative Craig Hosmer took Farr to task for this recommendation:

**Rep. Hosmer:** Now I suppose if you adopt that, if you have to keep the infant pure, you are going to have to do the same thing with the squashed-up carrots and peas and all the other things that come out of cans, because there will be some amount [of radioactivity] there, too.

**Dr. Farr:** There is some amount there. The major fraction that he will receive will be in the milk.

**Rep. Hosmer:** Now I am trying to get around to the point of whether you also feel we should build some lead cages in which to keep these kids for their first year or two.

**Dr. Farr:** No, sir; I do not. I think we have a great many environmental hazards that these children are growing up in, and we must strive to keep all of them at a minimum. We have to strike a balance. Radioactive fallout is only one of the environmental hazards and it cannot occupy all of our efforts.

**Rep. Hosmer:** This happens to be the one you are talking about today.

**Dr. Farr:** This is the one I am talking about today.

**Rep. Hosmer:** You do indicate, however, that fallout is with us and it has become a way of life, at the conclusion of your statement. Then you mentioned the effect on the child psyche. I suppose it is in the kind of category of a bogeyman or witches and things like that that you talk to children about.

. . . . .

**Dr. Farr:** A great deal of pediatrics is treatment of parents.

**Rep. Hosmer:** Yes. I am still trying to sort this out from a lot of other things. Here is a kid's parents who pick up a newspaper at night. Here in Washington

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Schull, who had been Chair of the Genetics Department with the Atomic Bomb Casualty Commission (ABCC), and a Director of its successor agency, the Radiation Effects Research Foundation (RERF).

someone gets killed every night. People get their homes invaded, and all this villainy and terror occurs. I suppose to my mind the people talk more about that and that should have a detrimental effect on children by a large order of magnitude over anything that is occasionally expressed about fallout.

**Dr. Farr:** I think this is a reasonable judgment.

**Rep. Hosmer:** We had better clean up our streets before we clean up our milk. (JCAE 1965, pp. 145-6)

Hosmer's argument is a clear example of the strategy of cost/benefit analysis, and it is a reflection of the notion that radiation risks were here to stay. Indeed, nuclear power was on the rise as an energy source and as a scientific and technological tool; industries were being built around it. The problem, then, was not to eliminate radiation, but to make its risks scientifically and culturally acceptable—and this interpretation supports the view that the epistemic and the political are intra-twined.

Perhaps the most caustic criticism of the FRC came from the geneticist Justin Frost of the Technical Division of the Greater St. Louis Citizen's Committee for Nuclear Information (CCNI). According to Frost, the FRC recommendations "represent not a simple scientific decision, but a complex combination of scientific, technological, social, economic, and political considerations" (JCAE 1965, p. 174). Yet, as Frost argued, the FRC's Protective Action Guides still did not apply to fallout contamination, as the Chairman of the FRC himself announced in August of 1962 (p. 175). As Frost concluded, "there remain no standards which are clearly guides to action in the case of radioactive fallout" (p. 176). Frost recommended the following:

Global fallout remains a serious problem and in parts of Alaska, exposure to cesium approaches quite closely to the Radiation Protective Guide. There are nations which continue atmospheric testing, and the possibility remains that some U. S. Agencies may wish to resume some testing at some time in the future. Should this occur, it would be most useful to have applicable protection standards before that time, so that they may be, as they should have been in the past, a factor in planning those tests, and in deciding whether such tests should be conducted at all. (p. 176)

Frost continued his attack on the FRC standards by calling for public discussion on the topic, as opposed to solely expert opinion. His opinion on the FRC's effectiveness was unequivocal:

By promulgating standards which cannot be evaluated and therefore must be accepted

by the public as unalterable fiat, the FRC is doing a great disservice to the Nation. The very pressing need for discussion of the problems of nuclear energy has been denied by the existence of these arbitrary standards, which short circuit the democratic process and prejudge very grave questions. (p. 180)

Representative Hosmer took issue with Frost's recommendations, and in his questioning of Frost, Hosmer attempted to undermine his field of expertise, genetics. Hosmer focused on the issue of natural background radiation, and the fact that some fallout levels were comparable to background radiation, to which humans are inevitably exposed:

**Rep. Hosmer:** I also have a little uneasiness about your saying that [natural background radioactivity] increases the risk to health to some extent, which is awfully loose language. You can't say to what extent, can you?

**Dr. Frost:** No, because this statement includes not only genetic damage but somatic damage.

**Rep. Hosmer:** Yes, to some extent, based on your opinion, but you can't tell us to what extent?

**Dr. Frost:** I would not say just based on my opinion. As I said, I cannot give you 100-percent answers. But there is very strong, exceedingly strong genetic evidence that radiation produces mutations and furthermore there is exceedingly strong evidence that most mutations are harmful.

**Rep. Hosmer:** If that mathematical theory you have developed can be proved or disproved.

**Dr. Frost:** It is not a theory. It has been established by experimentation.

**Rep. Hosmer:** We are talking about a continuous very, very low dosage situation. I won't accept that, Doctor. The next thing I have to quarrel with you about, you talk about establishing this risk versus benefits. You talk about Plowshare and nuclear reactors and all those things and challenge us to calculate benefits. I think a lot of times it has been done. It is spread around perhaps. You want to balance them against the risk but you don't have either side of the equation defined well enough to do any balancing job.

Have we, really?

**Dr. Frost:** It is obviously a very difficult thing to do but that is what has been done. This is what the FRC has done. (p. 188)

Again, this narrative concerning the FRC resonates with Ulrich Beck's (1992) analysis of the risks of modernization. Beck holds that the risks of modernization pose a global threat, and he proposes five theses: (1) that new knowledge presupposes modern risks and is dependent on them; (2) that modern risks affect all socioeconomic classes, yet extreme poverty correlates with extensive risks; (3) that risk policy serves the profit of industry, and

that such policy is predicated on the assumption of lessening risks, not eliminating them; (4) that scientific dispassion in part is the cause of these risks, and that acceptable limits or tolerance doses are the result (or MPDs or PAGs); and (5) that modern risks pose a threat to democracy in that the dangers involved may legitimate emergency actions, actions that may become totalitarian. All of these theses are supported by the narrative on radiation standards developed in this dissertation, although many aspects of the story are not developed here, including the business and socioeconomic aspects.<sup>267</sup> However, the present goal is not to explore Beck's analyses in detail, but to point out that such analyses, which take a deconstruction of epistemic sovereignty as a starting point, and which include research components such as socioeconomic class or other economic and political categories, are no less historical or philosophical by virtue of their using such strategies. On the contrary, the test of whether such an analysis is good scholarship should be how it adheres to History-1 and History-2, whether it reflects a commitment to reflexivity and the principles of postmodern historiography, whether it has a competent grasp of the epistemological and political components of the technological infrastructure of the scientific practices in question, and the level of its underlying reverence for truth.

Taking Beck's (1992) last thesis as an example, one could speculate what might have been the result in the mid-1960s if fallout levels did not wane, and certain populations at risk, such as the Eskimo populations in Alaska, were subject to radiation levels that exceeded the Protective Action Guides of the FRC. Forced removal of the populations might have resulted. With regard to the Alaskan populations, however, even though it was reported that

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<sup>267</sup> For examples of increased risks correlating to extreme poverty, see Andrew C. Revkin, "Poor Nations to Bear Brunt as World Warms," *The New York Times*, 1 April 2007, p. A1; David Brown, "Global Study Examines Toll of Genetic Defects," *The Washington Post*, 31 January 2006, p. A3; U. S. Government Accountability Office, "Poverty in America: Economic Research Shows Adverse Impacts on Health Status and Other Social Conditions as well as the Economic Growth Rate," GAO-07-344, January 2007, esp. pp. 13-14. According to the GAO: "Another reason that individuals living in poverty may have more negative health outcomes is because they are more likely to live and work in areas that expose them to environmental hazards such as pollution or substandard housing. Some researchers have found that because poorer neighborhoods may be located closer to industrial areas or highways than more affluent neighborhoods, there tend to be higher levels of pollution in lower-income neighborhoods. The Institute of Medicine concluded that minority and low-income communities had disproportionately higher exposure to environmental hazards than the general population, and because of their impoverished conditions were less able to effectively change these conditions" ("Poverty in America: Consequences for the Individual and the Economy," GAO-07-343T, 24 January 2007, pp. 12-13).

in the summer of 1964 that intakes of Eskimos of Sr-90 had exceeded FRC guidelines and that Cesium-137 levels were also high, the FRC concluded that the risk to the fetus is “essentially insignificant” (testimony of Tompkins, JCAE 1965, p. 53).

Apparently, the financial costs of admitting the health hazards was the dominant concern in this case in the eyes of Tompkins and the FRC. Indeed, if acute environmental contamination had occurred, it seems the only “protective action” possible would have been forced evacuation of the populations. Such was the fate of the Rongelapese exposed to the *Bravo* fallout of 1 March 1954. Tests found that the island of Rongelap, in the Bikini Atoll in the Marshall Islands, was so severely contaminated by *Bravo* that the U. S. government forcibly removed (temporarily) the native inhabitants. In response to lawsuits against the U. S. government, Congress finally voted compensation for the Rongelapese, and the administration of payments began in 1991. In addition, the House Natural Resources Committee, then chaired by Representative George Miller of California, reexamined the *Bravo* incident. Peter G. Crane, one of the three administrative judges of the Marshall Islands Nuclear Claims Tribunal, which oversaw compensation to the Rongelapese, described the outcome of the Congressional investigation:

The [Congressional] hearing left in tatters the long-held U. S. government position that the Rongelapese were irradiated only because of an unforeseen last-minute change in wind direction. In fact, U. S. government weathermen had reported the day before that the winds were blowing toward Rongelap. A former Atomic Energy Commission official testified that a classified report, issued several months before the test but then withdrawn and suppressed, had called for a precautionary evacuation of Rongelap and other nearby atolls. (Peter G. Crane, “Where March 1 is a Day of Mourning,” *The Washington Post*, 6 March 1994, p. C7)

Clearly, it is not a large stretch to imagine this scenario having been played out on a larger scale, and even in the continental U. S. and Alaska, if testing rates had been different, if atmospheric testing had again resumed on a large scale, or if other nuclear programs, such as Project Plowshare, had been pursued. Today’s problems—increased temperatures linked to human-generated greenhouse gases,<sup>268</sup> ice-melting in Alaskan native populations’

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<sup>268</sup> See, for example, Doug Struck, “Earth’s Climate Warming Abruptly, Scientist Says: Tropical-Zone Glaciers May Be at Risk of Melting,” *The Washington Post*, 27 June 2006, p. A3; Juliet Eilperin, “Study Confirms Past Few Decades Warmest on Record,” *The Washington Post*, 23 June 2006, p. A3; Juliet Eilperin,

homelands,<sup>269</sup> rising waters that threaten islands in the southern Pacific,<sup>270</sup> and other effects related to global warming; technical and political problems related to nuclear power plants and the storage of nuclear waste;<sup>271</sup> and the specter of the detonation of a radiological dispersion device or improvised nuclear device (i.e., “dirty bomb”) by a nation or terrorist group<sup>272</sup>—are further instantiations of the hazards of modernization. Without adequate foresight and policy planning, not to mention the best technical and scientific input into that process, the threat of another *Bravo* or Chernobyl looms as a possibility before decisive action is taken. And finally, such technical and scientific input, according to the technological infrastructure of science, is normative *and* political “all the way down,” and the stories we tell should reflect that interdependence of the epistemic and the political.

### **Futural Norms Revisited**

This narrative on the nuclear age, as indicated in the previous chapter, suggests our traditional views of scientific practice, research ethics, and experimental methods, need revision. In the traditional (Baconian, inductive) view of “the scientific method,” it is the experimental data that are supposed to determine, or influence in some more or less primary fashion, whether the hypothesis being tested will be tentatively accepted or rejected. The strategy of generating data for the specific purpose of supporting a previously held

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“Study Reconciles Data in Measuring Climate Change,” *The Washington Post*, 3 May 2006, p. A3; Steve Lohr, “The Cost of an Overheated Planet,” *The New York Times*, 12 December 2006, p. C1; John Collins Rudolf, “The Warming of Greenland,” *The New York Times*, 16 January 2007, p. F1.

<sup>269</sup> See, for example, Doug Struck, “Inuit See Signs In Arctic Thaw: String of Warm Winters Alarms ‘Sentries for the Rest of the World,’” *The Washington Post*, 22 March 2006, p. A1.

<sup>270</sup> See, for example, Juliet Eilperin, “Antarctic Ice Sheet Is Melting Rapidly: New Study Warns Of Rising Sea Levels,” *The Washington Post*, 3 March 2006, p. A1.

<sup>271</sup> See, for example, U. S. Government Accountability Office, “Nuclear Regulatory Commission: Preliminary Observations on Its Process to Oversee the Safe Operation of Nuclear Power Plants,” GAO-06-888T, 19 June 2006; Matthew L. Wald, “Nuclear Reactors Found to Be Leaking Radioactive Water,” *The New York Times*, 17 March 2006, p. A17; Shankar Vedantam, “Storage of Nuclear Spent Fuel Criticized: Science Academy Study Points to Risk of Attack,” *The Washington Post*, 28 March 2005, p. A1; Shankar Vedantam, “Nuclear Plants Not Keeping Track of Waste: GAO Study Faults Federal Government for Failing to Implement Safeguards,” *The Washington Post*, 12 April 2005, p. A19.

<sup>272</sup> See, for example, U. S. Government Accountability Office, “Nuclear Nonproliferation: DOE’s International Radiological Threat Reduction Program Needs to Focus Future Efforts on Securing the Highest Priority Radiological Sources,” GAO-07-282, 31 January 2007.

hypothesis or theory violates the empiricist foundation of the traditional view of twentieth century science; indeed, today it would conceivably open one to charges of scientific fraud or misconduct. The data are supposed to come *first* through experimental testing (of hypotheses), and be independent of the hypotheses or theories to be tested. Only then does one generate the explanation for the natural phenomenon or the explication of the policy position. Clearly, the explanation or theory is supposed to be *based on the data* in a stringently empirical and testable way.<sup>273</sup>

However, as described in Chapter VI, the epistemological status of evolutionary theory in general, and population genetics in particular, makes any easy interpretation of experimental data problematic. As with the ubiquitous underdetermination of historical explanations from the data (written evidence, artifacts, and a practical evaluation and understanding of the sequence of relevant events, which themselves must be supported by evidence), the epistemological problems of evolutionary genetics presented challenging problems to its practitioners, and it seems that one had to identify which “school” one belonged to—that is, which metanarrative one was to follow—before one could go on with the practice of doing genetics. In the former, one needs an historiography; in the latter, a commitment to some unifying principle in science that could both make sense of the data *and* promise future research practices. If this general description is accurate, then the traditional view of the scientific method is at best a gross distortion of scientific practice. More importantly, since a major component of scientific activity is securing future research activity

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<sup>273</sup> The use of strategies of obfuscation in interpreting and responding to scientific data—as presented in this and the previous chapter in attempts by members of Congress and other government officials to undermine scientists’ data on the health effects of fallout and to fit scientific data into preconceived policy positions—is part of the science policy scenario today. The strategies include creating deliberate confusion and uncertainty and clouding the interpretation of the scientific evidence; deliberately delaying the science policy process through political means; charging scientists with scientific fraud; hiding the identities of those challenging the scientific data; harassment of scientists; and above all, forcing interpretations of scientific data into existing policy positions. See, for example, Jonathan M. Samet, M.D., M.S., and Thomas A. Burke, Ph.D., M.P.H. (2001), “Turning Science Into Junk: The Tobacco Industry and Passive Smoking,” *American Journal of Public Health* 91: 1742-44; Linda Rosenstock, M.D., M.P.H., and Lore Jackson Lee (2002), “Attacks on Science: The Risks to Evidence-Based Policy,” *American Journal of Public Health* 92: 14-18; David Michaels, Ph.D., M.P.H. and Celeste Monforton, M.P.H. (2005), “Manufacturing Uncertainty: Contested Science and the Protection of the Public’s Health and Environment,” *American Journal of Public Health* 95: S39-S48; and Linda Rosenstock, M.D., M.P.H. (2006), “Protecting Special Interests in the Name of ‘Good Science,’” *Journal of the American Medical Association* 295: 2407-10.



in the face of contested epistemic foundations, *that* activity should be investigated. Moreover, it requires a research strategy that can both handle the relevant presuppositions *and* situate itself within those presuppositions; that is, it should be reflexive. This is the purpose of the technological infrastructure of science. It is designed to account for change in science, based on a reflexive, non-sovereign epistemic foundation, one that makes sense of the reality of the practice of science by acknowledging its political *and* epistemic components. In addition, these components are mutually implicated in any narrative that is constructed—and narratives, based on the best available empirical evidence, *must* be constructed—and these narratives themselves are subject to an epistemic uncertainty that the researcher using the technological infrastructure must reflectively and reflexively acknowledge.

### **Revisiting Wallace, Dobzhansky, and Heterosis**

Bruce Wallace's experiments on heterosis in *Drosophila* populations, and Dobzhansky's support of them, pose fascinating, yet complicated problems for the historian trying to interpret their significance. As discussed in Chapter VI, geneticists who were their contemporaries, including Crow and Lewontin, in addition to historians who have analyzed the classical/balance controversy, such as John Beatty and Diane Paul, have pointed out the issue of the underdetermination of the data, and they have indicated that there were political implications involved in the positions taken in the controversy. However, many historical questions regarding how to interpret the significance of the experiments, and why Dobzhansky accepted heterosis and supported Wallace's work, have not been fully answered. In this section, I present some tentative answers to these questions.

That is, I present an interpretation of a small slice of the story of genetics in the Cold War: Bruce Wallace's experiments on irradiated populations of *Drosophila*, Dobzhansky's support of them, and heterosis as an interpretation of the experimental data. Again, the ultimate purpose of this interpretation is to illustrate the principles of the technological infrastructure of science, using the narrative on radiation genetics and policy constructed in

this and the previous chapter, as well as more recent writings by Wallace, Lewontin, and others. This story will focus on the interpretation of Wallace's data in the contexts of Dobzhansky's work and of the professional and political standards of the time, reflexively realizing that this interpretation cannot avoid recurrence and the inevitable distortions and epistemological uncertainties it engenders and presupposes. The main principle of the technological infrastructure I emphasize in this section is the futural norm of *the promise for future research*. This principle permeates the present narrative on radiation genetics and policy. I argue that one component of the story of heterosis that should be considered is one that flows from Lewontin's (1995b) analysis of population genetics in the 1950s and 60s, and in particular Dobzhansky's work: this is the view that the experiments were designed to generate evidence in support of a theoretical position already held, a position one could argue is untestable or unfalsifiable in a practical sense (at least at that time), but not in practice (i.e., experiments *were*, in fact, performed). Regarding Wallace's heterosis experiments, I propose that heterosis was such a preconceived position, conceived and held for a variety of reasons, one of which was to continue his AEC-funded research program by attempting to "obtain evidence for selection to radiation resistance in *Drosophila*."<sup>274</sup>

### I. Wallace's Experiments

Turning now to Wallace's radiation genetics experiments, this was a research program in which the most controversial and extreme version of the balance model of population genetics—heterosis—was played out. In a series of experiments begun in 1949, Bruce Wallace investigated the effects of radiation on populations of *Drosophila melanogaster*, focusing on heterosis as an explanation of his experimental results.<sup>275</sup> From

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<sup>274</sup> This is how Wallace explained his research program to Curt Stern, who had written to Wallace for information on his experiments (Wallace to Stern, 21 April 1958, Curt Stern Papers, APS).

<sup>275</sup> As Wallace stated in his 1963 research report to the AEC: "The results of these experiments have been consistent with the following suggestion: If  $\underline{a}_i$  and  $\underline{a}_j$  are two of many alleles at the  $\underline{a}$  locus retained in a large population under the influence of natural selection, and if  $a^*$  is a newly induced alleles, the order of average viabilities can be given as  $a_i/a_j$   $a_i/a^*$   $a_i/a_i$ " ("The Genetic Structure of Populations," Bruce Wallace Papers, APS). This research report was mandated by an outside review of the Genetics Program of the Division of Biology and Medicine of the AEC, initiated in 1963. The review called for a "two or three page summary outlining (1) the reasons for the problem under investigation; (2) the specific objectives; (3) the experimental

1949 to 1957, Wallace published some 20 papers on this research project, which was funded by the Atomic Energy Commission. He described his project—in a 1963 research report to the AEC—as having found that:

Populations, in which frequencies of obviously deleterious genes were substantially different, sometimes altered in fitness in a direction opposite to that expected. Under the influence of chronic irradiation, the fitness of an experimental population could increase while the frequency of lethals and other deleterious genes increased as well. (“The Genetic Structure of Populations,” Bruce Wallace Papers, APS)

An earlier research proposal to the AEC—from 1958—summarized the significance of his findings as follows:

From the information we have analyzed so far it would seem as if the selective superiority of heterozygotes is extremely common both in terms of loci at which it exists and in the proportion of alleles at a given locus which exhibit it. (“The Investigation of the Genetic Structure of Populations,” Curt Stern Papers, APS)

Clearly, Wallace believed that “overdominance was a factor to be reckoned with” and that it “could not be neglected in population studies” (“The Genetic Structure,” *op. cit.*, above).

In 1958, when his AEC contract was up for renewal, and when he was preparing to move from Cold Spring Harbor to Cornell University, Wallace seemed to suggest that he felt isolated at Cold Spring Harbor:

It is essential that the complications of population genetics be understood. Commonly held concepts of population processes are . . . based almost entirely on the absence of such complications. Consequently, it is commonly believed that our understanding of populations can be adequately based on the knowledge of certain parameters such as mutation rates, numbers of gene loci, semidominance, “persistence”, and the like. Heeding the demand for “practical” information, experimentalists will undoubtedly shift their efforts more and more to the painstaking drudgery of determining numerical values for these parameters. Furthermore, in an effort to obtain information seemingly applicable to man, material will be chosen for study in which it will be virtually impossible to perform any experiment designed to test basic assumptions. As the financial and emotional investment in the determination of this “practical” information grows, it will become increasingly embarrassing for anyone to question the validity of those concepts which demanded such studies. Not only increasingly embarrassing to raise questions but also increasingly difficult to obtain an audience. (“The Investigation of the Genetic

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techniques used in attaining these objectives; and (4) a brief statement of results that have been obtained” (Edington to Wallace, 12 April 1963, Bruce Wallace Papers, APS).

Structure,” *op. cit.*, above)

Indeed, when discussing his move to Cornell University and his proposed future experimental work, Wallace offered the following:

The writer has recently accepted a university position. Although there were a number of reasons for this choice, among these was the feeling that the nature of his future research would be such that i[t] could best be done in a university atmosphere. A laboratory devoted to pure research was excellent for maintaining populations over many generations and for offering facilities for performing an enormous amount of work spent on routine analyses. Future experiments promise to be of a different sort. While some of the experimental procedures are quite obvious, others are not. The search for the right experimental approach, or for the phrasing of a hypothesis which makes an experimental test possible, or even for the proper experimental organism in certain cases can best be carried out in the company of students (using the term in its broadest sense) rather than in the company of assistants. (*ibid.*)

This self-evaluation, written as part of his 1958 research proposal to the AEC, reflects Wallace’s status as “the unofficial gadfly of population genetics.”<sup>276</sup> This assessment is in no way meant to suggest that Wallace’s work was not scientific, important, or otherwise meaningful; it reflects a view that Wallace’s work was often seen as pushing the boundaries of the discipline of population genetics. What is more significant for present purposes is what his work can reveal about the practice of science, and how it illustrates the technological infrastructure of science’s commitment to epistemic politics. In the case of Wallace, this is somewhat difficult to glean, because unlike Lewontin, who has written much on his commitment to Marxism and other political ideas, Wallace has been rather tight-lipped in his writings on personal comments, and he hesitates to take a stand on personal, political, and policy issues.<sup>277</sup> Some pertinent questions remain: to what extent was Wallace’s work

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<sup>276</sup> James Crow made this comment in a review of one of Wallace’s papers (quoted in Wallace 1991, p. 160). Lewontin’s comments on Wallace and Dobzhansky’s work, and the reception of Wallace’s work in the 1950s and 1960s, as presented in the narrative above, lend support to this view.

<sup>277</sup> In 1991, as a naïve beginning Masters student, I spoke to Bruce Wallace in his office at the Biology Department at Virginia Tech, and asked him about the political issues surrounding his work on radiation genetics in the 1950s. He did not have much to say, but instead gave me the preprints to his book, *Fifty Years of Genetic Load*, which was about to be published. We spent more time talking about a different gadfly, Steve Fuller, who had been denied tenure by a review committee of the Center for the Study of Science in Society (he was eventually granted tenure by a higher committee, and then left Virginia Tech). I asked Wallace to write the Dean in support of Fuller, who I believed at that time was being ostracized for reasons other than his scholarship and teaching (ironically, reasons mainly having to do with personal and philosophical

in this period, insofar as it lent scientific (and political) support to the AEC's policy position on the harmlessness of fallout from atomic testing, influenced by its political implications? Did Wallace's interpretations of his experiments—which were clearly influenced by Dobzhansky and Lerner—reflect a desire or need to ensure the continuation of his research program, which was criticized by other geneticists, including Demerec, his superior at Cold Spring Harbor, for needing future confirmation and for involving essentially tentative results that must await further developments?

It is clear that part of the motivation for Wallace's research program was the practical issue of the need for data on the genetic effects on populations from low levels of radiation. It is difficult to believe that without the atomic bombings of Hiroshima and Nagasaki, the advent of atomic testing, and the emerging nuclear industries, Wallace would have been working on the problem of radiation-induced mutations. However, it is not inconsistent with the historical data (to use the language scientists often use when making conclusions they know are anything but conclusive, but nevertheless supported by the data) to suggest that Wallace's research program was, at least in part, motivated by the practical need for continued funding. The need to secure future funding for genetics research, in a period in which the future of population genetics was uncertain and the epistemic foundations were themselves at stake, was a theme that permeated the above narrative on radiation policy.<sup>278</sup>

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disagreements on science and politics). In addition, we talked about the *World Watch* seminars, which Wallace organized at Virginia Tech, and which called attention to environmental and other problems that required action on a global scale. I attended many of these lectures and respected the work Wallace did on these issues and the political messages they implied.

<sup>278</sup> According to Lewontin (1997b), in the 1950s the AEC, the Office of Naval Research (ONR), and other governmental agencies were “funding research in universities and university research institutes by a system of contracts. The term ‘contract,’ conveying the notion of the procurement of a determined product specified by the purchaser, hides the reality. The ‘contracts’ with academic institutions were, in fact, grants to individual investigators or small groups to carry out research projects generated by intellectual forces internal to the disciplines, provided only that some general relevance to the mission of the federal agency could be established” (p. 16). In this dissertation, I do not suggest that Wallace's commitment to heterosis was solicited by the AEC or the U. S. government. I do suggest, however, that part of his motivation for attempting to raise radiation-resistant flies comes from his need to secure funding in a period when finding academic positions was difficult. As Lewontin (1997b) states, that there were many researchers working in government laboratories or funded by the AEC “was partly the historical remnant of the small number of academic positions available in the early 1950s. . .” (p. 16). For details on the administrative structure of the early biomedical activities of the AEC, its relations to other agencies of the U. S. government, and on its system of contracts, see AEC (1949, 1950, 1951). AEC (1950) states that the AEC “established a Division of Biology and Medicine [DBM] to conduct a Nation-wide program of research and to administer radiation protection, and appointed a permanent

In addition, developing an experimental research program designed, at least in one variant of the program, to produce flies with genetic resistance to radiation would, at the very least, attract the attention of the AEC, which expended much effort in the 1950s to deflect arguments suggesting the genetic dangers of fallout and low levels of radiation were significant. There is at least indirect historical evidence to support this interpretation.

In early 1955, in the midst of the international controversy over fallout, Wallace wrote to Ernst Caspari, asking him for help with an experiment he hoped to conduct:

I have had a hair-brained idea. Knowing that you are tolerant of such ideas, I have no qualms about approaching you on the matter. If you have no set plans for research one of these summers at C[old] S[pring] H[arbor] and think enough of it perhaps you would do the serological part of the following.

The basic question involves the heterosis observed between crosses of flies from different populations. Vetukhiv, Brncic, Frota-Pessaro, and I have enough data now to show that heterosis of inter-population hybrids is a general rule in *Drosophila*. This heterosis cannot be explained by selection.

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The underlying argument would go something like this: The biological and physiological processes leading to the formation of individuals of a given species are more constant than the genetic bases responsible for these processes. . . . Hybrid substances may indicate the existence of an interference between alternate biochemical processes. At the level that such interference or incompatibilities enter, we might expect to find that divergence is sufficient to interfere with heterosis.

If this scheme interests you, I would like (1) to obtain strains of hybridizing species (a rather large number of such pairs), (2) to measure the competitive ability of larvae of intra-species (inter-strain) heterozygotes and inter-specific hybrids, and (3) to help (observe, since I have had no experience in this field) you run tests for hybrid substances some summer. Whether there was a correlation or not, the two sets of experiments would be interesting in them selves: (1) a survey for heterosis and (2) a survey for hybrid substances. (Wallace to Caspari, 8 January 1955, Ernst W. Caspari Papers, APS)

Wallace's argument presupposes that heterosis in inter-population hybrids of *Drosophila* "is a general rule" and that inter-species hybrids sometimes exhibit heterosis. He suggested that the explanation for the eventual loss of heterosis he observed in his populations was the formation of "hybrid substances" that arise in the development of the hybrids, and that these

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Advisory Committee on Biology and Medicine [ACBM]" (p. 127). It also stated: "Responsibility for the AEC health physics program is assigned to the [DBM] in Washington, but practically all health physics work is carried out by contractor employees in the [AEC's] plants and laboratories" (*ibid.*).

cause heterosis to be blocked. Hence, he proposed that he and Caspari search for a correlation between the eventual loss of heterosis in the populations, and hybrid substances being produced in those flies in the generations that exhibit the loss of heterosis, and that even if the correlation was not found or was not statistically significant, the data would nevertheless be interesting and publishable.

Caspari, however, indicated in his reply that while Wallace's "ideas are very interesting," nevertheless "further thinking is necessary" before work on the project could be initiated. He explained his misgivings about the experiments as follows:

(1) The existence of hybrid substances has been demonstrated beyond doubt in pigeons. I have no doubt that they exist also in *Drosophila*. The question arises whether these hybrid substances can be related to heterosis. I do not see any way how this can be proved at present. (2) My real hesitation to attack the problem of heterosis in this way comes from the fact that I have made myself a different picture of the nature of heterosis, in part on the basis of my recent mouse investigations. You may have seen a paper by Grüneberg (*Nature* 173: 674, 1954) about variation in quantitative skeletal characters in the mouse. His findings, drawn from a number of his investigations, is that in general the variability [of the skeletal characters] of F1 is reduced as compared with the original strains. He explains that by assuming that in the heterozygote the developmental margin of safety, whatever that may be, is increased. (3) My experience with the mitochondria in the mouse points in a similar direction though the data are not quite as conclusive. . . . These data, as far as they go, seem to indicate that the increased margin of safety, or homeostasis—or whatever you want to call it—proceeds at the intracellular level and not only at the developmental level. (Caspari to Wallace, 15 January 1955, Ernst W. Caspari Papers, APS)

Caspari concluded by suggesting that the "fundamental nature of heterosis would consist not in the formation of hybrid substances but in a much better buffering of environmental influences" (*ibid.*).<sup>279</sup>

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<sup>279</sup> Wallace replied to Caspari: "Heterosis and, it seems to me, homeostasis, can be explained in inter-population or inter-strain hybrids in the following way: Each haploid set of the hybrid produces those portions of first-order substances [the primary gene products] that give the species-proportions of second-order substances. The presence of the two systems is more reliable than the presence of a single system and so we have heterosis and homeostasis in one step." He continued: "But we are still left with the problem of what happens at the point where heterosis disappears. . . . But what has really happened? On my working hypothesis, I would say that there is an interaction at the level of the second-order substances and this interaction may be reflected in the production of hybrid substances. This is hardly more than a guess based on the fact that, by definition, there are no hybrid substances produced in inter-population, intra-specific hybrids. I think that this point was misunderstood by you and that you thought I was trying to explain heterosis by the

What is significant in this exchange is that Wallace was not proposing a test for heterosis, although he claimed he and other researchers had enough data to support heterosis, as measured by larval competition, in *Drosophila*. Wallace presupposed a particular interpretation of heterosis, which was itself predicated on a number of assumptions regarding basic data on population genetics, for example the proportion of loci that are heterozygous or even the number of segregating genetic loci. These data, as indicated in Chapters VI and VII above, were in dispute in the 1950s and into the 1960s, and underlie the basic disagreements in the classical/balance controversy (which got its name from Dobzhansky in 1955). The experimental data Wallace proposed he and Caspari generate were not for the testing of whether heterosis, or some particular interpretation of it, was occurring. He proposed to generate data that demonstrated heterosis in action, given many assumptions about its operation, and in a period in which heterosis itself as even a general interpretation was contested and not generally accepted.

This is not to suggest that Wallace's actions constituted "bad science" or poor judgment. On the contrary, it seems with recurrence that it is sensible to conclude that interpretations of the sort Wallace made were common and even necessary if one was to get on with the business of doing the science of population genetics in the 1950s. It is clear, however, that many geneticists were aware of many of the epistemological problems that plagued population genetics (see Chapter VI above), yet they continued to search for answers and even to offer policy advice based on their tentative answers, even in the face of criticism by those in the medical and physical sciences, who still saw genetics as an emerging field that was, in a real sense, separate from medicine. Indeed, one could argue that the relevance of population genetics for medicine was generated in the post-war years, even before its own experimental foundations were sufficiently established. In any event, viewing heterosis as a scientific metanarrative that provided unifying theoretical power to experimental results that were acknowledged as inconclusive, suggests that the answers to how and why Wallace and Dobzhansky became committed to heterosis will be more complex than suggesting, for

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production of hybrid substances. I am not. I think that the level of divergence that results in the production of hybrid substances is the level at which one finds a lack of heterosis in species hybrids" (Wallace to Caspari, 17 January 1955, Ernst W. Caspari Papers, APS).



example, that the experimental results and the consensus of the scientific community were the deciding factors.

## II. *Interpreting Wallace's Experiments*

As indicated in Chapters VI and VII above and in the narrative on radiation policy above, Wallace's experiments generated public and professional controversy and were seen by some geneticists as inconclusive and needing further research. Nevertheless, Dobzhansky supported Wallace's work and held it to be the only relevant work on the issue of the genetic effects of radiation on populations. In this section, I contrast differing interpretations of Wallace's experiments: those of Dobzhansky and Crow, at the time, and of Lewontin, from his assessments in retrospect. Dobzhansky considered Wallace (his first graduate student) and Lewontin to be his best students,<sup>280</sup> yet it is striking to consider how these two population geneticists came to hold such differing views of their field and of their mentor.<sup>281</sup>

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<sup>280</sup> Dobzhansky stated in 1962 in his oral history memoir (New York: Columbia University) that Wallace and Lewontin "justified" his "earthly existence" (quoted in Costas B. Krimbas, "Introductory Remarks," in Singh and Krimbas 2000, pp. 1-4, quotation from p. 1); see also Levene (1995), pp. 16-18. A year or so later, Dobzhansky wrote to Lewontin regarding the Kimura-Li controversy; he accused Lewontin of blaming him for the controversy: "Unless my memory is faulty, there has not been a case when anything that I said, wrote, or done met with your approval. I suppose this phenomenon may have some Freudian explanation, and for that reason or otherwise, I am so used to your disapproval that it no longer hurts me as much as it used to. Hence, your blaming me for the Kimura-Li controversy, although unexpected, is not really surprising." He ended the letter this way: "Anyway, since when do you consider the Wisconsin-Indiana line [the Crow-Muller classical position] so sacred that it should not even be questioned? I thought that you like defiance of authority for defiance sake, and that a rebellion appeals to you as something heroic regardless of anything, so small as whether the rebellion is justified. Oh, please, credit Li at least with defiance and rebellion, even despite his rather accidental connection with me!" (Dobzhansky to Lewontin, 7 May 1963, Richard Lewontin Papers, APS).

<sup>281</sup> In the 1995 volume on Dobzhansky, edited by Louis Levine (1995), Wallace (1995) credited Dobzhansky with three main legacies: "(1) Populations are genetically heterogeneous collections of dissimilar individuals, and species are heterogeneous collections of genetically dissimilar populations. . . . (2) Speciation is a stage in the evolutionary process. Populations that have passed that stage can no longer exchange genes; their evolutionary paths are now essentially independent. . . . (3) The exchange of genes between species is prevented by isolating mechanisms whose initial genetic basis is provided by two or more complementary genes but whose elaboration is the result of natural selection acting in quantifiable, comprehensible ways" (p. 43). In the same volume, Lewontin (1995b) presents his unflattering view of Dobzhansky as a "theoretician without tools" who, "[o]ver and over again . . . performed experiments to exemplify general principles without attempting to cash them out in detail for specific cases" (p. 91). In describing experiments Dobzhansky performed on fitness and environments, Lewontin (1995b) concludes that they were designed to "illustrate the general principle that each genotype had its unique norm of reaction, that one could not predict how well a genotype would be in the environment from its performance in another environment, and that heterozygotes were generally less susceptible to environmental fluctuations than homozygotes. There was never any question

As indicated above, Dobzhansky supported Wallace's work on heterosis and asked Demerec to push for his inclusion on the BEAR II Genetics Committee. Wallace's work had been discussed in the BEAR I meetings, and there was general agreement that his experimental results were tentative and did not have relevance for human populations, since a large proportion of his populations died in each generation, and since the larval competition in the crowded population cages, used in determining the fitness levels, was not representative of human populations. Demerec did not think it a good idea to press the matter of getting Wallace on the committee, yet at one point, as Wallace (1991) recalls, Demerec asked his Assistant Director at Cold Spring Harbor to perform a computation related to the Genetics Committee's deliberations:

At one time Dr. Demerec asked me . . . to compute the effect on human populations of radiation exposure using certain background information and, from [the] results, to recommend a maximum permissible exposure. I begged off, arguing that all persons using the same background information would arrive at the same conclusion (although not necessarily at the same recommendation). I offered to try a different approach: In a population of 200 million persons, I reasoned, there are 400 million copies of each gene. If the mutation rate for recessive lethal alleles at each locus were  $10^{-6}$ , the equilibrium frequency of these lethals would be  $10^{-3}$ . That is, 400 new lethals should arise each generation, and the total number of lethal alleles at a given locus should be 400,000.

. . . . .

I reported to Dr. Demerec after several days that a maximum permissible dose as low as 3 R could be defended. He reflected for a moment. "No," he finally said, "we have already decided on 10 Roentgens; that's the exposure that must be justified." (pp. 20-21)

This recollection reinforces the notion, explored above, that the data on radiation genetics in the 1950s and 1960s were characterized by practical epistemological uncertainty, and that there was, as a result, a significant range of positions to hold that were consistent with the experimental data. It is clear, however, that heated disagreements arose over how to

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whether these propositions were true in general. The empirical problem was to illustrate them in practice. The 'experiments' were demonstrations" (pp. 91-2). Elsewhere, Lewontin (1997a) evaluates the legacy of Dobzhansky's *Genetics and the Origin of Species*: "Dobzhansky's construction of the problem of speciation as solely the problem of reproductive isolation was a piece of scientific synecdoche, substituting the process of reproductive isolation for the speciation process in its entirety. It is a testimony to the influence that *Genetics and the Origin of Species* that we continue to study the speciation process without reference to the world that organisms construct and occupy" (p. 555).

interpret the data, and to suggest that these disagreements can be explained by recourse to the “correct” interpretation of the data, especially with hindsight and recurrence, is tantamount to adhering to an epistemically sovereign philosophical position that negates the available historical evidence. Moreover, these disagreements sometimes had concrete and serious consequences (see below). Indeed, the classical/balance controversy was quite alive in the early 1960s, even after the BEAR II report appeared in 1960. As indicated above, although test-ban negotiations had been ongoing and the testing moratorium was still being observed by the U. S. and the USSR, the furor over fallout levels had motivated the Joint Committee on Atomic Energy to call hearings in 1959 on fallout, in 1960 on radiation protection standards, and again in 1965, after the test-ban treaty was signed, on the Federal Radiation Council’s Protective Action Guides (JCAE 1959, 1960a, b, 1965). Hence, the political climate regarding the biological dangers of radiation was hotter than ever, and scientists’ groups were more and more entering the political debates over the dangers of fallout and nuclear war. Moreover, by the late 1950s, McCarthy had been discredited, more university positions were opening up for academics (Lewontin 1997b, esp. pp. 16-17), and Kennedy was elected in 1960. The climate was right for more open discussions and debates among academics, and this is reflected in the emergence of many scientists’ groups in this period, and in the participation of representatives of these organizations in Congressional hearings and in other attempt to influence policy.<sup>282</sup>

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<sup>282</sup> In addition to the efforts in the late 1950s and early 1960s of scientists’ groups mentioned above (p. 425, fn. 11) in the context of the hazards of fallout and nuclear war, scientists were also involved in the ongoing test ban debate, both professionally and politically. Gilpin (1962) details the influence of scientists in the test ban debate (chs. V-X), and shows that certain scientists, such as Linus Pauling and Philip Morrison, helped to make “respectable and tenable” their moral position that “given the high probability of . . . health dangers, it was insane and criminal to continue bomb tests” (p. 155). According to Gilpin (1962) this position, in the period after 1955, “is characteristic of the great number of scientists who came to accept the view . . . [that it is] dangerous to test or to build bigger bombs because of their pathological and genetic effects” (pp. 155-6). As mentioned above, the Federation of American Scientists (FAS) supported a test ban during the Presidential election of 1956; Edward Teller, Freeman Dyson, and many other physicists, especially those who had been involved with the Manhattan Project, supported the continued testing and development of nuclear weapons, including the neutron bomb, while some physicists, such as Hans Bethe, and many biologists and geneticists, supported a test ban (Gilpin 1962, ch. VI). It is a mark of the significance of the period of the early 1960s to note that Dyson, who in the Eisenhower era had staunchly and publicly advocated continued testing, the neutron bomb, and other nuclear programs, such as Project Orion, was elected in 1960 to the council of the FAS. In 1961, the FAS helped lobby for the creation of the Arms Control and Disarmament Agency (ACDA), a new agency for which Kennedy needed Congressional approval. Indeed, in the summers of 1962 and 1963, Dyson

Again, coincident with these developments was the practical epistemological uncertainty underlying population genetics, and a real sense that the experimental techniques available were not up to the job. According to Lewontin: “What people were doing was a kind of unfocused collection of bits and pieces of the Dobzhansky program. What hung over all of our heads was the classic problem of whether there is a lot of heterozygosity or not at the genic level and whether the selection is purifying or not.” As for his view of population genetics, Lewontin stated:

The late 1950s and early 1960s were really the time when the field was beginning to collapse of its own internal contradictions. It seemed pretty obvious that we needed to find some way out that would enable us to actually observe what the frequencies of different variants at different loci were so we could settle once and for all how much heterozygosity on a per-locus basis was out there and what the frequency of the alleles was and, if possible, even try to then go on and determine the fitness relations. (Lewontin *et al.* 2001, pp. 34-5)

Lewontin notes that Wallace’s work was initially considered promising and original, as it seemed to give hope for breaking out of the epistemological standstill:

The one promising direction was Bruce Wallace’s very original work on the effect of newly induced heterozygous mutations on an otherwise homozygous background. This experiment was the first really new idea in the field. And when Bruce, in his huge experiment, showed that there was some average selective advantage to the newly arisen heterozygous mutations, Dobzhansky went wild. This was the thing he had been looking for, and Bruce became his absolute hero. But unfortunately, as time went on, the evidence got weaker, and it didn’t work out as Dobzhansky had hoped. The feeling of a thickening impasse pervaded the whole field. (*ibid.*, p. 35)

While Dobzhansky may have gone “wild” over Wallace’s work, many other geneticists did

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worked for the ACDA; in addition, he served from 1962-1963 as President of the FAS (Dyson 1979, ch. 12).

Moreover, it was in this period of the early 1960s that circulation of *Bulletin of the Atomic Scientists* (BAS) surged. While in its first two years (1945-46) BAS’s circulation went from about 500 to around 5,000, by the mid 1960s it passed the 20,000 mark, a number only superseded (reaching about 25,000) in the aftermath of the 1986 Chernobyl accident (Mary Ruth Yoe, “Nuclear Force,” *University of Chicago Magazine*, 98:1, October 2005, <http://magazine.uchicago.edu/0510/features/nuclear-print.shtml>, accessed 3-13-07). Finally, Paul Boyer (1985b) shows that the period of the mid-1950s to the early 1960s marks the emergence of the political voices of doctors and physicians in debates over testing and nuclear war (“Physicians Confront the Apocalypse: The American Medical Profession and the Threat of Nuclear War,” *Journal of the American Medical Association* 254: 633-43); while Watkins (2001) argues that the fallout controversy of this period set the stage for a sustained environmentalist movement that was launched with the publication in 1962 of Rachel Carson’s *Silent Spring* (Boston: Houghton Mifflin).

not feel that way. Indeed, when Dobzhansky wrote to Crow in early 1962, urging him to support Wallace's nomination to the National Academy of Sciences, Crow declined, arguing that he did not "think the chances of his being elected are very high." He asked: "Would he have the support of Muller, Sturtevant, and Wright? I am pretty sure he wouldn't. My preference is to wait pending confirmation of his more controversial theories." Regarding a paper by Wallace that Crow reviewed for the *American Naturalist*, Crow explained:

Incidentally, I have studied his recent manuscript since seeing you. It is good work, but after careful study I am far from convinced that all of his conclusions follow from the data. The conclusion that the deleterious effect of lethal heterozygotes has decreased over several years to a value not significantly different from zero after the cessation of radiation is very interesting. Whether it is slightly above or slightly below zero is not established by the data. On the other hand his notion that a lethal chromosome shows a greater hetero[t]ic effect when put opposite a chromosome from the same population than when opposite a "foreign" chromosome, though not unreasonable, is not supported by the data, which show just as much apparent heterosis when the lethal chromosomes are opposite CyL or Pm as when opposite one of their own normal chromosomes. I sent a detailed series of comments to the *American Naturalist* with the suggestion that they pass them on to Bruce. (Crow to Dobzhansky, 9 February 1962, Theodosius Dobzhansky Papers, APS)

Dobzhansky replied to Crow in a colorful, three-page letter, essentially accusing Crow and others of being unfair to Wallace:

It is precisely "falling into schools and camps" and party lines and axes that I deplore, the more so since this situation, not uncommon in other countries, was happily uncharacteristic of American genetics, and I very strongly feel that we should keep it so. I am also in favor of "healthy disagreement and full discussion of issues." But this must indeed be "healthy" and not malevolent, aiming to get at some approximation to truth and not to score a point. We should not accuse each other of being mystics or fools, should not denigrate each other's work, and especially not that of our students and collaborators. Perhaps the most difficult aim to live up to, we ought not to apply double standards to each other's work—acceptance of some unpublished and flimsy data as excellent if they please our views, and criticize in a far-fetched and captious manner data which do not fit. And further, we should not overlook data which fail to please us, not persistently refuse to mention relevant work, not to ascribe ideas and even suggested terms to somebody else just to avoid mentioning the names of those who avoid following the "party line."

I hope that none of the above was done deliberately and knowingly, but should we not keep a reign on our Freudian subconscious? Refusal to support Bruce "pending confirmation of his more controversial theories" is, I fear, on the borderline

of such unfair partiality. I hope that you do not suspect him of having faked his data. His “contr[o]versial” theories may or may not be confirmed in the future—the future will show. But is there any doubt that he has demonstrated the highest degree of ingenuity as an experimentalist, and also a true dedication to creative work in science? Has he not published more work than the average? Or should we elect exclusively those mild non-controversial figures who do not displease Muller and Sturtevant? I for one, have voted repeatedly for candidates who displeased me personally, and whose theories I regarded controversial. Is being controversial incompatible with being a first-rate creative scientist? (Dobzhansky to Crow, 14 February 1962, Theodosius Dobzhansky Papers, APS)<sup>283</sup>

After suggesting that, while Wallace did not believe his work to be the last word on lethals in populations, but that his paper for the *American Naturalist* was nevertheless an important contribution, Dobzhansky blasted those who follow the classical “party line”:

And finally, *ad hominem*. I am not known as a flatterer or apple-polisher, but I say that I admire yours, and Muller’s, and Morton’s intellectual gifts and talents as scientists. You are outstanding geneticists, and you and Morton and Wallace and Lewontin will have to carry the banner of population genetics when Muller and myself are no longer here. How unfortunate to find the action of people whom one thus admires to be short of admirable. To understand is (sometimes) to forgive, and I can understand Muller. Once upon a time Muller was a fire-eating communist, and the mental makeup which permits a person to be this (a deleterious mutant? or a heterotic one?) is the same which makes one intolerant of divergent opinion in science as well. Down with those who do not follow the party line! They must be not merely fools, they are also wicked! Fire them from their jobs, send them to concentration camps! I find this attitude foreign to my genotype or to my phenotype. This is why I am in the Western rather than in the Eastern Hemisphere. Should not scientists keep away from such attitudes? (*ibid.*)

Clearly, the opposing positions in the classical/balance controversy were strongly held. But more than this, they were not *only* positions held because of the data, which was inconclusive nevertheless; they had personal, political, ethical, and professional implications, implications embedded in power relations that were inextricable from epistemic matters.

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<sup>283</sup> Ironically, one month prior to writing the letter to Crow, Dobzhansky wrote to Wallace regarding a young researcher he had met: “In Denver I met a nice young lady, Nanette Band, University of British Columbia, which has 2 excellent studies of genetic load in *melanogaster*, showing quite convincingly that the S.O.B. from Wisconsin is wrong and you are right. I told her about your (or ours) analysis of the replicate data, and suggested that she does it on her data, and also that she writes you if she needs further indications of what and how to do. She is a really nice kid, so if she writes treat her nicely” (Dobzhansky to Wallace, 4 January 1962, Bruce Wallace Papers, APS).

Indeed, when Crow wrote back to Dobzhansky a month later, he not only encapsulated the underlying epistemological problems with Wallace's experiments, and much of the field of population genetics in general, he also attempted to appease Dobzhansky on the issue of not supporting Wallace's nomination to the National Academy of Sciences:

As regards the question of the relevance of chromosomal polymorphism studies to population genetics in general, it depends I think on what the question is. No doubt chromosome polymorphism is an important fact in *Drosophila* evolution, and of a great many other species where it occurs. But it doesn't offer any evidence, it seems to me, on one question of interest—and that is whether single-gene heterosis is or is not an important factor in maintaining population variance. Certainly much of the normal variation in populations, such as man or maize, where chromosomal polymorphism of a major sort does not exist has to be explained; what maintains it? Is it largely overdominance or not? Despite a great many works written on this by evolutionists and by animal breeders, nobody knows.

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I haven't replied to your earlier letter because of indecision as to what to say. Please don't think that I do not admire Bruce Wallace for his experimental work, despite some differences of interpretation. I regard him as a close friend, and on a personal basis there is no one I would rather have in the Academy. However, it seems to me as if the people I nominated (Ray Owen, Ed Lewis, Bill Russell, James Watson, and Jim Neel) are more deserving of nomination. This is simply a personal judgment, and others might hold different opinions of course. Not to include Bruce in this select company should surely not be interpreted as any lack of admiration for him or his work. (Crow to Dobzhansky, 16 March 1962, Theodosius Dobzhansky Papers, APS)<sup>284</sup>

Lewontin (2006) believes that Wallace “was kept out” of the NAS by Muller, Crow, and others (he was later elected), and that there were “political motivations” for this—that is, it was not that Wallace was a bad experimentalist or scientist. On the contrary, there was the general feeling among geneticists that Wallace's experiments were original and significant.

However, Lewontin (2006) indicates that there was also the feeling that while

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<sup>284</sup> The exchange of letters between Crow and Dobzhansky continued, with Dobzhansky trying to convince Crow of his position; he sent Crow a reference to a paper that attempted to calculate the number of segregating loci. Crow responded: “I'm afraid this doesn't help much, for we still don't know whether most loci are segregating (for reasons other than recurrent mutation) or not—for a large number, say a few thousand, may still be a minority of all loci. I suspect that this is a question that will have to wait for development of the chemistry of nucleotides for a precise answer. We are not much nearer an answer than geneticists were in 1930” (Crow to Dobzhansky, 21 June 1962, Theodosius Dobzhansky Papers, APS).

Dobzhansky was important in terms of his legacy for evolution and Darwinian natural selection, he was nevertheless “not a careful thinker.” Moreover, Lewontin (2006) believes Wallace “was a front man for Dobzhansky” and that this perception had a role in the classical/balance controversy and Wallace’s exclusion from the NAS in 1962. Additionally, regarding the interpretation of Wallace’s heterosis experiments, Lewontin (2006) recalls that “Dobzhansky, Lerner, and Wallace pushed it to please the AEC.” As he stated:

I know that in the big Muller-Dobzhansky fight a lot was made about where the money came from and where Dobzhansky had bent, Bruce Wallace had bent—not the truth but their interpretation of it—to suit their patrons. (Lewontin *et al.* 2001, p. 50)

We have here a picture, then, of the practice of the science of population genetics in which epistemic matters—the interpretation of experimental results, the presupposition of theoretical prescriptions—and political matters—whether to elect someone to the NAS, whether someone was bending their interpretations to suit funding agencies, whether someone was following the theoretical line of their mentor in a way that was somehow inappropriate—both play a role. One cannot account for the practices of these scientists by considering their epistemic practices in isolation from the political. Similarly, one cannot account for their interactions with their material surroundings—for example, their *Drosophila* experiments—as abstracted from the discursive articulations about them, that is, the arguments embedded in the interpretations of experiments and in the classical/ balance controversy.

But we can say more than the political motivations Lewontin indicates are intertwined with the epistemic. Indeed, we can further say that each of the issues or norms mentioned above can be seen as having both a political and an epistemic component. For example, the issue of whether to elect Wallace to the NAS in 1962 had an epistemic component—whether his experimental conclusions followed from his data, whether his “more controversial theories” will be confirmed in the future, and the like. In addition, it had a political component—personal disagreements and enmities related to the classical/balance controversy, feelings about the mentor-protégé relationship, concerns about tailoring interpretations to suit funding agencies, concerns about the policy implications of heterosis in the context of the fallout controversy, and the like. The epistemic and the political are, as



Rouse (2002a) argues, “intra-twined.” In fact, each of the subcomponents mentioned above can be seen as having both epistemic and political components itself; the boundary between each of them is blurred in the practices of the real world.

For example, the weighty issue of whether one sees experimental conclusions as following from the data, in the face of the epistemological uncertainty involved in interpreting experimental results designed to separate signal from noise, itself presupposes political (power/knowledge) concerns: for example, the reputation and professional standing of who is making the interpretation in relation to others in the relevant community; the reputation and professional standing of those in the “schools” of the relevant community or discipline who will evaluate the interpretation; the accepted norms of data interpretation or statistical evaluation of data, which ultimately are culturally embedded and historically contingent decisions that change over time; the interests and political motivations of the relevant funding agencies; the personal and professional motivations of individuals involved; and so on.

To illustrate further the significance of the issue of how scientists come to see experimental conclusions as following from the data, and how that “coming to see” is epistemic/political (in addition to material/discursive, etc.), consider the following example regarding peer review and experimental interpretation. In 1977, Lewontin was asked by Robert R. Sokal, the Acting Chairman of the Department of Ecology and Evolution at State University of New York at Stony Brook, to provide a confidential recommendation of Francisco Ayala, one of Dobzhansky’s former students, who was being considered for a senior position in the department.<sup>285</sup> Lewontin put off answering Sokal’s letter for almost a month, and then wrote to Sokal, stating that “it would be wrong for me to evade your request, so I am answering frankly and fully.” He indicated that he did “not have a favorable impression of Ayala.” Lewontin continued:

This unfavorable impression has built up over a number of many years since he was a graduate student of Dobzhansky’s. A number of events and the whole general trend of his professional work combine to convince me that Francisco is not a healthy influence either on our field or on the training of graduate students. Francisco is the

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<sup>285</sup> Sokal to Lewontin, 28 March 1977, Richard Lewontin Papers, APS.

extreme case of a careerist who has allowed his career ambitions to mold everything he does. As a result, although I do not believe he has actually ever falsified any data, or told any direct lies, he has gone to the very edge of scientific honesty, and I would classify him as generally intellectually dishonest. (Lewontin to Sokal, 20 April 1977, Richard Lewontin Papers, APS)

On the subject of Ayala's interpretation of his data, Lewontin went into some detail on why he thought Ayala's interpretations of his data were on "the very edge of scientific dishonesty":

On several occasions, in past years, while Dobzhansky was still alive, Francisco sent me manuscripts of papers that he was about to publish asking for my comments. In each case, I found that the summary of the observations did not correspond to the tables of data and, indeed, in one case, there seemed to be a very serious discrepancy within the data set given. When I pointed this out to Francisco, he excused himself by saying that there were yet other data that he did not have space enough to give in the paper which would substantiate his conclusions, and in the case of the inconsistent data set, he said, well, it was too late to do anything about it because the paper was already approved for publication. The inconsistency involved the estimates of heterozygosity for a large number of populations over a large number of enzymes in one of his very big studies. It appeared that the average heterozygosity when averaged over enzymes came out to be different than the average heterozygosity when averaged over populations. My further analysis showed precisely what had happened. In some populations, Francisco had looked at a lot of enzymes, and in some populations, only three or four. In those populations where a very large number of enzymes were looked at, it turned out to be a moderate amount of heterozygosity. In those populations in which only three or four enzymes were looked at, the heterozygosity was extremely large. What in fact he had done was to look only at the polymorphic loci in many of the populations, since he got tired of looking at all those monomorphic ones. When I pointed out to him that there was this immense discrepancy and therefore the results were biased, he excused himself by saying, well, the paper was already in press and it didn't really matter. (*ibid.*)

Lewontin also wrote in his letter that Ayala had "engaged in a shameless sycophancy in regard to Dobzhansky in order to further his own career. . . ." Indeed, Lewontin also indicated that he had "received bitter complaints from two of [Dobzhansky's] former graduate students. . . ." <sup>286</sup> He concluded: "This is a man . . . for whom I have very little use"

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<sup>286</sup> The complaints Lewontin received were "to the effect that he [Ayala] used them and their data without any permission and in one case actively prevented one of them from publishing a paper while he, Francisco, hurriedly produced the same experiment and sent it in." Lewontin also indicated that "there has been no particular bad blood between Ayala and me and I am not damning him for some petty personal reason.

(*ibid.*).

One point that is significant about Lewontin's evaluation of Ayala, and regarding Lewontin's view of science in general, is that he believes that Ayala's actions are *not* the exception in science (see also Lewontin 2004): "I am aware that these matters are not uncommon in science, but I do not think that we should give them our approval" (*ibid.*). Lewontin (2006) described another example of Ayala "playing a game" with statistics. In one paper by Ayala that Lewontin peer reviewed, Ayala performed two experiments to test an effect. The first experiment, if statistically significant, would have confirmed Ayala's finding, and the second, if not statistically significant, would have refuted the claim of the first experiment. The first experiment, as Lewontin (2006) recalls, was significant with  $p=3\%$ , which supported his (Ayala's) claim. However, the second experiment resulted in a significance of  $p=7\%$ , and with the confidence level set at 5%, it should have resulted in Ayala's rejecting the claim. Although Lewontin (2006) believes there is nothing in statistical techniques that ought to be reified, and that the confidence levels chosen are "arbitrary," he recommended against publication; the paper was not published. His recommendation against publication amounted to a consistency argument; in effect, even though the statistical confidence levels are ultimately arbitrary, you cannot have it both ways.<sup>287</sup>

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There exist a number of people in science who make a profession out of challenging me, but I nevertheless have immense respect for them. Francisco is not a scientific opponent of mine and I have reported to you only reluctantly my evaluation of his work and career" (Lewontin to Sokal, 20 April 1977, Richard Lewontin Papers, APS). Lewontin (2006) recalls that Ayala did not get the job.

<sup>287</sup> The controversy over the withdrawn arthritis drug Vioxx, marketed by the pharmaceutical company Merck (in addition to Pfizer's COX-2 inhibitor, Celebrex), is one recent illustration of how the epistemic and the political are intratwined in practice. Of course, one could argue that this example is a relatively isolated instance, and is not representative of how "real" science is done. However, the debates over the interpretation of the Vioxx data, and the related debates spawned concerning conflicts of interest among the officials of the U. S. Food and Drug Administration (FDA) and their ties to the pharmaceutical industry, in addition to the questioning of the efficacy of the peer review process in properly reviewing and vetting prospective papers, suggests that many of the same issues at work in the radiation genetics narrative above are still here with us today, and are apparently ubiquitous. See, for example, Gregory D. Curfman, M.D., Stephen Morrissey, Ph.D., and Jeffrey M. Drazen, M.D., "Expression of Concern Reaffirmed," *The New England Journal of Medicine* 354: 1193, 16 March 2006; Alex Berenson, "Scientists Again Defend Study on Vioxx," *The New York Times*, 23 February 2006, p. C18; Andrew Pollack and Reed Abelson, "Why the Data Diverge on the Dangers of Vioxx," *The New York Times*, 22 May 2006, p. C1; Ceci Connolly, "Distance Sought Between Doctors and Drug Industry," *The Washington Post*, 25 January 2006, p. A8; Marc Kaufman, "Drug Safety Panel Is Criticized: Efforts to Protect Consumers at Risk, Say Senator, FDA Official," *The Washington Post*, 8 June 2005, p. A5; Alastair J. J. Wood, M.D., Jeffrey M. Drazen, M.D., and Michael F. Greene, M.D., "A Sad Day for Science at the FDA," *The New England Journal of Medicine* 353: 1197-99, 22 September 2005; Lawrence K.

### III. *The Significance of Wallace's Experiments*

Lewontin's interpretations of the actions and work of Dobzhansky, Wallace, and Ayala, in conjunction with the narrative on radiation genetics, suggest several significant claims about science. First, as indicated, it is difficult to maintain that there is a sharp

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Altman, M.D., "For Science's Gatekeepers, A Credibility Gap," *The New York Times*, 2 May 2006, p. D1; Bruce M. Psaty, M.D., Ph.D. and Sheila P. Burke, M.P.A., R.N., "Protecting the Health of the Public: Institute of Medicine Recommendations on Drug Safety," *New England Journal of Medicine* 355: 1753-55, 26 October 2006; Jeffrey Avorn, M.D., "Dangerous Deception: Hiding the Evidence of Adverse Drug Effects," *The New England Journal of Medicine* 355: 2169-71, 23 November 2006; R. Jeffrey Smith and Jeffrey H. Birnbaum, "Drug Bill Demonstrates Lobby's Pull: Democrats Feared Industry Would Stall Bigger Changes," *The Washington Post*, 12 January 2007, p. A1.

In addition, a *Washington Post* article reported that in the "first large-scale survey of scientific misbehavior," researchers found that a relatively large number of scientists engaged in "misconduct," which included throwing out data, bypassing human research protections, improperly including their names on others' papers, changing "a study's design or results to satisfy a sponsor, or ignor[ing] observations because they had a 'gut feeling' they were inaccurate," plagiarism, faking data, using the same data in more than one paper to beef up their résumés," and using research designs they knew would not give accurate results. According to the article: "A preliminary analysis of other questions in the survey, not yet published, suggests a link between misconduct and the extent to which scientists feel the system of peer review for grants and advancement is unfair" (Rick Weiss, "Many Scientists Admit to Misconduct: Degrees of Deception Vary in Poll; Researchers Say Findings Could Hurt the Field," *The Washington Post*, 9 June 2005, p. A3).

Finally, there is the voluminous public record of the Bush (George W.) Administration's politicization of scientific research activities, including global warming and climate change, stem cell research, clean air and water research, emergency contraception, gay adoption and sperm donation, and others, in addition to the allegations of pressure put on the FDA to approve new drugs, and of an unethical pipeline of officials moving from government regulation to industry lobbying positions. Each of these examples, if probed sufficiently, shows how, in practice, the epistemic and the political are intratwined. For a comprehensive review, see Union of Concerned Scientists (2006), "Evidence of Political Interference: The A to Z Guide to Political Interference in Science," [http://www.ucsusa.org/scientific\\_integrity/interference/a-to-z-guide-to-political.html](http://www.ucsusa.org/scientific_integrity/interference/a-to-z-guide-to-political.html), accessed 16 January 2007. See also Juliet Eilperin, "Debate on Climate Shifts to Issue of Irreparable Change: Some Experts on Global Warming Foresee 'Tipping Point' When It Is Too Late to Act," *The Washington Post*, 29 January 2006, p. A1; Christopher Lee, "NASA Administrator Calls for Openness in Statement to Staff," *The Washington Post*, 5 February 2006, p. A9; Juliet Eilperin, "Censorship is Alleged at NOAA: Scientists Afraid to Speak Out, NASA Climate Expert Reports," *The Washington Post*, 11 February 2006, p. A7; Peter Baker, "President Vows Veto On Stem Cell Research: Bipartisan Measure Seeks to Ease Curbs," *The Washington Post*, 21 May 2005, p. A6; Juliet Eilperin, "Looser Emission Rules Rejected: Court Says Changes By EPA Violated Clean Air Act," *The Washington Post*, 18 March 2006, p. A1; Juliet Eilperin, "EPA Faces Internal Outcry On Airborne Emissions Plan," *The Washington Post*, 4 April 2006, p. A4; January W. Payne, "Is Plan B 'Unsafe'? Current Research Does Not Support Fears Of Day-After Pill Dangers," *The Washington Post*, 6 September 2005, p. HE1; Marc Kaufman, "FDA Official Quits Over Delay on Plan B: Women's Health Chief Says Commissioner's Decision on Contraceptive Was Political," *The Washington Post*, 1 September 2005, p. A8; Susan F. Wood, "When Politics Defeats Science," *The Washington Post*, 1 March 2006, p. A17; Michael Kranish, "Beliefs Drive Research Agenda of New Think Tanks: Study on Gay Adoption Disputed by Specialists," *The Boston Globe*, 31 July 2005; Marc Kaufman, "Gay Groups Assail Sperm Bank Rule," *The Washington Post*, 7 May 2005, p. A2; Marc Kaufman, "FDA Is Criticized Over Drugs' Safety Problems: Response to Approved Medications Cited," *The Washington Post*, 24 April 2006, p. A5; Editorial, "Lobbying From Within," *The New York Times*, 17 June 2005; Marc Kaufman, "Former FDA Chief Joins Lobby Shop," *The Washington Post*, 8 February 2006, p. A6.

distinction between epistemic matters and political matters in practice. Where does the epistemic end and the political begin? One cannot adequately answer this question when considering the actions of the scientists described in this chapter, if one bifurcates the two and considers them as separate components of practice. In addition, there is no sharp distinction between the material and the discursive. That is, where the material ends—for example, the induced mutations in the chromosomes of *Drosophila* and their structural and inherited reality—and the discursive begins—for example, descriptions of Wallace’s experiments and his interpretations of the generated data, is again blurred in practice. Recourse to the eventual “correct” answer, even if available, does not help, for it cannot help adequately to account for the actual historical context of the time, and requires appeal to some form of epistemic sovereignty. Rouse’s (2002a) commitment to blurring the boundaries between material/discursive and epistemic/political, embodied in his account of causal intra-action in the world, clearly has import for this story of radiation genetics in the Cold War.

Finally, the present analysis of Dobzhansky’s and Wallace’s work fits in well with the critiques of the adaptationist program explored in Chapter VI above. One of the central issues there is the supposed unfalsifiability of the core of the research program—in effect, the research program itself. Adaptationism, and in Dobzhansky’s and Wallace’s case, heterosis, was the grand metanarrative imposed on the data and theory to unify and to attain explanatory power, and to ensure future experiments. At stake was the issue of testability, in the present and in the future. With heterosis, Wallace had a mechanism that promised future experimental practices, practices that would generate tests, if not of heterosis, then of phenomena explainable by heterosis. Acceptance of heterosis guaranteed *future testable predictions*; it made the doing of evolutionary genetics possible. As Wallace wrote in his 1958 research proposal to the AEC:

This type of investigation has been working well and the directions of future work are fairly obvious: analyses of dosage-response relationships, comparisons of variations in irradiation dosage with variations in “genetic” dosage, analyses of the effects of random mutations on a series of backgrounds varying systematically in the proportion of homozygous loci, and diverting the argument to traits other than viability. (“The Investigation of the Genetic Structure of Populations,” Curt Stern Papers, APS)

As Robert Brandon (1978) notes, without adaptationism “there is no *theory* of evolution, there are only low level theories about the evolution of certain organisms in certain environments. . . . With [adaptationism] Darwinian theory is possible” (p. 204, emphasis added).

These stories suggest that questions concerning the history and philosophy of scientific experimentation should be answered without a commitment to epistemic sovereignty. Indeed, a commitment to epistemic sovereignty is incompatible with the present historical analysis. How could maintaining the sovereign objectivity of science make sense of the political and epistemic problems involved in the history of radiation genetics, on the one hand, and still make sense of the successful practice of science, on the other hand?

For example, had I followed Franklin (e.g., 1986, 1990, 1995, 1997, 2002) and limited myself to published scientific papers, this analysis would have been quite different, in a number of ways. And remember, Franklin’s early primary methodology was to rationally reconstruct science using Bayesian probability theory, as though scientists were “Bayesian agents” who calculate—if not consciously, then in practice—prior probabilities and then plug them into the equation for Bayes’ Theorem in order to proceed and make theory choices rationally. Indeed, Franklin (2002) adheres to the standard inductive view of experimental practice described above, and is fully committed to the view that scientific practice is based on a rational and epistemically privileged methodology:

Scientists decide what the valid experimental evidence is and then make their theory choice, *not vice versa*. Scientists have an interest in producing scientific knowledge, as well as a career interest in being correct, and such a procedure [based on rational epistemological and methodological criteria] is far more likely to produce a correct choice [than one interpreted on a constructivist view of science]. Without evidence as a guide, how are scientists supposed to make such a choice? They might just as well flip a coin. (pp. 243-44, emphasis added)

As presented in the narrative above, scientists *do* use scientific evidence as a guide, and that evidence has a basis, although that basis must be *interpreted*; the data do not speak for

themselves and “nature” is present only indirectly, at best,<sup>288</sup> in the phenomena of the real world. In this sense, then, Franklin is correct to reject constructivist approaches that deny *any* role to evidence, and its connections to the phenomena of the real world. However, to suggest that the arguments and methods, contained or reflected in the published papers of scientists, exhaust the ways scientists in their practices come to generate or accept scientific theories, is at best a caricature of actual scientific practice.<sup>289</sup> Methodologically, Franklin’s adherence to this view is an historiographical move, and for the present analysis it would have had the following effects:

First, it would have been limited to those aspects of the technological infrastructure of science inside the laboratory. This would involve solely an examination of the arguments for and against the classical and balance views of population genetics, limited to the arguments presented in the published papers. Further, if one were to choose a “winner,” so to speak, one would be presuming exactly what needs to be problematized—that is, epistemic sovereignty, located *somewhere*.

Second, a number of intersecting and overlapping narrative contexts would have had to be severed from the analysis. These include virtually all aspects of the technological infrastructure *outside* the laboratory, including the informal, yet very real, network of interactions among scientists—examples are peer reviews of papers, confidential recommendations, and a number of types of personal interactions involving power relationships. Additionally, the significant historical context of radiation exposure policy-setting in the Cold War—which clearly intersected with the classical/balance narrative—

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<sup>288</sup> These lessons come from Latour’s (1987) *Science in Action: How to Follow Scientists and Engineers Through Society* (see ch. 2, esp. pp. 67, 70-74). Although we may take issue with Latour’s silence on the role of the material (micro)world, and with his conflation of the agency of humans and material objects—“in practice, there is not much difference between people and things: they both need someone to talk for them” (p. 72)—there is still much we can learn from Latour.

<sup>289</sup> One could argue, of course, that Franklin’s prescriptions concerning physics, the focus of his writings, might still be tenable, since the methods of physics might still be found to be superior to those of the biological sciences, especially early population genetics. What is needed, then, are accounts of the episodes he analyzes written from the perspective of the technological infrastructure of science. Such accounts—written, for example, in the spirit of Galison’s (1987) *How Experiments End* and *Image and Logic: A Material Culture of Microphysics* (1997), supplemented with more emphasis on power/knowledge analyses and upfront specifications on how physics captures material reality (if at all)—may turn out to provide coherent reconstructions of Franklin’s episodes that come to very different conclusions concerning the practice of science, even if the results of many particular physics experiments turn out to be overdetermined.

would have to be excluded from the analysis. But the present analysis of the historical evidence shows that this context had quite demonstrable ramifications for the process by which experimental systems and laboratory results were extended beyond the laboratory and into the future. The genetics committees of the National Academy of Sciences, the United Nations, the Atomic Energy Commission—all played roles in legitimating various experimental results, *and* in creating new narrative contexts (for example, danger from the genetic effects of radiation) that spawned new experimental systems and new experimental results and new narrative contexts . . . and so on.

Indeed, this narrative reconstruction of the technological infrastructure of radiation genetics had to proceed by first doing away with the presupposition of epistemic sovereignty, if an understanding of this episode was to be compatible with the postmodern and naturalistic presuppositions presented in Chapters II, III, IV and V above. As Hacking (1992) put it:

The process of modifying the workings of instruments—both materially (we fix them up) and intellectually (we redescribe what they do)—furnishes the glue that keeps our intellectual and material world together. It is what stabilizes science. (p. 58)

However, Hacking continues, we should not make the

metaphysical mistake of thinking that truth or the world explains anything. . . . [W]hat we want to be the case in mission-oriented research is that the reproducible apparatus (or chemical or whatever) also has happy effects in the untamed world. But it is not the truth of anything that causes or explains the happy effects. (p. 60)

Hacking's analysis, focusing on the physical sciences and the creation of laboratory phenomena, it seems, can be extended to other realms of science, including the experimental biological sciences and the medical sciences, for which statistical arguments help to furnish the persuasiveness for deciding whether or not signal has been separated from noise.



## CHAPTER IX

### Conclusion

Speaking a lot about something does not in the least guarantee that understanding is thus furthered. On the contrary, talking at great length about something covers things over and gives a false impression of clarity to what is understood, that is, the unintelligibility of the trivial.

—Martin Heidegger<sup>290</sup>

### Signal/Noise Revisited

The analysis of the case study on radiation genetics at the end of Chapter VIII returns the discussion to where it started in the beginning of Chapter I above—namely, the issue in the Philosophy of Experiment of separating signal from noise. I began this dissertation by noting that one way to conceptualize what scientists do (at least part of the time) is to view them as engaging in practices that involve interacting with their world and creating material/discursive arguments for why they believe signal has been separated from noise, or entity has been isolated from artifact, in particular cases. I claimed that several disciplines, such as the Philosophy of Science and the History of Science, whose practitioners share this basic viewpoint, have been dominated by an epistemological concern to show how and why the practices or methods of the sciences are epistemically privileged—this is epistemic sovereignty, as borrowed from Joseph Rouse (1993a, 1996a). I argued that by rejecting epistemic sovereignty and instead focusing on narrativity, it should be possible to create stories of scientific experimentation that are more faithful to the past, *wie es eigentlich gewesen ist*, than stories that take epistemic sovereignty and other metanarratives of modernity as their starting points. To do this, I explored several issues in historiography, philosophy of history, and philosophy of science, including reality, temporality, and causality, in order to show how a postmodern perspective, which includes a non-linear view of time, might handle them in the light of an emphasis on narrativity. I used several

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<sup>290</sup> Heidegger ([1953] 1996), p. 154.

examples to illustrate this, including scientific creationism and the Smithsonian's *Enola Gay* exhibit. I developed this focus on narrativity into a (preliminary) philosophical position—postmodern naturalism—based largely on the works of Joseph Rouse and Hans-Jörg Rheinberger, and used this position to construct a prescription for creating narratives about science, called *the technological infrastructure of science*, based significantly on Joseph Pitt's construct of the same name. I explained how this construct prescribes that one ought to problematize certain dualisms of modernity, such as epistemic/political, material/discursive, theory/experiment, nature/normativity, and nature/culture, and that one should instead see these traditional binary oppositions as inextricable, or “intra-twined.” After showing how the technological infrastructure construct differs from other efforts to account for scientific change, I deployed it and deconstructed a text on Einstein's special theory of relativity, and showed how some of the major claims of the text could be re-evaluated in terms of their philosophical tenability and historical accuracy. Then, after probing some of the epistemological issues underlying evolutionary biology, I used the technological infrastructure of science to construct an historical account of population genetics and radiation standard policy-setting in the Cold War, involving many intersecting narratives, scientists, and policy-makers. In this story, the significant futurally-oriented norms that helped account for the events considered included the prospects for future research, the enculturation of radiation risks, deliberately creating epistemic confusion, and the fitting of evidence into pre-conceived scientific and/or policy positions. Finally, central to the norms expressed by the practices I described, was the ongoing effort of scientists to attempt to separate signal from noise.

What, then, am I entitled to say about the sciences—the practices dedicated to separating signal from noise—given the presuppositions developed through postmodern naturalism and the technological infrastructure of science? First, stories of the phenomena and entities of science should be told from the local, partial, cultural perspectives of those (humans and nonhumans) interacting among them. As Arthur Fine (1984, 1986) instructed us, to begin with, take scientists at their word; science is what its practitioners say it is. However, from the lessons of narrativity, cultural studies and cultural anthropology, and

Rouse's (2002a) view of normativity, reflexively recognize that you (I have again changed narrative strategies and am consciously using "you," as I am making prescriptions) are an author, an agent, who exists in an already given narrative/cultural context, and that you are subject to what is at stake in your own practices: norms that are futural, diachronic, often ineffable in real time, indeterminately reconstructable in the future, contestable. Clearly, you (the s/he who is telling or writing the story) must tell the story from within your own culture; however, even if you are an anthropologist, studying scientific activity in real time, you should use to your advantage the differences between your (sub)culture and the culture of those you are studying, as Sharon Traweek (1992) told us. Therefore, you are permitted to evaluate the epistemological foundations (epistemic norms) of the science you are studying, if you pay careful attention to time, place, and context—as does Donna Haraway (1997)—in the same way that you evaluate the broader cultural norms of the historical period you are studying.

Following Rouse's (2002a) normative causal intra-action, it is clear that stories about science involve physical and biological entities and phenomena that can have agency. Indeed, they do often play a role in the stories of science, and if the evidence suggests they have agency or even (perceived) ontological status, then the story should reflect that. Yes, this means that you can use later events as guideposts, but when you are retrospectively studying science in action, you must recognize that the entity or phenomena did not have the same epistemic/cultural status that it does today. Ultimately, the posthumanist stories that you tell ought to be faithful to the available evidence and not explicitly be based on pre-conceived philosophical or political positions, as Wilfrid Sellars (1968) instructed us, especially those that are submerged in the text. By advocating "evidence-based" research (or ethnography, cultural studies, cultural anthropology, etc.) I do not mean to exclude qualitative research. On the contrary, the varieties of qualitative research are critically important and should be defended against those who would marginalize them (e.g., the Bush Administration), but its results should nevertheless be based on the evidence it generates in ways that resonate with a community (or communities) of scholars.

Finally, for all the urgent need for a postmodern deflation of the varieties of truth we

have inherited from the Enlightenment and modernity,<sup>291</sup> a reverence for truth should continue to be your primary scholarly commitment, as Paul Woodruff (2001) instructed us. Without it, you would not be much different from Karl Rove, Rush Limbaugh, or many other lobbyists, politicians, and pundits. In effect, you would be in the postmodern hell David Harvey (1989) so adeptly predicted, or that is depicted in television shows such as *24*, and films such as *Slaughterhouse Five*, *Network*, *Blade Runner*, *Brazil*, *12 Monkeys*, *Pulp Fiction*, *Natural Born Killers*, *Memento*, and *The Name of the Rose*.

### **Prospects for Future Research**

To reflexively apply the principles of the technological infrastructure to my own role as author, I now undertake a specification of avenues for future research and a recognition of certain areas of this dissertation that would benefit from revision, re-evaluation, and/or improvement. First, improvement on the framework of postmodern naturalism would provide a more firmly situated position within contemporary philosophical scholarship. When I constructed this position in the period from late 2003 to late 2004, I based it on my own reading of Rouse (2002a), Rheinberger (1994, 1997), and Pitt (2000) and not on any secondary literature on their works. Indeed, most reviews of Rouse's book appeared after my reading and construction. Having now constructed it, replying to his critics would be helpful to my position.<sup>292</sup> For example, a deeper probing into the issues of norms, where they come

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<sup>291</sup> The irony here is that Rouse and Rheinberger owe more to Kant than to Hume, thus underscoring the notion that postmodernism is not (or should not be) a rejection of all aspects of modernity.

<sup>292</sup> See, for example, Stephen P. Turner (2005), "Normative All the Way Down," *Studies in History and Philosophy of Science* 36: 419-429; Rebecca Kukla (2004), "Review [of Rouse 2002a]," *Philosophy of Science* 71: 216-219; Sharon Clough (2004), "Joseph Rouse, *How Scientific Practices Matter: Reclaiming Philosophical Naturalism*," *Notre Dame Philosophical Reviews*, 7 October 2004, <http://ndpr.nd.edu/review.cfm?id=410>. In her review, Kukla points out, as I argue above in Chapters III and V, that there are problems regarding Rouse's naturalism in the light of historiography; she argues that Rouse's (2002a) "constitutive dependence" of material phenomena on normative practices "cannot be a temporal dependence, for it is important that it turn out that historical inquiry can disclose a determinate past consisting of objects and phenomena that have the proper kind of causal links to our current situation. And such inquiry must *now* disclose a world of objects that, though we now have stakes in their character, managed, prior to any inquiry, to exist independently of such stakes" (p. 219). She concludes that Rouse "will need to find a subtler temporal language for talking about how our practices *now* can both constitute and correctly disclose a past with appropriate naturalistic connections to the present" (*ibid.*). In this dissertation, I attempt to supplement and

from, and the justification of why nature should be taken to be always already normative, should be a fruitful endeavor. In addition, working out more fully the temporal problems created by Rouse's radical philosophical naturalism, not to mention probing more deeply into the philosophical views of Ricoeur, Heidegger, Derrida, Kermode, White, Haraway, Eco, and even Rheinberger, would also be helpful in understanding what issues need to be addressed when linking postmodern naturalism to broader historiographical concerns and the epistemology of time. Moreover, considering more deeply Rouse's Quinean, Nietzschean, and Davidsonian influences, in the negative and positive senses, (not to mention those of Robert Brandom and John Haugeland),<sup>293</sup> and the philosophical debates they engender, would also help to clarify the framework of postmodern naturalism.

In particular, if nature (the world in which we find ourselves) is always already normative, and if practices (human actions, including discursive articulations, which are or should be binding) express the norms (and not an overlay on an anormative natural world, as for Sellars) that are binding for those practices, so that norms, in some sense, are bodily, then how do we historically "account for norms," if norms are not meant in an explanatory sense? Did I violate Rouse's (2002a) radical naturalism when I used historical data to construct an account of what norms appeared to be operating in the past? That is, did I adequately construct my narrative to reflect the prescription that the norms I identified are not meant to "explain" anything, but are instead meant to be expressed by the past practices I identified? Was my effort to show how postmodern narratives are neither explanatory nor reductionist—and are in fact, if we accept the non-linear epistemological principles of time of Ricoeur, Rheinberger, White, Kermode and others, inherently indeterminate and lead to inevitable distortions—adequate in breaching the gap between Rouse's radical naturalism and the unavoidable historiographical concerns we will have to deal with if we are going to say

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reconfigure this gap in Rouse's (2002a) effort by synthesizing the technological infrastructure of science by using the work of several philosophers, historians, and other scholars, including Rheinberger, Pitt, White, Kermode, Ricoeur, and Eco.

<sup>293</sup> Rouse's (2002a) view of normativity is significantly influenced by the views of Brandom and Haugeland, whom I did not consider in this dissertation. Rouse devotes much effort to showing how his naturalism is both dependent on their views and an improvement upon them. See Robert Brandom (1994), *Making it Explicit: Reasoning, Representing, and Discursive Commitment*, Cambridge: Harvard University Press; and John Haugeland (1998), *Having Thought: Essays in the Metaphysics of Mind*, Cambridge: Harvard University Press.

anything meaningful about science, technology, and culture? And finally, did my argument concerning Heidegger's *Umsicht* and *Geschichtlichkeit*, according to which even scientists *in medias res* have as part of their practices a concern to recur and interpret historically, provide the needed link between Rouse's naturalism and postmodern historiography?

Second, I could ask the following question in a more systematic manner: do most scholars, especially historians and philosophers of science and technology, buy into the subversion of the epistemic/political dichotomy? If so, there are different "ways" to do this:

For example, one could agree that scientists' epistemic decisions/practices can be influenced by "nonepistemic" factors, but nevertheless conclude that such influence is something undesirable and reflects bad methodology. This is the outdated, internalist historiography discussed in Chapter II above.

Or, one could agree that scientific practices reflect the culture of the historical time period being investigated. For example, scientists investigated *this* particular subject matter because of cultural influences, or the scientists investigated it *in this way*, because of these particular cultural factors. One could still be committed to a bifurcation of nature/normativity—that is, the usual sense of philosophical naturalism, in which norms are "imposed" or an overlay on an anormative natural world, as for Sellars. In addition, one could still accept a bifurcation of epistemic/political in this case, if one's anormative naturalism meant that epistemic factors are (or should be) derived from the "behavior" or the "nature" of the natural world, while political factors are human constructs and essentially separable from the epistemic.

Or, one could go the postmodern route, as with Rouse and Rheinberger (and Foucault, Derrida, Nietzsche, and some philosophically-minded literary theorists, etc.), and argue that the epistemic and the political are "intra-twined" in the sense that we ought not view the "natural world" as anormative—it is normative "all the way down." You cannot have a clearly-defined natural world without discursive activity (including scientific activity that discloses and expresses the nature of that natural world), and such activity includes the operation, and later explication by historians and others, of *norms*. In addition, you cannot have norms without a natural world already being there (the world in which you find

yourself). So, as Rouse argues, neither makes sense without the other—they are not “inter-twined,” because that implies they are separate and just somehow mix together (as in the above examples). They are “intra-twined,” since they are mutually constitutive of each other.

My concern (or at least one of them) has been to show the “cash-value” of holding one or the other of these positions, as William James might say. Postmodern naturalism is pragmatic in at least this sense, and this is one reason that Pitt’s pragmatism-influenced technological infrastructure was a sensible starting point and reference point for this dissertation; it actually got me started on what I am doing and is now a part of it. One way to evaluate the cash-value is to analyze what kinds of stories about how science and technology work are generated when the story-teller—the person constructing the narrative—holds one of the above positions. To conduct such an analysis for specific groups of scholars, for example contemporary historians of science, would be an interesting project.

Further, if the story-teller does not explicitly engage such philosophical issues (which was a criticism Hayden White and others made of historians), then what can we say of those stories? One could say that those constructing them are not being reflective or even reflexive enough. However, if one accepts certain presuppositions about narrativity (along the lines of Derrida, Ricoeur, and other people who study narrative), then one could say that the historians are not excused, because answers (or implications) to the philosophical questions I asked above will be submerged in (or implicit in) their stories, their narratives, whether they intended them to be there or not. Indeed, original intent is not the issue—specifying how cultural factors influence one to tell stories one way or the other is not a direct function of mental intent, at least because reducing past events to mental intent leaves out the natural world. The issue should be to show the inseparability of norms and the natural world, and exploring how and whether scholars do this, especially historians of science, would be fruitful.

Third, in the extended narrative on radiation genetics in the Cold War of Chapters VII and VIII above, I focused on the epistemic/political dualism while only briefly considering several others, including material/discursive, theory/experiment, nature/normativity, and nature/culture. To probe more deeply the problematization of these dualisms, for the Cold

War narrative and for other historical case studies, would provide more empirical flesh for the technological infrastructure of science construct. For example, showing why experimental practices ought to be viewed as both material and discursive, and not as discursive mappings of inert nature, would be useful. I could probe into the details of Bruce Wallace's experimental strategies, for example, and show the limitations of the traditional view by illustrating how his experimental practices reflect the agency of the natural world, in addition to Wallace's discursively articulated agency, while being reducible to neither. Indeed, an account of his practices should require us to see that his practices existed in a cultural/material context in which both the material and the discursive are needed to express the norms that appear to be operating, norms that we must identify by doing historical research. To accomplish this, I would need to do further research utilizing the Bruce Wallace Papers, in addition to other geneticists' and scientists' papers. Furthermore, I would need to undertake a sustained and detailed examination of Wallace's experimental program on radiation genetics.

Fourth, expanding on the themes of the Cold War narrative could lead to numerous avenues of future research. For example, following the theme of risk enculturation farther into the future, and considering the controversies related to the development of nuclear power—for example, the infant mortality controversy, the issue of the genetic effects of radiation, the suppression of evidence and dissent within the United States government, and more general safety concerns regarding nuclear power and low-level radiation, such as medical and dental X-rays, shoe flourosopes, household radon, nuclear waste, and radium spas—would lead to a more comprehensive narrative on radiation risks and how they became enculturated in the U. S. or in other countries (see, e.g., Sternglass 1981).<sup>294</sup> Moreover, comparing the risks from radiation to other types of risks, such as cigarette smoking, pesticides, and other environmental contaminants, would be an interesting comparative project.

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<sup>294</sup> See also Walker (1994) and John W. Gofman, Ph.D., M.D. and Arthur R. Tamplin, Ph.D. (1971), *Poisoned Power: The Case Against Nuclear Power*, Emmaus, PA: Rodale Press. Gofman and Tamplin were asked by the AEC in 1963 to study the long-term effects of nuclear power, and eventually accused the AEC of suppression of evidence and intimidation.



Fifth, expanding the Cold War narrative on scientists and their political activity, including their government service and political activism, to include more historical detail on the geneticists involved and on other scientists, would be a fruitful project. Several works have considered the role of physicists and other “atomic scientists” in the years following the Manhattan Project and the establishment of the Atomic Energy Commission (AEC), which took over control of Manhattan Project research on 1 January 1947 (e.g., Gilpin 1962, Divine 1978, Seaborg 1981, Mazuzan and Walker 1984, Boyer 1985, Hewlett and Holl 1989). In the period from 1945 to about 1949 or 1950, the atomic scientists were cultural heroes. However, after the USSR exploded its first atomic bomb in 1949, with the outbreak of the Korean War in 1950, and the advent of increasing anti-Communist paranoia and McCarthyism, the political climate for even physicists and others in the “hard sciences” deteriorated. Clearly, those government scientists who did not dissent, as did Oppenheimer on the decision to build the hydrogen bomb, and were instead involved in weapons development, such as Edward Teller, were kept in the highest circles of government policy-making.<sup>295</sup>

Nevertheless, with the waning of extreme McCarthyism and with the advent of the international controversy over the dangers of fallout, which emerged in the years after the 1954 *Bravo* shot, the cultural climate began to change for the political dissent and activism of scientists (and others, as well). With the Presidential election of 1956 and the emergence of the banning of atmospheric testing of atomic weapons as a political issue—not to mention the 1957 Pugwash Conference, the 1957 Congressional Hearings on Fallout, and Sputnik—there was in the period of the late 1950s and the early 1960s, despite the 1962 Cuban Missile Crisis and the U. S. government’s continued strategic reliance on an arsenal of both first-strike and tactical nuclear weapons, the beginnings of a cultural change with regard to the political dissent and activities of scientists, both inside and outside of government. In Chapters VII and VIII above, I pointed to some evidence for this and suggested that a sustained historical examination of this period, particularly if the examination focused on the biological and

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<sup>295</sup> See, for example, Gilpin (1962); Don K. Price (1956), *The Scientific Estate*, Cambridge: Harvard University Press; Daniel S. Greenberg (1967), *The Politics of Pure Science*, New York: New American Library; and David Dickson (1984), *The New Politics of Science*, New York: Pantheon Books.

medical sciences, might result in a narrative that showed how and why this cultural change occurred. Many of the works that have examined this period have not fully explored the rise of scientists' groups and the increasing political activity of scientists, beginning in the period of the early 1960s (e.g., Walker 1992, 1993, 1994), while others have not fully or carefully explored the epistemological issues involved in the scientific disputes of this time (e.g., Sternglass 1981; Gofman and Tamplin, *op. cit.*; Commoner 1971). An analysis of the period from the Partial Test Ban Treaty of 1963 to the Three Mile Island nuclear powerplant accident in 1979, one constructed from the perspective of the technological infrastructure of science, ought to be a fruitful project.<sup>296</sup> Clearly, concerns regarding nuclear power, low-level radiation, and nuclear war began to emerge in this period, while an environmental movement focused on the dangers of pesticides was also beginning (Watson 2001). Connecting these concerns to the culture of the 1960s and 1970s, using the principles of the technological infrastructure, is a work waiting to be written.

Finally, a traditional issue in the Philosophy of Science has been methodology—that is, what is or should be the proper methodology (or methodologies) of science, and how we ought to characterize that methodology. In Chapters VII and VIII above, I explored this issue in the context of the practice of population genetics and radiation standard policy-making in the 1950s and early 1960s. I argued that the narrative I constructed indicated that the traditional, Baconian inductive methodology, according to which scientists first generate data in experiments and then decide what theory they will accept that explains the data, was inapplicable to the experimental practices I described (scientists do use a variety of other

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<sup>296</sup> A number of relevant archival sources are available that would potentially be useful in such a project. These include Atomic Scientists Miscellaneous Records at the University of Chicago Library; various papers on the BAS, peace, and other topics in the Ava Helen and Linus Pauling Papers at Oregon State University; the genetics collection at the American Philosophical Society Library in Philadelphia; the minutes of meetings of the AEC, ACBM, ABCC and other papers at the National Archives in Washington, D. C. and College Park, Maryland, and at the DOE archives in Germantown, Maryland; papers on the ACBM at the University of Tennessee at Knoxville; the John McCone Papers at the Dwight D. Eisenhower Library in Abilene, Kansas; the Nuclear Testing Archive in Las Vegas; the Paul C. Aebersold Collection at Texas A and M University Libraries; the Hermann J. Muller Papers at Indiana University; the Project Sunshine Reports and the Miscellaneous Physics papers at the Niels Bohr Library of the American Institute of Physics at College Park, Maryland; and various books, papers, and archives at the National Library of Medicine in Bethesda, Maryland. Clearly, the archival sources mentioned here are only the tip of the iceberg. Many other sources are available that could provide fruitful sources of documentary research, including numerous Congressional hearings in the 1960s and 1970s.

methods, but they generally do not condone researchers searching around for confirming data on a theory in need of support). This version and related varieties of “the scientific method,” still taught in many public schools in the U. S. and even in some college classes,<sup>297</sup> is all but self-refuting propaganda from the viewpoint of most scholars in science studies. An interesting project would be to show to what extent the discipline of Philosophy of Science, in addition to other disciplines and avenues (e.g., media sources), has contributed to this cultural viewpoint, one that still seems to be prevalent among the general public. An irony of contemporary American culture is that on the one hand, Americans seem to adhere to a view of science that incorporates epistemic sovereignty and a fixed, objectivist, and privileged scientific method, while on the other hand, a majority of Americans also believes in some form of creationism or Intelligent Design as the explanation for the diversity of life on earth, at the expense of evolutionary theory. Indeed, when speaking of Newton or Einstein, one speaks of “the law of gravity” or “the special theory of relativity” as though these scientific theories are well-confirmed, established, and even eternal truths (when, in fact, their epistemological foundations are just as contestable as evolutionary theories), while the “theory of evolution” is deemed “only a theory” as though “theory” is in this case to be used in the sense of “not well-confirmed,” merely “provisional,” or otherwise secondary in epistemic value to ostensibly well-established scientific laws.

Exploring how American culture got to this point, to what extent the Philosophy of Science has contributed to this cultural phenomenon, and whether it, or related fields of inquiry, can be useful and/or successful in changing these widespread beliefs, ought to be an interesting and useful project. There is no doubt that the efforts of many past thinkers, who have called themselves philosophers of science, have contributed immensely to our understanding of many issues that are relevant to our practices as scholars in science studies. Indeed, without those past efforts, our interdisciplinary field would not be where it is today,

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<sup>297</sup> See, for example, Carol L. Smith and Laura Wenk (2006), “Relations among Three Aspects of First-Year College Students’ Epistemologies of Science,” *Journal of Research in Science Teaching* 43: 747-785; Fouad Abd-El-Khalick and Norman G. Lederman (2000a), “Improving Science Teachers’ Conceptions of Nature of Science: A Critical Review of the Literature,” *International Journal of Science Education* 22: 665-701; Fouad Abd-El-Khalick and Norman G. Lederman (2000b), “The Influence of History of Science Courses on Students’ Views of Nature of Science,” *Journal of Research in Science Teaching* 37: 1057-1095.

and all those who contribute to science studies should have an adequate understanding of that historical fact, if not a reverence for the truths those past philosophers of science have generated, in addition to those concepts and principles that have been consigned to the trash-heap of history.

Nevertheless, we can and should ask: “Should the History of Science be Rated X?” any longer (Brush 1974); “Should the Philosophy of Science be Rated F?” where “F” stands either for “fictional” or “fecund” (which is it?); and “Should Science and Technology Studies be Rated CSI?” where “CSI” stands for “culturally sterile and irrelevant”—these are all questions, or perhaps article titles, or perhaps cultural criticisms in the spirit of Umberto Eco, that mark possible futurally-oriented configurations of embodied normative causal intra-action in the real world. And so it goes.<sup>298</sup>

And any action

Is a step to the block, to the fire, down the sea’s throat  
Or to an illegible stone: and that is where we start.  
We die with the dying:  
See, they depart, and we go with them.  
We are born with the dead:  
See, they return, and bring us with them.  
The moment of the rose and the moment of the yew-tree  
Are of equal duration. A people without history  
Is not redeemed from time, for history is a pattern  
Of timeless moments.<sup>299</sup>

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<sup>298</sup> Kurt Vonnegut, Jr. (1922-2007) used this phrase (“So it goes.”) in his novel *Slaughterhouse-Five; or, The Children’s Crusade: A Duty-Dance With Death* ([1969] 1991), in which he tells the story of how he witnessed, as an American prisoner of war, the firebombing of Dresden, Germany near the end of World War II. He used it to indicate death in a way that is both ironic and humorous, although *not* to trivialize death, but to encapsulate in three words both the immense, overwhelming sadness and incomprehensibility of mass killing, and also the ultimate, ironic realization that we must, in order to live, come to terms with it. My sadness and stinging irony that I experienced when writing this dissertation was Vonnegut’s own death five days before the tragic events of April 16 at Virginia Tech. I was not in Blacksburg on that day, but I feel like a witness. For all its irony, I can say: I am proud to be a Hokie. So it goes.

<sup>299</sup> T. S. Eliot, “Little Gidding,” (1942) lines 225-235, in *Four Quartets* (©1943, 1971), San Diego: Harcourt, Inc., p. 58; reprinted by permission of Harcourt, Inc.

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Curt Stern Papers

### II. DOE/NV Nuclear Testing Archive, Las Vegas, Nevada

### III. National Library of Medicine, Bethesda, Maryland

Linus Pauling Papers  
Shields Warren Oral History

## APPENDIX A

### List of Acronyms and Abbreviations

AAAS	- American Association for the Advancement of Science
AAP	- American Academy of Pediatrics
ABCC	- Atomic Bomb Casualty Commission (of NAS-NRC)
ACBM	- Advisory Committee on Biology and Medicine (of AEC)
AEC	- United States Atomic Energy Commission
AECU	- Atomic Energy Commission Unclassified
ANP	- Aircraft Nuclear Propulsion
APS	- American Philosophical Society Library, Philadelphia, PA
BAS	- <i>Bulletin of the Atomic Scientists</i>
BEAR	- Biological Effects of Atomic Radiation (NAS-NRC Committee)
BEIR	- Biological Effects of Ionizing Radiation (successor to BEAR)
CCNI	- Greater St. Louis Citizens' Committee for Nuclear Information
CEH-AAP	- Committee on Environmental Hazards of the AAP
Ce-137	- Cesium-137 (fission product)
CTBT	- Comprehensive Test Ban Treaty (1996)
DBM	- Division of Biology and Medicine (of AEC)
DHHS	- Department of Health and Human Services
DHS	- Department of Homeland Security
DOC	- Department of Commerce
DOD	- Department of Defense

DOE	- Department of Energy
DOL	- Department of Labor
DOT	- Department of Transportation
DRH-PHS	- Division of Radiological Health of Public Health Service
EPA	- Environmental Protection Agency
FAS	- Federation of American Scientists
FEMA	- Federal Emergency Management Agency
FRC	- Federal Radiation Council
GAC	- General Advisory Committee (of the AEC)
GAO	- Government Accountability Office (previously General Accounting Office)
HEW	- Department of Health, Education, and Welfare
I-131	- Iodine-131 (fission product)
IAEA	- International Atomic Energy Agency
ICRP	- International Commission on Radiological Protection
JAMA	- <i>Journal of the American Medical Association</i>
JCAE	- Joint Committee on Atomic Energy (of U. S. Congress)
LD-50	- Lethal dose, 50% fatalities
MDDC	- Manhattan District Declassified
MPD	- Maximum Permissible Dose
NACR	- National Advisory Committee on Radiation (of PHS)
NAS	- National Academy of Sciences (of the USA)

NCRP	- National Committee on Radiation Protection (under National Bureau of Standards, Department of Commerce)
NRC	- National Research Council (of NAS)
NRC	- Nuclear Regulatory Commission (of DOE, successor agency to AEC)
NRDL	- Naval Radiological Defense Laboratory
NRL	- Naval Research Laboratory
NYO	- New York Operations Office (of AEC)
ONR	- Office of Naval Research
PAG	- Protective Action Guide (FRC radiation guidelines)
PHS	- Public Health Service
PTBT	- Partial Test-Ban Treaty (1963)
Rad	- Radiation absorbed dose; unit of absorbed dose equal to 100 erg/gm
REM	- Roentgen Equivalent Man (unit of dose equivalent)
RERF	- Radiation Effects Research Foundation (successor to ABCC)
RHC-FAS	- Radiation Hazards Committee of FAS
Roentgen	- unit of radiation that measures its power to ionize a given amount of air
SANE	- Committee for a Sane Nuclear Policy
SCRI	- Scientists' Committee for Radiation Information (of New York City)
SLAM	- Supersonic Low-Altitude Missile
Sr-90	- Strontium-90 (fission product)
UNSCEAR	- United Nations Scientific Committee on the Effects of Atomic Radiation
WHO	- World Health Organization



## APPENDIX B

### Questions for Richard Lewontin

**From Michael Seltzer  
June 18, 2006**

In general, my dissertation is about doing research on history, philosophy, social studies, etc. of science (i.e., science studies). To that extent, it is a theoretical position on how to approach the sciences when constructing narratives about them. I construct an argument, utilizing the work of scholars such as Joseph Rouse, Hans-Jörg Rheinberger, Richard Burian, Richard Lewontin, Hayden White, Joseph Pitt, and others, that (among other things) rejects the idea of a definitive history; embraces the idea that the epistemic and the political (in the sense of Foucault's power/knowledge) are intertwined; embraces the notion that the norms at work in science (or culture, in general) are futural in the sense that any attempt to fully conceptualize them must wait for future developments (so that attempts to base scientific developments on so-called "background knowledge" will fail to adequately account for novel developments); argues that attempts to account for the past involve recurrence (looking back), and that to recur means to reinterpret and hence "distort" (to some extent) the past "wie es eigentlich gewesen" for purposes relevant to present norms (as Nietzsche told us); questions the current prevalent assumption (among mainly philosophers of science, but also some scientists) that epistemic sovereignty—the idea that a starting point for evaluating the sciences should be that their superior epistemic status is what makes them successful—is an appropriate way to describe and analyze the place of the sciences as cultural practices; I think that it is not.

In chapter 6 of my dissertation, I provide a narrative on geneticists' participation in the efforts in the 1950s to establish radiation exposure guidelines, including the Genetics Committee of the National Academy of Sciences study on the Biological Effects of Atomic Radiation, on which many prominent geneticists served, including Dobzhansky, Muller, Crow, Demerec, and Beadle. Also on this committee were two AEC officials, both non-geneticists, namely Gioacchino Failla, a Columbia University biophysicist, and Shields

Warren, a Harvard pathologist (Hollaender was another, but he seemed level-headed). Failla and Warren were two of the AEC's main spokesmen on the biological effects of fallout (with Merrill Eisenbud and Willard Libby), and they consistently argued that the biological effects of fallout from atomic testing were negligible, including the genetic effects. Included in their arguments were at times references to the notion that a small increase in the mutation rate from fallout might be beneficial to the human race, as it would provide mutations that natural selection could operate on and improve fitness. These references were clearly taken from the ongoing classical/balance controversy in population genetics, and specific reference was often made to Bruce Wallace's experiments on heterosis, which Dobzhansky said at the time was the only relevant work done on the problem of the genetic effects of radiation (and hence his plea for Wallace to be included on the Committee, which was rejected).

While Dobzhansky and Wallace both publicly rejected the idea that an increase in mutation rate from fallout might benefit humans, at the same time they both seemed quite committed to heterosis as an interpretation of the experimental results, which others described as interesting but needing more confirmation and as possibly having problems with the design of the experiments (Demerec, for example). While quite a bit has been written on this episode, there are some questions, which I have formulated from my historical research, but which also derive from your (Lewontin's) views on the episode and on Dobzhansky's work, for which answers or further interpretation would be historically and philosophically interesting.

1. In the interview by Paul, Beatty, and Krimbas, you suggest that Dobzhansky and Wallace bent their interpretation of experimental results to get money out of the AEC; you said you found out about this later. Can you be more specific about this? That is, how did you find out about this, and in what sense do you think they bent their interpretation? Wallace wrote to Curt Stern in 1958 stating that one of his experimental questions was "whether one could obtain evidence for selection to radiation resistance in *Drosophila*", that is, "to test the effect I had in mind, namely, genetic resistance to radiation." Do you think that Dobzhansky and Wallace tailored their interpretations of certain experiments to make them attractive to the AEC in that they might be used to infer that selection could result in resistance to radiation? Was this common at this time?

2. Dobzhansky tried to get Wallace on the NAS genetics committee in 1957, but even Demerec did not think it was a good idea (the official reasons given were his experiments were AEC-sponsored and that there would be too many people from Cold Spring Harbor). Do you think the main reason had really to do with the perceived sense that that Wallace (who had AEC funding since 1949) was bending his interpretation to get funding with the AEC, or was it because of the perhaps obvious political implications of taking the balance or homeostasis views (which Wright was arguing on the committee) too seriously in a report that would get much public attention? In addition, when Wallace was being considered for nomination to the National Academy of Sciences in 1962, Crow refused to send the nomination forward, citing problems with Wallace's experiments, and stating that the nomination would probably fail anyway. Do you think this episode had anything to do with the issue of tailoring research to fit the funding?
  
3. You have expressed strong feelings and standards on how some researchers have inappropriately interpreted their data, including Dobzhansky. You have also criticized Dobzhansky's research as being designed to demonstrate selection, rather than to test for it (i.e., that it was presupposed rather than the subject of the experiment); and you suggest he designed many of the experiments for his students. You harshly criticized Ayala's research abilities when he was recommended for a position at SUNY at Stony Brook in 1977, stating that he was "intellectually dishonest" and implying he had essentially come to conclusions that were not based on the data, but instead tried to make the data fit his interpretations. In my investigations, I am trying to say something about this idea of making the data fit preconceived interpretations, both at the scientific level and at the policy level (and in terms of historical interpretations). In 1962, the Executive Director of the Federal Radiation Council (the organization, composed of Eisenhower's cabinet members, that took over radiation protection responsibilities in 1959 after criticism of the AEC put too much pressure on the Administration), Paul Tompkins, wrote to an AEC commissioner proposing the following policy strategy:

If any reasonable agreement on this subject [of radiation standards] can be reached among the Agencies, the basic approach to the [FRC] report would be to start with a simple, straightforward statement of conclusions. It would then be a straightforward matter to select the key scientific consultants whose opinions should be sought in order to substantiate the validity of the conclusions or recommend appropriate modifications.

What do you think we can say about the notion of making the data fit the already-held conclusions? This goes against the prevalent view of "the scientific method" that holds that the data come first, and then the conclusions based on the data. A similar notion holds for expert-based policy-making: the policy recommendations should be based on the best scientific evidence, and preconceived political positions (i.e., Bush, the FDA and RU-486, or global warming) should not drive policy, for this would

amount to a sort of political interference. But if the epistemic and the political cannot be separated in practice, on what do we base prescriptions regarding making data fit preconceived conclusions?

4. Dobzhansky wrote to you in 1963 and stated that “there has not been a case when anything that I said, wrote, or done met with your approval.” This was in the context of the “Kimura-Li controversy”; Dobzhansky accused you of blaming him for this controversy and for considering the “Wisconsin-Indiana line so sacred that it should not even be questioned.” Did Dobzhansky hold this view of you throughout his life? Also, what exactly was the Kimura-Li controversy, and how do you figure into that?
5. Why, ultimately, do you think Dobzhansky accepted heterosis as an adequate interpretation of the genetic structure of populations? Didn’t this to some extent go against his unwavering commitment to selection as an explanation for everything? Since heterosis as an interpretation of various experimental results was so underdetermined, what can we say about his conversion to heterosis?

## VITA

Michael William Seltzer graduated second in his class from Wall High School, Wall Township, New Jersey in 1980. After studying chemical engineering and physics at Bucknell University from 1980 to 1985, he completed his undergraduate degree at Rutgers University in 1988, receiving a B.A. in Physics, magna cum laude, with a minor in Science, Technology, and Society. After taking graduate classes at Rutgers University and the University of Oklahoma, he entered the Science and Technology Studies program at Virginia Tech in 1990, where he taught Knowledge and Reality, and Reason and Revolution in Science for the Department of Philosophy. In 1993, Michael received an M.S. in Science and Technology Studies, and his thesis, "Atomic Testing and Population Genetics: The AEC and the Classical/Balance Controversy, 1946-1957," won the Graduate Research Award from the Virginia Tech Chapter of Sigma Xi, The Scientific Research Society; he was elected as an associate member in 1995. In 1996, he accepted a temporary position at the Lyman Briggs School at Michigan State University, where he taught Introduction to Science and Technology Studies, Philosophy of Technology, and Technology and Culture, until 1999. From 1999 to 2003, Michael temporarily stopped work on his dissertation to concentrate on his children, for whom he engaged in a prolonged divorce and custody dispute. Upon the successful resolution of that dispute, he began working again on his dissertation, the majority of which was written from 2004 to 2006. Michael married Barbara Jean Forcier on 16 August 2003 at St. Katherine's Episcopal Church in Williamston, Michigan. They live with their three children, Gabi, Dieter, and Paul, and two cats, Scotches and Whiskey.