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# Foragers, Genuine or Spurious?

1990

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## Situating the Kalahari San in History<sup>1</sup>

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Recent studies of societies hitherto portrayed as autonomous and self-regulating have sought to re-situate them in the context of wider regional and international economies, politics, and histories. In this revisionism there is danger of imputing links where none existed and assuming that evidence for trade implies the surrender of autonomy. Examination of the different historical experiences of two San groups, one largely dependent on its Bantu-speaking neighbours and the other (until recently) substantially autonomous, suggests that contact may take many forms, not all of which lead to dependency, abandonment of foraging, or incorporation into "more powerful" social formations.

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The present paper was submitted in final form 23 VIII 89.

1. Solway wishes to acknowledge the generosity of the Social Sciences and Humanities Research Council of Canada for a post-doctoral fellowship for support during the writing of this article. Earlier versions of it were presented at the annual meetings of the American Anthropological Association (1981) and the Canadian Anthropology Society (1989) and at the Universities of Massachusetts and Connecticut. We thank the participants in these meetings for their useful suggestions, which we have not always followed. We particularly want to thank Alan Barnard, Alec Campbell, Peter Carstens, Norman Chance, John Cole, Moitaly Dikgogwane, Christine Gaily, Rob Gordon, Bob Hitchcock, Tim Ingold, Art Keene, Sue Kent, Michael Lambek, Michael Levin, Ben Magubane, Toby Morantz, Shuichi Nagata, Tom Patterson, Peter Rigby, Larry Robins, Gerald Sider, Jeffery Tsheboagae, Diana Wylie, and John Yellen. Responsibility for errors and interpretations rests with us.

One of the dominant themes of critical anthropology in the 1970s and '80s has been the critique of ethnographic models that depict societies as isolated and timeless. Where an older generation of anthropologists tended to see societies as autonomous and self-regulating, the newer generation has discovered mercantilism and capitalism at work in societies hitherto portrayed as, if not pristine, then at least well beyond the reach of the "world system." Thus the Nuer (Gough 1971, Newcomer 1972, Sacks 1979, Kelly 1985), Samoans (Freeman 1983), Tallensi (Worsley 1956), Kachin (Friedman 1975, 1979; Nugent 1983), Maya (Lewis 1951, Wasserstrom 1982), and many other "classic" cases have been the subject of critical scrutiny. These studies have sought to re-situate these peoples in the context of wider regional and international economies, politics, and histories (see Wolf 1982).

Studies of hunting-and-gathering peoples have been strongly influenced by this revisionism (see, e.g., Endicott 1988; Woodburn 1988; Ingold, Riches, and Woodburn 1988; Headland and Reid 1989; Howell, cited in Lewin 1989; Bower 1989; Lewin 1989). It was in the spirit of this endeavor that we produced a critical analysis of the impact of the fur trade on the 19th-century Kalahari San (Solway and Lee 1981). A number of other scholars have focussed on the San, uncovering the early interactions between San foragers and Bantu farmers, herders, and traders within the complex historical dynamics of the Kalahari Desert (Schrire 1980, 1984a; Wilmsen 1983; Gordon 1984; Denbow 1984, 1986; Partridge 1984; Denbow and Wilmsen 1986).<sup>2</sup> In their zeal to discover links and to dispel myths of pristinity, however, these scholars are in danger of erecting new straw men and of doing violence of a different kind to the data—imputing links where none existed and assuming that where evidence exists for trade it implies the surrender of autonomy. What is perhaps most troubling about the Kalahari revisionism is its projection of a spurious uniformity on a vast and diverse region.

In this paper we present two case studies that demonstrate the varied nature and consequences of San contact with non-San in the Kalahari. By examining the different historical experiences of two San groups, one largely dependent on its Bantu-speaking neighbours and the other (until recently) substantially autonomous, we intend to make clear that contact may take many forms, not all of which lead to dependency, abandonment of foraging, or incorporation into "more powerful" social formations.

The attribution of dependency to societies formerly considered autonomous resonates with other themes in the culture of late capitalism. Borrowing an image from the popular film *The Gods Must Be Crazy*, we call this view the "Coke Bottle in the Kalahari Syndrome," whereby modernity falls mysteriously from the sky, setting in motion an inevitable spiral of cultural disintegration that can only be checked by the removal of the foreign element. This is clearly a caricature, but it re-

2. Lee (1965) had already noted the diversity of non-foraging adaptations among the contemporary San.

veals the common and unstated perception of foraging societies as so delicately balanced and fragile that they cannot accommodate innovation and change. Sahlins's (1968:2) summary law "Cultural dominance goes to technological pre-dominance" could be the foragers' epitaph. The "Coke Bottle in the Kalahari" imagery also bears a subtext, the rueful recognition of the unlimited capacity of "advanced societies" to consume everything in their path.

We challenge the notion that contact automatically undermines foragers and that contemporary foragers are to be understood only as degraded cultural residuals created through their marginality to more powerful systems. We consider the possibility that foragers can be autonomous without being isolated and engaged without being incorporated. And we follow Marx (1977 [1887]:89-92) in proposing that exchange can occur in the absence of "exchange value." Further, our argument calls into question any model of social change that implies linearity; the historical record reveals protracted processes, with fits and starts, plateaus and reversals, and varied outcomes. While many historical foragers have assimilated to other societies, a number, such as the African Pygmies and the foragers of South and South-east Asia, have developed stable forms of interaction with agricultural neighbours and persisted alongside them, sometimes for centuries (see, e.g., Leacock and Lee 1982, Endicott 1988, Petersen 1978). The fact that foragers have coexisted with farmers for so long is testimony to the resilience of their way of life. The position adopted here is that 20th-century foragers are neither pristine nor totally degraded and encapsulated. The historical status of African foraging peoples must be seen as the complex product of the dynamics of the foraging mode of production itself, of long interaction between foragers, farmers, and herders, and finally of dynamics growing out of their linkages with world capitalism.

## The Problem

By the mid-20th century, San societies in Botswana exhibited a wide range of "adaptations." Along the Nata, Botletli, and Okavango Rivers there were "black" San who fished, owned cattle, and practiced agriculture (Cashdan 1987, Tlou 1985, Hitchcock 1987); in the Ghanzi freehold zone of western Botswana many San had become farm labourers, dependent squatters on their traditional lands (Guenther 1985, Russell 1976); in the Game Reserve areas of Khutse and the Central Kalahari, the /Gwi and other San groups lived relatively independent lives, hunting and gathering, raising small stock, and gardening (Kent 1989a, Tanaka 1980, Silberbauer 1981); and in the central sandveld many San lived clustered around Tswana cattle posts, where the men were employed as herders (Hitchcock 1978).

The historical antecedents of this diversity have been difficult to discern. Until the 1970s the available ar-

chaeological evidence indicated that the Kalahari had been a stronghold of hunter-gatherer societies and the diversity was the product of the last few hundred years (Phillipson 1977). Recent excavations, however (Denbow 1980, 1984, 1986; Wilmsen 1983, 1988; Denbow and Wilmsen 1983, 1986), have demonstrated a much earlier Iron Age presence, in parts of the Kalahari as early as A.D. 500. Later Stone Age (LSA) sites, commonly associated with populations ancestral to San hunter-gatherers, are present as well and in some areas remain predominant, but a number of these sites have Iron Age materials indicating contact between farmers and foragers. Thus the time depth of contact with non-hunters has increased from a few centuries to a millennium or more, and the presence of "exotic" goods is evidence for regional trade between hunters and non-hunters.

A second line of evidence for the revisionists springs from rereadings of 19th-century accounts of exploration and trade in the Kalahari interior. Gordon (1984), for one, has argued that the interior San were so deeply involved in trade, warfare, and diplomacy that they bore little resemblance to the "autonomous" societies described by 20th-century ethnographers. A closely related issue is the question of San servitude for black overlords. Indeed, many 19th- and 20th-century sources describe the San as living in a condition close to serfdom, a perception that has coloured observations of them.

The revisionists have used these lines of evidence to call into question the claims to authenticity of a number of foraging peoples studied by Marshall (1976), Lee and DeVore (1976), Lee (1979), Silberbauer (1981), Tanaka (1980), and others. Schrire (1980, 1984b), for example, argues that the San are not hunter-gatherers at all but failed pastoralists who oscillate between herding and foraging from century to century.<sup>3</sup> Labelling recent ethnographies of the San "romantic accounts of Bushman isolation and independence," Denbow (1986:1) dismisses them as "an ahistorical and timeless caricature." He suggests that whatever hunters persisted through the long period of contact did so not as autonomous societies but as "part of long-standing regional systems of interaction and exchange involving neighboring peoples with quite different economic and sociopolitical orientations" (p. 27). Wilmsen (1983), the most outspoken critic, referencing the perspective pioneered by Wolf, challenges the idea that the flexible egalitarian sharing documented for several San groups has anything to do with the dynamics of a foraging mode of production, concluding that "it is more than merely possible that the San are classless today precisely because they are the underclass in an intrusive class structure" (p. 17). In the same vein, Schrire (1984b:18) asks,

Are the common features of hunter-gatherer groups, be they structural elements such as bilateral kinship

3. Schrire's model in turn is drawn from Elphick's (1977) studies of the 17th-century Cape San, who were observed to move into herding as opportunities arose and back to foraging when the livestock was lost or stolen.

systems or behavioral ones such as the tendency to share food, a product of interaction with us? Are the features we single out and study held in common, not so much because humanity shared the hunter-gatherer lifestyle for 99% of its time on earth, but because the hunter-gatherers of today, in searching for the compromises that would allow them to go on doing mainly that, have reached subliminal consensus in finding similar solutions to similar problems?

The questions raised by the revisionists are challenging ones, and the claims they make go well beyond the reinterpretation of Kalahari archaeology. Yet it is an open question how much of their revision arises from the data and how much rests on unexamined inference and assumption. It will be useful to set out their claims as a series of propositions in order to clarify the boundary between fact and interpretation. They propose that (1) the Iron Age settlement of the Kalahari is earlier than previously thought, and therefore (2) hunter-gatherers were absorbed into regional economic networks and (3) ceased to exist as independent societies well before the historic period. They go on to argue that (4) if these societies continue to exhibit characteristics associated with hunting and gathering it is because of (a) their poverty (Wilmsen) or (b) their resistance to domination by stronger societies (Schrire). Of these only Point 1 can be considered well established; Points 2 and 3 draw unwarranted conclusions from scanty data while Point 4 relies heavily on discourses that are as ideological as they are analytical.

What kinds of questions need to be asked in order to evaluate the conflicting claims of the Kalahari ethnographers and their critics? It is necessary, first, for both parties to attend to issues of regional variation. Some foragers certainly were drawn into farming and herding centuries ago, and some of these became part of regional economic systems, but, as we spell out below, both archaeology and ethnohistory contradict the view of a uniform grid of economic interdependency throughout the Kalahari. Second, we need to sensitize ourselves to the assumptions we make about the nature of "contact." For some "contact" appears to be unconsciously equated with "domination." The possibility of trade or exchange *without* some form of domination is excluded from the range of outcomes. When considering the Kalahari we need to ask further whether the conditions for domination existed there before, say, 1850. Were the societies with which the foragers came in contact after A.D. 500 sufficiently powerful to compel San servitude? Again the evidence shows that outcomes were variable and that in a number of areas the foraging life persisted. Third, and related, we need to examine our assumptions about the transformative power of the commodity—the view that when a society is linked to another by trade or tribute that linkage will necessarily transform social organization and create dependency. Are there other outcomes possible in which exchange relations do not undermine existing relations of production? Finally, we

need to assess the evidence for San servitude; the contradictions in the literature suggest that appearances may be deceiving and in some cases San subordination may be more apparent than real. We wonder whether the current vogue for projecting unequal tributary and mercantile power relations into the past and for debunking the "myth of the primitive isolate" has not created a climate of scholarly opinion in which contact with domination is accepted as the normative or inevitable condition—thus making it impossible to examine actual cases treating the impact of trade as problematic rather than as given. It seems prudent not to exclude a priori the possibility of societal and cultural autonomy.

## Case Studies

### THE WESTERN KWENENG SAN

Many San peoples today live on the fringes of Bantu communities or white-owned farms;<sup>4</sup> the Western Kweneng San are one example. In contrast to the Dobe San, whose contact with non-San has traditionally been intermittent, these Southern San have lived amongst Bantu-speaking peoples for at least 200 years. The peoples of the Duitwe area, in the southern Kalahari 250 km west of Gaborone (fig. 1), include three intermarrying San groups (Tshassi, Kwa, and Khute) and the Bantu-speaking Kgalagadi. The Kwena, a Tswana chiefdom, occupy the better-watered eastern edge of the desert.<sup>5</sup> The dominant Tswana-Kgalagadi cultural model posits a hierarchical social order in which the San and other servile peoples occupy the social and physical margins. This "Tswanacentric" model does not, however, fit everywhere with the same precision, nor has it fit equally through time. The historical record reveals a variety of linkages between San and their neighbours, with a variety of consequences. San encapsulation within the orbit of Bantu-speaking peoples and loss of autonomy have been neither automatic nor, in most instances, complete. The San of Western Kweneng have not always worked for their Bantu neighbours, nor, in spite of the pronouncements of current Kalahari residents, is there

4. For some the fringes are more social than physical, and San live, as servants, in Tswana, Kgalagadi, or Herero households. At the same time, many San live literally on the outskirts of Bantu communities or, in the case of the Ghanzi district, of white-owned farms and oscillate between client-like relations with their Bantu employers, hunting and gathering, and stock raising and agriculture. Such arrangements are described for much of Botswana (see, e.g., Silberbauer and Kuper 1966, Guenther 1985, Solway 1987, Vierich 1982, Hitchcock 1987, Kent 1989, Bieseke et al. 1989). In the densely settled eastern regions of Botswana, San tend to be tightly linked to Tswana communities and have little opportunity to hunt and gather or to claim a "hinterland" area to which to retreat (Motzafi 1986). In the southern and central Kalahari, where settlement is relatively sparse, San generally maintain more options and greater cultural integrity and economic diversity.

5. The southern Kalahari, drier than the north, has no year-round standing water, but the pans that dot the desert have high water tables and hold water, often salty, for varying periods of time after the rains.

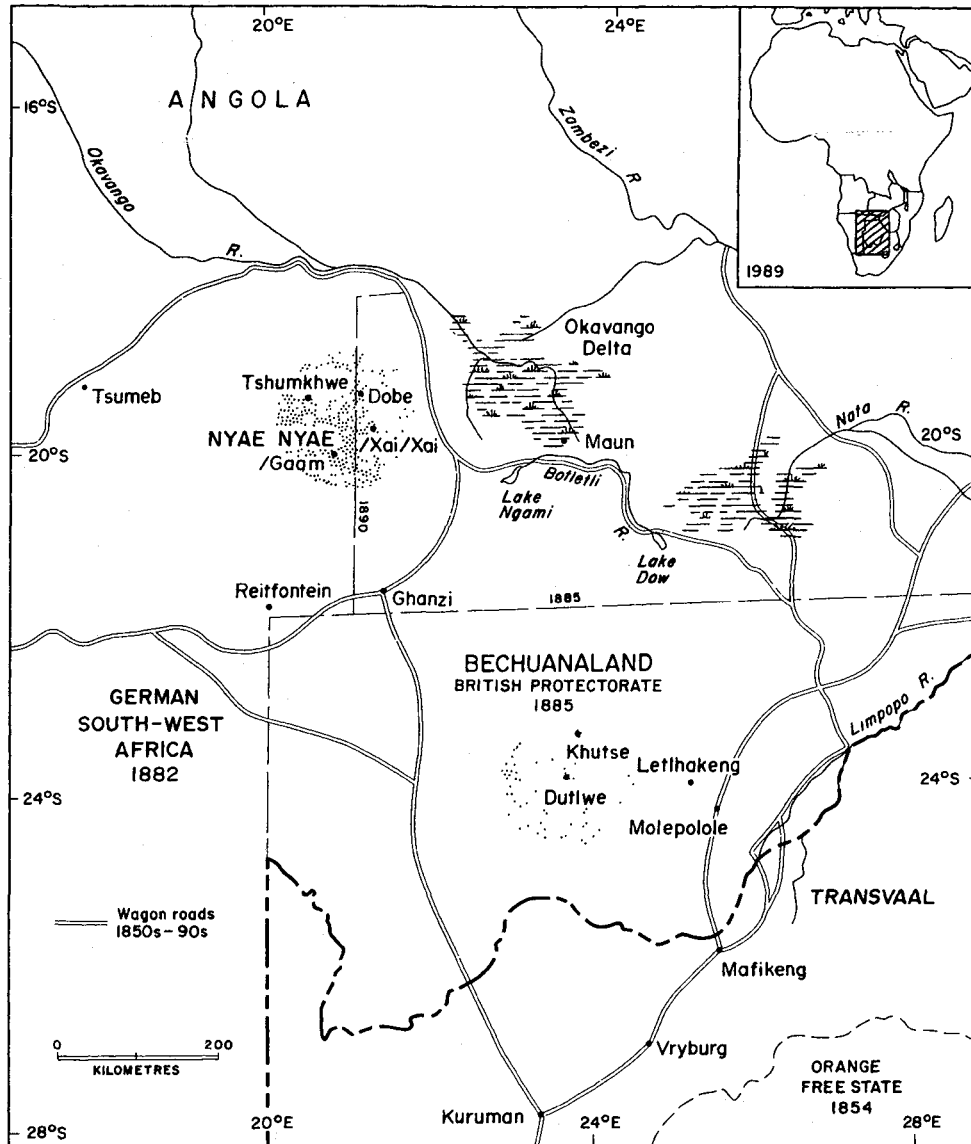


FIG. 1. The 19th-century Kalahari, with relevant contemporary boundaries and political divisions superimposed.

anything "natural" about the state of affairs that exists today.

*The pre- and protohistoric period.* Oral traditions obtained from current residents indicate that relations between Kgalagadi and San were largely symbiotic in the early period.<sup>6</sup> All were nomadic and lived primarily by hunting and gathering, although the Kgalagadi may have

practiced some horticulture. After 1820 new waves of Kgalagadi, refugees of the wars of the turbulent period known as the Difaqane, retreated into the desert with their goats, sheep, and dogs. The Kgalagadi credit the San with having taught them desert skills, and the San made use of Kgalagadi animals, especially hunting dogs. According to the Kgalagadi, their ancestors were able to migrate to western Kweneng with their goats and sheep in the early 19th century because the animals could obtain virtually all of their moisture from melons during the trek. These new immigrants chose a more sedentary life than their predecessors, and the pans on which they settled were also San water sources. In a Mokgalagadi's words, "The Basarwa [San] were already here. They just move around a lot. . . . They were not driven away."

*The fur-trade period.* In the period following 1840 the

6. Ethnohistorical data on the western Kweneng for the 19th century are limited. Maps in Livingstone (1857), MacKenzie (1971 [1871]), and Chapman (1971 [1868]) exclude the area, and Leistner (1967:30) notes that the mid-19th-century explorers avoided the "inhospitable southern wastes." Oral traditions collected in the field by Solway in 1977-79 and 1986 (Solway 1987) and by Okihiro (1976) contain few specific data on transactions between the ethnic groups in the 19th century. We know of no archaeological work in the region.

Kwena, who themselves had fared badly during the Difaqane (Thompson 1975:396), were attempting to reassert and consolidate their hold on the Kalahari periphery. Threatened from the east by the Boers, they were eager to accumulate Western trade goods, particularly guns (Livingstone 1857:39).<sup>7</sup> To do so they needed desert products such as furs, ostrich feathers, skins, and ivory, and vast quantities of these were obtained from the peoples of the area as tribute; Livingstone writes (p. 50) that while he was living among the Kwena he observed "between twenty and thirty thousand skins . . . made up into karosses; part of them were worn by the inhabitants and part sold to traders."

The San participated only indirectly in the tribute system; they and the Kgalagadi were the primary producers, hunting and preparing skins, but in most cases it was the Kgalagadi (and usually only the elite among them) who dealt with the Kwena.<sup>8</sup> The San hunted with dogs and occasionally with guns owned by others; they brought the hides and often some of the meat to the owners and kept a portion of the meat for themselves (see, e.g., Silberbauer and Kuper 1966, Hitchcock 1987, Schapera and van der Merwe 1945, Stow 1964 [1905]). Tobacco, grown and/or obtained by trade, was a central commodity in the system, exchanged for skins and labour. Contact between Kgalagadi and San was concentrated in the winter months, when the fur-bearing animals were most desirable and water most scarce. In this period there was little difference in the objective conditions of life of San and Kgalagadi. Their relations were less coercive than Kwena-Kgalagadi relations and resembled trade more than tribute.

Towards the end of the 19th century the Kwena's control over the periphery began to break down. The desert was difficult to police; Kwena rule was thin and maintained largely through periodic displays of force. The Kgalagadi as a result were able to begin to accumulate property, especially cattle (see Okihiro 1976; Schapera and van der Merwe 1945:5), thus laying the groundwork for an agro-pastoral base that did not develop among the Kweneng San. Inequalities between the San and some Kgalagadi also began to grow. The Kgalagadi attempted to replicate in their relations with the San the Tswana hierarchical model that subordinated them to the Kwena, but the material conditions for institutionalized servitude were absent.

In 1885, with the imposition of British colonial rule, the tribute system was officially disbanded; the Kgalagadi were allowed to trade their goods, and instead of

tribute a tax, of which Kwena chiefs received 10%, was collected (although in practice the transition from tribute to tax was not automatic) (Schapera and van der Merwe 1945:6). The colonial state was intrigued by the San and voiced concern over their condition, but in fact the new government had little direct impact on their life.

A colonial officer travelling through western Kweneng in 1887 considered the San the Kgalagadi's "slaves pure and simple," but at the same time he reported, "They have no fixed residence, often living miles from water and living on the melons and roots, changing their abode, as these are scarce or plentiful" (Botswana National Archives 1887a:17). (If the San had truly been slaves they would not have been following the melons but would have been working for the Kgalagadi.) This apparent contradiction emerges repeatedly; the San are described as slaves and yet as "scoundrels, snakes, and rascals" who will not stay in one place and move about as they wish (Botswana National Archives 1887b; cf. MacKenzie 1871:128-32 for the Central district). Again, an 1899 report states that the Masarwa (San) "lives a nomadic life in a wild state and hunts for the masters" (HC. 24, quoted in Schapera and van der Merwe 1945:4), thus portraying them as simultaneously enserfed and nomadic foragers.<sup>9</sup>

In part this contradiction may reflect European observers' response to the contempt in which the San were held by the eastern tribes. The San's disregard for the pastoral values of preservation and accumulation, their nomadism, and their lack of a hereditary chiefly line went against the grain of Tswana society, inverted the natural order, and were, in certain respects, incomprehensible and frightening to the Bantu-speaking peoples and many Europeans. In speaking of the San some European observers romanticised them for their free-spiritedness and lack of material desires; others, heeding the Tswana standards of a proper life, more resonant with their own, looked upon them as candidates for civilizing. According to the latter and predominant view, the San were the victims in southern Africa, and their Bantu-speaking neighbours bore some responsibility for that state of affairs.<sup>10</sup> It is also possible that administrators were told exaggerated stories and lacked the breadth of experience that would have placed these in a more realistic light. Lack of experience might also

7. In 1852 the Boers attacked the Kwena capital and killed 60 Africans. Livingstone (1857:121), whose home was destroyed in this raid, notes that African refugees from the Boers came to Kwena afterward, buttressing their power. In terms of trade, the Kwena capital remained the center and launch point to the north until the 1860s (Parsons 1977:119).

8. Like the San of the Central district described by Hitchcock (1987:234), the Dutlwe-area San may well have engaged directly in trade with Europeans, but there is little evidence to support this view. Few traders ventured into western Kweneng.

9. Russell's (1976) analysis of Ghanzi San subordination contains similar contradictions. It is reported that "in the Bushman view [the Boers' arrival] presented an alternative to Tswana overlordship" (p. 189), yet a 1910 Boer petition is quoted as requesting that "the Bushmen be placed in a location . . . for being in such a wild state they are very little use as servants." Motzafi (1986) notes that in the Tswana world view the San's "wild" qualities emerge as much from their lack of social standing as from their association with the bush. Thus, for the Tswana, "wildness" and servility are not necessarily incompatible. It is doubtful that this cultural understanding can be attributed to the European view.

10. There was a strong ideological component to this; by showing how Africans could mistreat other Africans the Europeans could feel less culpable about their own treatment of the Africans.

have made it difficult for them to distinguish between the claims of Bantu overlords to their "serfs" and the degree of real servitude in the Kalahari. The evidence does seem to indicate that some San were subservient to some Bantu, but we argue that the institutionalization of San subordination came later.

*Agro-pastoralism.* The fur trade remained for some time the primary link between San and Kgalagadi. The Kgalagadi elite who owned cattle in the early 20th century relied upon their poorer relatives rather than San for herding labour. At the same time, the development of agro-pastoral production was beginning to undermine the San's foraging base. Permanent settlement, population increase, cattle herding, and agriculture combined to reduce the environment's hunting-and-gathering potential. The desertification noted by elderly residents and by ecologists alike can be traced not simply to over-hunting but to human habitation (Campbell and Child 1971, Leistner 1967). Every bush or tree cleared to make way for cultivation, especially plowing, reduces the ground cover, disrupts root systems, facilitates erosion, and reduces the soil's ability to absorb and retain moisture. It was increasingly only in the bush, away from the better water sources, that the San could maintain their autonomy. The Central Kalahari has remained (by law) free of large-scale village and livestock development and served as a "hinterland" for the San, a place where their culture and mode of subsistence have persisted and where many Western Kweneng San claim roots, refuge, and restoration. Many of the elder San now living in western Kweneng were born in the bush, had a youth of mostly hunting and gathering, and only came to spend most of their time in the village later, in their own words because of "fear of lions and thirst." The integrity of the San's own flexible social organization and communal property arrangements permitted fluidity between village and bush. The existence of the hinterland, in spite of its diminished productivity and diminished appeal for some San, provided them with a hedge against complete subordination.

*The organic link.* By the 1940s, local agro-pastoralism was well established. With trading revenues and migrant labourers' wages, the Kgalagadi accumulated cattle and plows and imported new well-digging techniques that permitted expansion of the livestock sector. Cultivated water sources such as wells and boreholes came to be considered the private property of the group that dug them,<sup>11</sup> and eventually many of the better-watered pans (which probably had been dry-season homes of the San [Vierich 1977]) were associated with the Kgalagadi; now to obtain drinking water the San had to enter into un-

equal relations with the Kgalagadi. Plow agriculture and animal husbandry increased the workload at precisely the time when able-bodied young Kgalagadi men were leaving for contract work on the South African mines, and it was San labour that filled the gap (Solway 1987). By the 1950s the San had become the Kgalagadi's casual labour force. The Kgalagadi today frequently try to minimize the importance of San labour and like to think of themselves as humanitarian for "helping" them, but when pressed many will quietly admit, "We are lucky, we have Bushmen."

That the Kgalagadi's greater demand for labour occurred in concert with the growing precariousness of foraging in the area was not a result of conscious conspiracy, but neither was it a coincidence. The Kgalagadi's new productive base altered the environment; it changed their labour demands, transformed property relations over water sources, and increasingly distinguished the Kgalagadi from the San. In the 19th century differences in material conditions between the groups were small, but by the mid-20th century the hierarchical model in which the San occupy a marginal and servile position more closely matched reality than it had in the past. Hunting for the Kgalagadi had not undermined the San's foraging subsistence strategy; it is doubtful whether the Kgalagadi of the 19th century had the resources or power to compel San servitude, except in the very short term. The Kgalagadi of the 20th century, in contrast, had control of water, milk, grain, and purchased items such as tobacco, clothing, guns, and wagons, and these resources, in the face of diminishing returns from foraging, tied the San to them more thoroughly than in the past. New kinds of work that followed the rhythm of the agricultural and livestock cycle resulted in more intimate and regular association than that created by the hunting arrangements. With the expansion of Kgalagadi agriculture, San women entered the workforce in greater numbers, which meant that San social reproduction increasingly took place in the Kgalagadi's domain. Today, a few San live permanently as domestic servants with Kgalagadi; the Kgalagadi claim to "take these San as our children," but they are children who never achieve adult status. There are a number of San homesteads on the periphery of the villages, their populations waxing and waning with the seasons. The spatial marginality neatly reflects the San's social marginality and positioning somewhere between village and bush.

Although the hinterland persists and some San forage full-time in it (Kent 1989, Silberbauer 1981), most Western Kweneng San work for the Kgalagadi at least during the agricultural season, arriving "after the flowers appear on the melon plants." Sixty years ago, coming to the village and working for the Kgalagadi was seen as a "break" from foraging in an increasingly unproductive environment. Now the village end of the cycle has taken precedence, and most San are resigned to the fact that they can make a living only by working for the Kgalagadi, begging, or accepting government aid. Forag-

11. Peters (1983, 1984) presents a structurally similar case in which common property (pasture) surrounding private property (borehole) becomes identified with the private and treated as such. Again, for the Ghanzi area it is reported that "Boer skill at digging wells gave them an economic advantage over Bushmen for the first time. The further decline of the water table necessitated the more complex technology and capitalization of borehole and pump, and Bushman dependence accelerated" (Russell 1976:190).

ing offers only an occasional supplement. Some San still return to the bush in the wet season. According to one woman, "We are happy to be away from the Kgalagadi. There are water roots and berries. If we come upon a tortoise or a dead animal we will eat and dance all night. We only come back because of thirst."

#### THE DOBE SAN

The Dobe area, 700 km north of Duitwe, was far from the turmoil of 19th-century colonial southern Africa.<sup>12</sup> The Dobe people were not affected by the Difaqane, though they had heard about it, and they were not subject to tribute. More important, the wave of black settlement did not reach them until 1925. Surrounded by a waterless belt 70–200 km in depth, the Dobe area is difficult of access even today; it would have been accessible to Iron Age peoples with livestock for only a few months in years of high rainfall, and even then only after an arduous journey. It would be risky to assume that contemporary patterns of contact (or lack of contact) were characteristic of all periods of prehistory. Fortunately, the data of archaeology can be brought to bear on this kind of question.

*The pre- and protohistoric period.* Despite the abundant evidence of Iron Age settlement elsewhere in northwestern Botswana dating from A.D. 500 or earlier and despite concerted efforts to find the same in the Dobe area, there is *no archaeological evidence of Iron Age occupation of the area until the 20th century*

12. Although the !Kung San of the Dobe-Nyae/Nyae area are arguably the most thoroughly documented hunting-and-gathering society in this century, they are markedly underrepresented in the historical literature. Tabler's (1973) definitive compendium contains only 10 references (out of 334) to Europeans who entered the area prior to 1900. Thus the classic accounts of Baines (1973 [1864]), Chapman (1971 [1868]), Galton (1889), Anderson (1856), and Livingstone (1857) refer only elliptically to the peoples of the central !Kung interior. The earliest firsthand account, that of Passarge, dates from 1907, while Wilhelm's observations from the period 1914–19 were not published until 1954.

In his ethnohistorical examination of !Kung exchange, Gordon (1984) uncritically conflates accounts from all over northern Namibia, distorting the picture of 19th-century !Kung San by portraying a number of highly acculturated and distant San peoples as if they were San of the central !Kung interior; less than 20% of his material refers to the !Kung of the Dobe-Nyae/Nyae area. For example, he refers to a group of San controlling a rich copper mine near Tsumeb and marketing 50–60 tons of ore each year as if they were !Kung (pp. 212–13), but the San in question are the Nama-speaking Heikum and Tsumeb is located 400 km west of Dobe.

Sources of data for this case study consist of extensive interviews with San, Tswana, and Herero informants between 40 and 80 years of age, mainly during three years of fieldwork between 1963 and 1969. A number of the older informants had been alive at the turn of the century. All informants drew upon older oral histories and oral traditions. Individual accounts were checked for consistency against the growing corpus of material, and follow-up visits were made to resolve discrepancies wherever possible. These accounts were placed in the context of the growing historical literature for the region, including Tlou (1985), Tlou and Campbell (1984), Vedder (1966 [1938]), Drechsler (1980), Parsons (1982), Palmer and Parsons (1977), and Clarence-Smith (1979).

(Brooks 1989, Yellen and Brooks n.d.). What does exist in Later Stone Age archaeological deposits, along with a classic stone tool kit, is a few fragments of pottery and a few iron implements, items best interpreted as evidence of intermittent trade with Iron Age settlements to the east and north.

!Kung oral traditions reinforce this view. Elders speak of their ancestors' maintaining long-term trade relations with "Goba" while maintaining their territorial organization and subsistence as hunter-gatherers in the Dobe area and to the west of it. Some have gone so far as to insist that the first visitors on a large scale to their area were whites rather than blacks. According to !Xamn!a, who was born at the turn of the century, "The first outsiders to come to /Xai/Xai were /Ton [European] hunters. . . . They used to shoot guns with bullets one and one-half inches thick. But this was before I was born. My wife's father, Toma!gain, worked for the /Tons." When asked which of the Tswana ruling clans had first arrived in the Dobe area in the last century, a !Goshe elder emphatically replied, "None! The /Tons [Europeans] were first." And when asked if his "fathers" knew of blacks of any origin in the area, he replied, "No, we only knew ourselves."

The picture that emerges from the archaeological, ethnohistorical, and oral-historical evidence can be sketched as follows: The Dobe area has been occupied by hunting-and-gathering peoples for at least several thousand years. The evidence of unbroken LSA deposits 100 cm or more in depth, with ostrich eggshells and indigenous fauna from bottom to top, with a scattering of pottery and iron, and with European goods in surface levels supports a picture of relative continuity.<sup>13</sup> At some point between A.D. 500 and 1500, the interior !Kung established trade relations with "Goba" to the east and northeast and carried on trade with them in which desert products—furs, honey, and ivory—were exchanged for iron, tobacco, ceramics, and possibly agricultural products. It is unclear whether the Goba made reciprocal visits to the Dobe area or even whether the ceramics that are found are of outside origin.<sup>14</sup>

Thus, on the eve of the European colonial incursions, the !Kung were evidently occupying the interior on their own as hunter-gatherers and producing a small surplus of furs and other desert products for barter with agriculturalists on the western margins of the Okavango swamps. The few accounts from the precolonial era that do refer to the !Kung of the interior—called KauKau or MaKowkow—treat them with respect as a fierce and

13. For example, at Nxai Nxai, excavated by Wilmsen (1978), Levels 6–10 (60–100 cm) produced 4 sherds, Levels 1–5 (10–50 cm) 32, and the surface levels 348. Fragments of a single bovine maxilla found in the 60-cm level were identified as domesticated cow. That no further examples of domesticates have been found has led some archaeologists to suggest that this specimen was intrusive (Yellen and Brooks n.d.).

14. Some !Kung insisted that their ancestors had made the pottery from local clays. With the advent of European iron pots, the art was lost.



independent people (a reputation that has persisted to the present among neighbouring blacks). Chapman (1971 [1868], vol. 1:165), travelling through the Ghanzi area in 1855, had this to say:<sup>15</sup>

The inhabitants of these parts are a much finer race of Bushmen than we had generally met with. Freedom, and the enjoyment of their own game for food and the skins for clothing, are the main causes. They acknowledge no chief and are in the habit of defending themselves against oppressors and intruders, either from Lake Ngami or the Namaqua region; in former times they have often combined to resist marauding parties sent out by the Batwana and other tribes. Their minds are free from apprehension of human plunderers, and the life they lead is a comparatively fearless one. The population is numerous, and they are more attached to each other than in other parts.

*The fur-trade period.* Two kinds of economic networks were involved in the San articulation with the "world system": indirect involvement through black intermediaries—the Goba and later the Tswana—and direct contact with European hunters and traders. The indirect form resembled the precolonial African trade that the San had carried on for centuries and therefore involved no basic restructuring of relations of production. The direct European trade, while intense and disruptive, did not last very long. It was not until the 1920s and '30s, with the arrival of black settlers in the Dobe area, that basic production relations began to be modified and incorporative processes set in motion.

Several accounts exist of the lively trade that went on in the "Gaamveld" between the "Bushman" and Afrikaner, German, and English hunter-traders in the period 1870–90 (Lee 1979:78; Solway and Lee 1981). The first European known to have visited the Dobe-Nyae/Nyae area was Hendrik van Zyl, whom Ramadjagote Harry, a Tswana born in 1903, describes as "the hunter who was responsible for killing all the elephants and rhinos in the west." Tabler (1973:114) confirms that in 1877 alone van Zyl's party killed 400 elephants in the Gaamveld and took out 8,000 lb. of ivory. !Kung recall the period with a great deal of affection as a time of intense social activity and economic prosperity. They were provided with guns and ate enormous quantities of meat. One could find no trace of regret in these accounts for the carnage and diminution of wildlife; elephants, regarded as pests by the !Kung, are rarely hunted today. The legacy of this brief but intense irruption for the Dobe-area people can be briefly set out. One small family of !Kung, fully integrated into the Dobe community, is acknowledged to be descended from a member of van Zyl's party and a local !Kung woman. Few other impacts

15. As this passage indicates, the !Kung's willingness to defend themselves may be involved in the persistence of their autonomy. Chapman's reference to their fierceness is echoed by many of the !Kung's non-San neighbours today. The Herero see them as unpredictable and prone to flare-ups, an image at odds with some anthropological views of !Kung temperament (Lee 1979:370–400).

are evident. Even though firearms were widely distributed to African populations (Marks and Atmore 1971) and though many 19th-century-vintage weapons remained in African hands into the 1960s, only a single !Kung man, a tribal constable who had purchased his weapon with wages, possessed a gun in 1963.

A second instance of European presence, also short-lived, was the cattle drives sent by a group of Afrikaner trekkers from Angola to the Transvaal via Lewisfontein (!Kangwa), a large perennial spring in the centre of the Dobe area. The "Dorsland" Trekkers reached Angola only in 1880, and according to Clarence-Smith (1979: 59–60) the trek route had fallen into disuse by 1900 (see also Gordon 1984:202).

Since most European goods—iron pots, beads, etc.—continued to be obtained through Bantu intermediaries, one would be hard put to argue that the sporadic European presence from 1870 to 1900 had transformed !Kung society. On the other hand, it is likely that the European penetration of the !Kung interior was the catalyst for incursions by Tswana and others.

!Kung call the period after the departure of the Europeans and before the arrival of permanent black settlers *koloi* (wagon), a reference to the ox-carts used by the Tswana from the 1880s to about 1925. A number of Tswana had been employed on the European hunting parties as hunters, trackers, and gun bearers. After 1880 Tswana hunter-traders with wagons began making their own trips to the Dobe area; this was part of the general expansion of the Tswana state after 1874 (Tlou 1985:49). In the !Kung oral traditions it is the !Kung and not the Tswana who are the initiators of this trade. As !Xammla tells it,

When the Europeans left, the Zhu/twasi were all alone. My *≠tum* [father-in-law] said, "Let's go to the Tswana, bring their cattle here and drink their milk." So then my *≠tum* organized the younger men and went east to collect the cattle. . . . Then they chopped a brush-fence kraal under the camel thorn trees and kraaled them there. The Tswana came up to visit and hunt, then they went back leaving the San drinking the milk. Then my *≠tum* got *shoro* [tobacco] from the Tswana and smoked it. When the *shoro* was all finished the young men collected all the steenbok skins and went east to bring back more *shoro*. The boys shouldered the tobacco and brought it back. Later they drove the cattle out to Hxore Pan where they built a kraal and ate the *tsin* beans of Hxore while the cattle drank the water. So they lived, eating *tsin*, hunting steenbok and duiker, and drinking milk. When Hxore water was dry, they loaded the pack oxen with sacks of *tsin* [for the !Kung to eat] and drove them back to /Xai/Xai. At the end of the season the cattle boys loaded the pack oxen with bales and bales of eland biltong and went east with it to collect the balls of *shoro* and sometimes bags of corn. These they would deliver to my *≠tum*.

This account provides a good description of two forms of economic linkage: the barter system, in which desert



products are exchanged for agricultural and manufactured products, and the *mafisa* system, whereby well-to-do Tswana farm out cattle to others—fellow tribesmen or members of subordinate groups. The first form of linkage does not lead to incorporation and loss of autonomy, especially when the level of trade is modest and the element of coercion is absent. *Mafisa*, by contrast, does alter the character of production at the levels of both forces and relations. Animal husbandry places foragers in a different relation to land and to predators and necessitates a shift in the patterns of labour deployment. Energy is drawn away from hunting and reallocated to herding, and in return the producers are rewarded with a more secure food source, at least in the short run. At the level of production relations, *mafisa* is a form of loan-cattle-labour exchange set in the context of a patron-client relationship.

Briefly, the *mafisa* system in northwestern Ngamiland operated as follows (see also Tlou 1985:52): The San client maintained the herd on behalf of the Tswana patron, who retained ownership of the beasts. In return San could consume all the milk the herd produced and the meat of any animal that had died of natural causes, including predation. A tally was kept of beasts lost, and all animals had to be accounted for when the patron made a periodic visit. If he was satisfied with the performance of the *mafisa* holder he might pay him a calf, but this was not obligatory. If he was not satisfied he could withdraw his animals and seek another client. Similarly, the client was free to withdraw his services—with notice—and either leave *mafisa* entirely or seek another patron and a new herd of cattle.

On the face of it, *mafisa* appears to resemble a system of agrarian dependency: ownership of the means of production, in this case cattle, is in the hands of the overlord, who at his whim can withdraw the herd and thus deprive the client of his livelihood. Clients therefore existed, it would seem, in a highly vulnerable state of dependency. Only a minority of Dobe-area people became involved in *mafisa*, however, and families with cattle retained links with families fully immersed in hunting and gathering, which remained viable as an alternative economic strategy throughout the *koloi* period and beyond. Had *mafisa* been the only means of subsistence for the people of the Dobe area, then the withdrawal of the cattle would have caused a crisis in subsistence and the threat of it would have been sufficient to produce a condition of virtual serfdom. But the *mafisa* families were not peasants; they were islands of pastoralism in a sea of hunting and gathering, with benefits flowing in both directions. When cattle were withdrawn, as they often were, the bush was there to fall back on, and that same bush beckoned as an alternative if the responsibilities of keeping cattle grew too onerous.

Thus we have to consider seriously the !Kung's view of *mafisa* as something that operated in their favour. Far from having the system forced upon them or being forced into it by circumstance, !Kung who entered into it did so voluntarily, for the opportunity it provided to supplement a foraging diet with milk and occasional beef.

Some of the men who went into *mafisa* did become "big men" of a sort, acting as brokers in transactions between San and black. But a large majority of !Kung remained hunter-gatherers and never relinquished their claims to foraging *n!ores*, the collectively owned hunting lands that were the foundations of their communal mode of production (see Lee 1979:333–69; 1981). In fact, many of the ranges where cattle were grazed were superimposed on these *n!ores*, and the herds were managed by members of the groups that held them. Thus the niche that had sustained the communal foraging mode of production was modified and expanded to encompass *mafisa* cattle husbandry without destroying the preexisting adaptation.<sup>16</sup>

*Agro-pastoralism.* Permanent settlement by non-San came late to the Dobe area. Starting in the mid-1920s, Herero pastoralists moved into the area at cattle posts both east and west of the Namibian border.<sup>17</sup> The Herero began to deepen the waterholes and dig new ones to accommodate increased numbers of cattle. At first only a handful—about 50—came, but their herds grew rapidly and created a growing need for labour. After 1954, when an influx of Herero immigrants increased the area's non-San population fivefold, the demand for !Kung labour rose still further. Dobe-area Herero remained oriented to subsistence pastoralism rather than moving into production for market; the market was distant and the price for cattle low. Except for a few cattle sold to pay for special purchases, such as guns, horses, or sewing machines, the Herero preferred to let their herds expand and to draw additional !Kung labour as necessary into the work of managing them. By the late 1950s the job of herdboys had become normative for Dobe-area !Kung men between the ages of 15 and 25.<sup>18</sup> In 1963 there were about 460 !Kung in the Dobe area, 340 Herero and other non-San, and about 2,000 head of cattle. About half of the !Kung young men of the age-category called *≠doiesi* (adolescents) were working on the cattle at any one time. Eventually most men returned to their camps to marry and raise families, but some married men stayed on in a semi-permanent arrangement with Herero families.

By the 1960s an alternative economy had begun to crystallize, and the Dobe !Kung were found distributed between two kinds of living groups. About 70% lived in camps—bandlike multifamily units whose members engaged in a mixed economy of foraging, *mafisa* herding, and some horticulture. The rest lived in client groups

16. The *mafisa* cattle may well have degraded the environment, but the appearance of these effects was delayed.

17. The Herero had been driven out of Namibia by the German colonists in 1904–5, and, cattleless, had sought refuge among the Tswana in Bechuanaland. Through *mafisa* they had quickly rebuilt their herds.

18. The gradual shift of the Dobe-area !Kung onto the cattle posts contrasted sharply with developments in the adjacent Nyae/Nyae area. In 1960, 800–1,000 !Kung were rapidly recruited to a South African settlement scheme at Tsumkhwe (Volkman 1983, Marshall 1980). The effects of the settlement on the Dobe !Kung are discussed in Lee (1979).

consisting of retainers and their families attached to black cattle posts. Despite the variety of economic strategies that supported them, camps continued to exhibit the characteristic patterns of collective ownership of resources and food sharing that have been documented for hunter-gatherers around the world (Lee 1979, Leacock and Lee 1982). The client groups offered an instructive contrast, being in effect appendages of the domestic economy of their Herero masters. The men worked alongside their Herero counterparts herding the cattle, while the !Kung women shared in the domestic tasks with the Herero women. Some client groups consisted of a !Kung woman married to a Herero man and her relatives, and a few involved a !Kung man, his (!Kung) wife, and their children and relatives (Lee 1979:54-61). The camp-living !Kung also maintained ties to the cattle posts; Dobe residents frequently went to Mahopa to ask for milk, meat, or other items. The cattle-post !Kung acted as conduits for the transmission of Herero goods to the population at large.

The stage was now set for the final act in the transformation of the Dobe-area !Kung from a relatively autonomous people with long-standing but non-decisive linkages to the larger regional pastoral, tributary, and mercantile economy to a people bound to the region and the world by ties of dependency. Having survived long-distance trade, contacts with European hunters, Tswana overlordship, *mafisa* herding, direct employment on cattle posts, even forced resettlement in Namibia, the !Kung became dependent largely as a consequence of the inability of their land to support a foraging mode of production. The bush had always been the backdrop to economic change, giving the !Kung security and a degree of freedom not available to the great majority of the agrarian societies of southern Africa. Tlou (1985:54) speaks of the Tawana's difficulties in exacting tribute or service from the "BaSarwa" (San) and concludes, "The sandbelt BaSarwa rarely became serfs because they could easily escape into the Kgalagadi Desert." By 1970, however, four decades of intensive and expanding pastoralism had begun to take their toll on the capacity of the environment to support hunting and gathering. Cattle grazing and the pounding of hooves had destroyed the grass cover over many square kilometers and reduced the available niches for dozens of species of edible roots and rhizomes. Goat browsing had destroyed thousands of berry bushes and other edible plants. The reduction or removal of these food sources placed added pressure on the remaining human food sources; for example, mongongo nut harvests noticeably diminished in the 1980s. The drilling of a dozen boreholes in Bushmanland, Namibia, just to the west of Dobe, in the early 1980s aggravated these trends by lowering the water table. Hunting remained viable but became subject to much stricter controls by the Game Department, and many men, fearing arrest, stopped hunting.<sup>19</sup> The effect of

19. In 1987, after an aerial survey indicated that game was plentiful, the administration eased the game regulations, and hunting increased but still without guns.

these changes was seriously to undermine the foraging option and to force the Dobe-area !Kung into dependency on the cattle posts and particularly the state. The latter responded with large-scale distribution of food relief between 1980 and 1987, which further deepened dependency.

## Discussion

What common and contrasting patterns of change can be discerned by a comparative analysis of the two case studies?

In the earliest period for which we have information, the pre- and protohistoric (ca. 1820), the Western Kweneng San were already sharing their land with Bantu-speaking Kgalagadi, who mediated their contact with the wider world. The Dobe San, by contrast, were in unmediated though distant and intermittent contact with riverine peoples to their east and north. A second point of difference concerns the nature of social formations on the San peripheries after 1830. The Kwena in the south became more mobile and expansive, ranging widely in search of trade and tribute, while the neighbours of the northern !Kung were sedentary, river-oriented peoples who did not expand into the arid interior.

The fur-trade period (mid-19th century) was marked by social, political, and economic turbulence, yet by the time its ripple effects reached the interior of the Kalahari the impact was often attenuated.<sup>20</sup> If in Parsons's (1977:119) terms the 19th-century Tswana economies were becoming the "periphery of the periphery" of European capitalism, then surely the Kalahari must have been the "deep periphery." Driven by trade and external threat, strong chiefdoms arose in the south. The Kwena's need for guns to defend themselves against the Boers was a powerful impetus for the articulation of tributary and mercantile systems. Guns could only be obtained in exchange for desert products. The Kwena subjugated the Kgalagadi, who in turn enlisted the San to aid in primary production. While unequal exchange characterized British-Kwena and Kwena-Kgalagadi relationships, the Kgalagadi-San relationship was symbiotic if not entirely equal. In contrast, Dobe was part of a much more tenuous and extended trade network. The Ngwato occupied the pivotal position between mercantile and tributary networks. Their junior partners were the Tawana, nominal overlords of Ngamiland, who in turn relied on Yei and Mbukushu ("Goba") intermediaries to accumulate desert products from the San, including the distant !Kung. The Tawana's power was contested by other chiefs, and they were never able to consolidate their hold on the hinterland as effectively

20. Exceptions, of course, exist. Tlou and Campbell (1984:109) describe a battle between the Ngwaketse and the Amandebele in the Duiwe area ca. 1830. The skirmish was brief, and neither group remained in the area, nor had either been resident there for any appreciable length of time.

as the Kweneng (Parsons 1977; Livingstone 1857; Tlou 1985:66–67). As a consequence there was less pressure on the !Kung to enter the system, and when they did they were able to retain more control over the terms of trade. In neither instance, however, did the fur trade have much impact on the internal organization of San societies. San exchanged their products after the completion of the productive process. Linkage was predominantly through the sphere of exchange, not production, and intervention in San society remained limited (see Bonner 1983 and Harries 1982 on similar processes elsewhere).

The expansion of herding and farming to the remoter Kalahari did not signal the end of the fur trade, but the incorporation of cattle into the desert economy shifted the priorities in the deployment of land and labour. Western Kweneng San and Dobe San entered the cattle economy under different circumstances and with different statuses. In Dutilwe, Kgalagadi acquired cattle and rendered them as *mafisa* to their poorer relatives; eventually San became their herdboys. Because cattle were kept in the village, not at distant cattle posts, San herders were in regular interaction with their employers and had their subordinate status frequently reinforced. In Dobe it was the San themselves who entered into *mafisa*, a privilege they held exclusively until the 1920s. The Tawana were absentee cattle owners; the Dobe San bore responsibility for the productive enterprise, made routine decisions, and determined their daily activities. This arrangement was much more compatible with foraging than the Western Kweneng San's situation. In neither case did even a majority of the San enter into cattle service. Many relatively independent groups remained on the peripheries of villages and cattle posts, subsisting on wild foods and continuing to provide furs for the trade. Reciprocity between foraging and non-foraging San allowed each group to enjoy the fruits of the other's labour. In lean years the foraging San would provide a safety net and alternative subsistence for their "employed" relatives, and even in good years San contact with pastoralists was largely limited to certain seasons. At all times the hinterland provided a cultural point of reference and locus of reproduction. Thus in both cases the complete incorporation, as dependents, of the San into the agro-pastoral system was delayed as long as the bush held the possibility of an alternative livelihood.<sup>21</sup> An important source of the continued viability of the San's foraging option was the strength of the egalitarian and reciprocal communal relations of reproduction that characterized life in the bush. As even the revisionists (e.g., Wilmsen 1989:66) acknowledge, this way of life, while far from ideal, provides an extraordinarily rich and meaningful existence for those who prac-

tice it. Communally based societies offer their members a sense of social security, entitlement, and empowerment (Lee 1988, n.d.; Rosenberg n.d.). Aspects of this quality of life persist in both Dutilwe and Dobe even today.<sup>22</sup>

Several factors combined to undermine the viability of the dual subsistence economy of the Dutilwe and Dobe San. Expansion of the numbers of cattle through natural increase, purchase with wages from other areas, and migration of cattle keepers (as in Dobe after 1954), along with expanding opportunities for migrant wage labour, especially in the 1960s, created a rapidly increasing need for San labour. It is fair to say that without the availability of a reservoir of San labour to replace absentee blacks the Kgalagadi and Herero could not have enjoyed the prosperity they experienced in the 1960s and 1970s. (San men also went to the mines but in much smaller numbers; in 1969 7% of Dobe San men and in 1978 25% of Dutilwe San men had done so.) At the same time, plow agriculture was expanding, especially in the south, increasing the demand for both male and female labour.

The retreat from foraging by the San began as the agro-pastoral complex drew larger and larger numbers of labourers, male and female, into its employ. In the last analysis, however, a critical factor in moving the San into a position of dependency has been environmental degradation, which has, like an unintended scorched-earth policy, deprived them of an alternative means of livelihood. In the south, dependency increased throughout the century, and many San entered into a relationship of perpetual minor status. Cultivated food and water have a powerful attraction for people foraging in a degraded environment, and it was the possession of cultivated food and water sources that distinguished the haves from the have-nots. In the north, where the local blacks have a healthy respect for the San's determination to protect their water claims, in the last century by force of arms and today through the courts, the organic link came later. For the Dobe San comparable levels of dependence and integration were approached only in the 1970s and 1980s, and it was the government, not neighbouring blacks, that provided the material incentive in the form of wells, schools, grain, and other forms of aid.

### Foragers Genuine and Spurious: The Limitations of World Systems

What kinds of socioeconomic arrangements characterized the Kalahari San in the 19th and 20th centuries, and what kinds of explanatory frameworks best account for them? These questions must be approached at two levels: the level of fact, in which the archaeological, ethnohistoric, and ethnographic evidence is set out and interrogated, and the level of discourse, in which the ex-

21. Both the Kgalagadi and the Herero were "devolved" pastoralists with the socioeconomic infrastructure to facilitate the rapid reabsorption of livestock into the cultural system. If, as is suggested by Schrire (1980), the San were also "devolved" pastoralists, why did they not follow in their neighbours' path and become predominantly pastoralists in the 20th century?

22. These qualities may coexist with dependency, but we see no reason to believe that they are caused by it. For a recent statement of what primitive communalism is and is not, see Lee (n.d.).

planatory frameworks themselves become the focus of interrogation.

The archaeological record shows a diversity of economic adaptations in the 19th century and earlier. The interaction of Stone Age with Iron Age cultures resulted in dramatic economic shifts in some areas, while in other areas the effects were more subtle. Kalahari trade was widespread, and in many instances when tributary formations emerged in the 19th century ties of domination/subordination were superimposed on preexisting linkages. But not all San groups experienced this pattern of early linkage and later subordination. Interrelationships were strongest on the river systems and the margins of the desert and weaker as one moved into the interior. Thus there were large areas of semi-arid southern Africa that lay outside tributary orbits, where trade was equal, non-coercive, and intermittent and where independent—but not isolated—social formations persisted into the 20th century.

In attempting to explain this situation, it is important, first, to recognize that trade and exchange cannot simply be equated with domination and loss of autonomy. Exchange is a fundamental part of human life and appears in all cultural settings (Mauss 1925, Lévi-Strauss 1949). Hunter-gatherer peoples have participated in exchange with farming and market societies for hundreds of years (in India, South-east Asia, and East Africa) while maintaining a foraging mode of production (Leacock and Lee 1982). Even with "hunters in a world of hunters," exchange was part of social life (see, e.g., Thomson 1949, Wilmsen 1974, Earle and Ericson 1977, Ericson 1977, Torrence 1986). The evidence for long-established trade relations between foragers and others has been glossed by some as evidence for the fragility of the foraging mode of production. But if it was so fragile, why did it persist?

Throughout these debates about the status of Kalahari and other foragers there has been a lack of attention to the meanings of key terms. Just what is meant by "autonomy," "dependency," "independence," "integration," and "servitude" is rarely made clear. Without consistent, agreed-upon definitions it will be difficult or impossible to resolve the issues with which we are concerned. "Autonomy," for example, has a wide range of uses. Given its currency, it is remarkable how unreflexive its anthropological uses have been. We will confine our discussion to economic autonomy, since much of the debate in hunter-gatherer studies seems to revolve around it. One of the rhetorical devices of the revisionist view of hunter-gatherers is to equate autonomy with isolation—a definition so stringent that no society can possibly satisfy it. But autonomy is not isolation and no social formation is hermetically sealed; we take it as given that all societies are involved in economic exchanges and political relations with their neighbours.

As an economic concept, autonomy refers to eco-

nomically self-sufficiency,<sup>23</sup> and self-sufficiency in turn hinges not on the *existence* of trade—since all societies trade—but on whether that trade is indispensable for the society's survival. To demonstrate autonomy one must demonstrate self-reproduction. Dependency therefore may be defined as the inability of a society to reproduce itself without the intervention of another.<sup>24</sup>

Politically, two kinds of autonomy may be provisionally defined: imposed and asserted.<sup>25</sup> In the former, the economic autonomy of a subject group may serve the interests of the dominant group. Subordinates are encouraged to pursue their habitual activities at their own pace while providing goods or services—often on equitable terms—to the dominant group. In the latter, the autonomous group asserts its claims through its own strengths and political will. In practice these two forms may be difficult to distinguish, and which form is considered to be present will depend heavily on subjective judgements both by the peoples involved and by observers.<sup>26</sup> Thus the Mbuti pygmies observed by Turnbull (1962) appear to be entirely subservient to their black neighbours while they are in the villages but quite autonomous in the forest.

Autonomy is best regarded not as a thing or a property of social systems but as a relationship—between social groups and between a group and its means of production. At any given moment a society may exhibit elements of both autonomy and dependency, and it should be possible to assess the degree of each through empirical investigation.

The camp-dwelling people of the Dobe area were economically self-sufficient during the 1960s. They owned the bulk of their means of production and paid no rent, tribute, or taxes in money or kind. They hunted and gathered for the large majority of their subsistence requirements and for the rest tended *mafisa* cattle or worked as herdboys for their Herero neighbours. The latter tasks provided income that was a welcome supplement but not essential to survival. How can we demonstrate its non-essentiality? First, San *mafisa* holders and herdboys were observed to leave "service" without visible detriment to their well-being. In fact, it was common for young men to work on cattle for a few years and then return to the bush at marriage (Lee 1979:58, 406–8). More compelling, in the drought of 1964 Herero crops failed and cows were dry yet the San persevered without evident difficulty. In fact, the Herero women were observed gathering wild foods alongside their San neigh-

24. For example, Memmi's (1984:185) definition of dependence as "a relationship with a real or ideal being, object, group, or institution that involves more or less accepted compulsion and that is connected with the satisfaction of a need" is consistent with our own usage.

25. We are indebted to Gerald Sider for this suggestion and some of the discussion that follows.

26. The subjectivity involved in determining whether a given autonomy is asserted or imposed has been a major problem in articulation theory (e.g., Foster-Carter 1978, Clarence-Smith 1985) regarding whether a given "tribal" communal social formation was preserved because its maintenance was "functional" for capitalism (Wolpe 1972) or because the local system and the local people were strong enough to resist (Beinart 1985).

23. Political autonomy, by contrast, hinges not on a society's capacity to reproduce itself but on the willingness of other (dominating) societies to let it remain autonomous. Neither Dutilwe nor Dobe could be said to have been politically autonomous in the 1960s.

bours (Lee 1979:255). Since the San carried on through this period without visible hardship (Lee 1979:437-41) despite the withdrawal of Herero resources, it is clear that the latter were not essential to their reproduction.

These lines of evidence argue for the economic autonomy of some Dobe !Kung in the 1950s. Obviously a great deal more could be said on the question of autonomy, especially from the cultural and political points of view. Even the simplest historical judgements will involve a series of mediating judgements concerning economy, polity, voluntarism and coercion. Automatically classifying 2d-millennium San societies as dependent, incorporated, or "peasant-like" seems no more legitimate than classifying them as "primitive isolates."

Turning to "servitude," we are confronted with a literature replete with reports of San "dependency," "serfdom," "slavery," "vassalage," and the like.<sup>27</sup> In contrast to the early sources cited above (and see Wilson 1975:63), which tended to portray all San as dominated, recent ones such as Silberbauer and Kuper (1966), Tlou (1977), Russell and Russell (1979), Hitchcock (1987), and Motzafi (1986) employ these terms more critically, but even here usage tends to be imprecise. Silberbauer and Kuper (1966), for example, use the term "serfdom" but note its inapplicability—the San being bound neither to the soil nor to a particular master. Guenther (1985:450) reinforces the ambiguity when he speaks of a "benignly paternalistic form of serfdom" that departs from the European pattern. Tlou (1977), Wilson (1975), and Biesele et al. (1989) use the term "clientship" to refer to a loose association between peoples with unequal access to resources that they distinguish from the classic patron-client relationship. Russell and Russell (1979:87) further qualify the term "clientship" by contrasting the rights and obligations of "employed" San with those of "client" San. The latter are said to maintain a "foot in both worlds," one in the bush and one on the farm. Thus in their terms clientship is a partial relationship from which San can disengage.

Difficulties on several levels are encountered when we try to pin down the forms and content of San servitude and dependence. First, it is obvious that terms such as serfdom and chattel slavery, developed in a specific European context, are not easily grafted onto Kalahari social relations. More specifically, the language that is used in the Kalahari itself appears to overstate the degree of dependence. Both Vierich (1982) and Solway were struck by the exaggerated descriptions of servitude by San and black alike. The cultural vocabulary of superior/subordinate relations further illustrates the difficulty of translating words that lack cognates in the language of

the observer. Silberbauer and Kuper, for example, show that the Sekgalagadi term *munyi*, used for "master" in San-black relations, is also used for the senior in asymmetrical kin relations, i.e., "elder brother." It denotes authority but falls short of our concept of master-ship or ownership. Similarly, they note that the Tswana "jural model" of *bolata* (hereditary servitude) signifies something stricter than actually exists. This misunderstanding, they assert, may be the reason social commentators from 19th-century missionaries to 20th-century anthropologists have assumed that *bolata* was worse in the past and only recently has become more humane. They argue that "the practice of serfdom in Bechuanaland is much more humane than the indigenous jural model would lead one to expect: in the past some observers may have been led into assuming that the jural model represented the past, while the easy-going actuality was equated with the enlightened present" (p. 172).

At the level of concrete social relations, there is a puzzling incongruity between the exaggerated degree of inequality described by Kalahari residents and the relative ease (and frequency) with which the San "serfs" disappear into the desert for periods of time, leaving their "masters" high and dry. Vierich (1982:282) has argued that "interdependence" more accurately describes the relationship between San and non-San and that San simply "play the beggar" to get handouts. While this may be overstating the case, clearly there is a disjunction between model and practice. In no instance in which hereditary serfdom has been asserted by Kgalagadi in theory has it been observed in practice. The Dutlwe-area San may be dependent and have to work for someone at some time, but they retain some choice of when to work and for whom. An observer will find some San in relations of dependency and others not, but closer examination will reveal that the same individuals will move into clientship, out to the bush, and back again to clientship. Wealthy blacks will have full-time San labourers living in their compounds while their neighbours rarely or never retain San clients.<sup>28</sup> The full-time labourers living with blacks will be the most conspicuous to casual observers, and this may account for the prevalence of this kind of report in both the early and the more recent literature, but such reports fail to do justice to the complexity and fluidity of the situation. We certainly do not want to minimize the degree of San dependence and subjection to discrimination, but we would suggest that this is best seen as a product of underdevelopment and not a primordial condition.

### Hunter-Gatherer and Agrarian Discourse: Making the Transition

We have traced in some detail the historical pathways followed by the Dutlwe and Dobe San as they changed

27. The issue received international attention in the late 1920s and 1930s, when a series of reports was commissioned by the London Missionary Society, the British, and the Ngwato chief concerning the status of the San in the Ngwato Reserve. The question was whether the San were in a state of slavery. As might have been predicted, there was no consensus, and while instances of hereditary servitude were noted it was clear that this condition was not general and that many San persisted in their "miserable nomadic existence" (Tagart 1933, quoted in Miers and Crowder 1988:188).

28. Ethnic boundaries may also be blurred, and in many instances poorer blacks and San live similar lives, intermarry, and defy any neat ethnic categorization or hierarchy.

from autonomous foragers to clients and labourers increasingly subject to and dependent upon local, national, and world economies. In order to understand these processes it is necessary to make a second transition, from discourse about hunter-gatherers to discourse about agrarian societies and the emerging world system.

In agrarian discourse structures of domination are taken as given; it is the *forms* of domination and the modes of exploitation and surplus extraction that are problematic (Amin 1972, Hindess and Hirst 1975, Shanin 1972). In the literature on the agrarian societies of the Third World, stratification, class and class struggle, patriarchy, accumulation, and immiseration constitute the basic descriptive and analytical vocabulary. In hunter-gatherer discourse it is not the forms of domination that are at issue but *whether domination is present*. This question is often side-stepped or ignored.

We are not alone in our concern about the tendency for the discourse of domination to be imposed on pre-capitalist societies. Beinart (1985:97), for example, dealing with the Eastern Cape—an area under far greater pressure than the Kalahari—cautions against granting omnipotence to capitalism or the state or assuming that the migrant-labour system automatically destroys the integrity of rural societies:

Even in so coercive an environment as South Africa, the patterns of domination were constrained—in part by fear of the consequences of other routes and in part by the defensive responses of the dominated. Certainly, capital and the state . . . had only limited power to shape social relationships in those areas which were left under African occupation. . . . the fact that a migrant works for a wage, even for a number of years, does not necessarily determine the totality of his, much less his family's, class position and consciousness. The importance of defensive struggles in the rural areas, amongst communities which included seasoned migrants, has generally been underestimated.

Silberbauer (1989:206–7) challenges the view that hunter-gatherer contacts with other societies necessarily preclude autonomy:

[The] concept of coexisting states, tribes, and hunter-gatherer bands can be found accurately documented in any authoritative history of the appropriate part of Africa. It does not require that any of the coexisting societies be in a state of compulsory, day-to-day mutualism with all others. Interaction can occur at sufficiently low intensity and be of such a quality as to allow hunters and gatherers (for instance) to retain cultural, social, and political, and economic autonomy (i.e., in the philosophical sense, not in that of isolated, complete independence). At least in southern Africa and Australia that state of affairs persisted only when the hunter-gatherers were able to retain control of enough resources of sufficient variety to be largely . . . self-sustaining.

Perhaps the most serious consequence of imposing agrarian discourse on hunter-gatherers is that it robs the

latter of their history. What is at issue here is an intellectual neo-colonialism that seeks to recreate their history in the image of our own. This revisionism trivializes these people by making their history entirely a reactive one. Even at its best revisionism grants historical animation and dignity to the San only by recasting their history as the history of oppression. But is their oppression by us the only thing, or even the main thing, that we want to know about foraging peoples? The majority of the world's foragers are, for whatever reason, people who have resisted the temptation (or threat) to become like us: to live settled lives at high densities and to accept the structural inequalities that characterize most of the world. Many former foragers—and that includes most of us—now live in stratified, entrepreneurial, bureaucratic society, but not all have followed this route, and the presence or absence of inequality and domination can be investigated empirically.

Ultimately, in understanding the histories of Third World societies or of our own, we will have to rely on the histories of specific instances and not allow preconceptions to sway us. This caveat applies equally to those who would place the hunter-gatherers in splendid isolation and those who would generalize the power relations of contemporary capitalism to most of the world's people through most of their historical experience.

## Comments

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Solway and Lee are to be commended for helping to set the record straight on historical and contemporary Kalahari forager-pastoralist relations. Of course present-day Bushmen or San are genuine foragers, and of course they have a history (or histories). The problem in much recent anthropological work has been an oversimplification of the relationship between foragers and non-foragers both by specialists and by other anthropologists who have made use of their data.

There is still a widespread belief that the !Kung (if not other Bushman groups) lived in a state of isolation until the 1950s and 1960s, when the Marshall family, and subsequently Lee and his students, arrived on the scene (cf. Barnard 1989). On this score, Lee has become the victim of his own popularity. In earlier books and articles (e.g., Lee 1984:2–3) he has urged his readers to be wary of regarding the !Kung and other foragers as "lost tribes" or remnants of the Pleistocene, but his vivid descriptions of !Kung life have been taken to imply a state of isolation and "original affluence" nowadays almost unique in the world. These descriptions have met with hostile criticism from those who favour approaches which give greater emphasis to outside influences.

As Solway and Lee point out, scholars who have concentrated on historical issues have added much to our



understanding of the Kalahari past, but their perspective is nevertheless different from that of either Solway (who has concentrated mainly on Kgalagari rather than Bushman society) or Lee. In fact, I would isolate three rather than two major theoretical perspectives in Bushman ethnography and archaeology. These need not necessarily be regarded as opposing viewpoints. To some extent they complement each other, and there is no reason one should not adopt one or another according to one's particular purpose. Those who hold essentially to a regional-historical perspective, such as Denbow (1984), Schrire (1980), Gordon (1984), and to a lesser extent Wilmsen (1989), attempt to account for broad trends within the culture area (or set of overlapping culture areas) as a whole and ask questions about the relative significance of contact, trade, small-scale pastoralism, etc. Those who hold a more ethnographically specific perspective, such as Lee and Silberbauer in most of their work (e.g., Lee 1979, Silberbauer 1981), emphasize cultural units (!Kung, G/wi, et al.) and examine their contact with other such units. The third perspective, which is characterized by a concentration of interest in economic pluralism and dependency rather than history or culture per se, is found in much of the work of Guenther (e.g., 1979) and Hitchcock (1978), among others.

I read the present paper as an implicit attempt to reconcile these various perspectives. In bending towards more historical and culture-contact approaches, we should nevertheless be careful not to lose sight of the immense diversity of culture-contact situations in the Kalahari or of the historical continuity of !Kung, Tshasi, Kwa, and Khute cultures (cf. Barnard 1988a). If a wider understanding of foraging society can be reached in the process, then all the straw men of the past will have served their purpose.

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Solway and Lee's paper is welcome both for its topic and for its tenor. Many of us have tried to call attention to the excesses of an intellectual backlash with regard to hunting-gathering peoples. The debate has become polarized, with post-Stewardian cultural ecologists (for lack of a better term) at one end and "revisionists" at the other, each camp impervious to opinions other than its own. Post-Stewardian cultural ecologists (among whom I would place myself as well as Lee) are represented as misguided apologists for the eternal, ubiquitous, pristine, isolated, affluent hunting-gathering band. While we do stress the adaptive value of a societal form with demonstrably effective material and social technologies, no one has ever suggested that such cultures are immune to change. What makes them extraordinary is that they reject the premature epitaphs of advocates of unilineal progress. Solway and Lee acknowledge the validity of the revisionists' queries while arguing for diversity of modes of acculturation among hunter-gatherers in pre-

historic and historic times. Since I agree with them and appreciate their supportive documentation, I will just mention a few points that I believe deserve reiteration.

1. The planet on which we live is a dynamic system, and therefore continuous and ubiquitous *change is a given*; it is not an option either for the "primitive" or for the "civilized." As Solway and Lee suggest, it is only the rate and mode of change that are open to observation and interpretation.

2. Revisionists seem to take a unilineal stance, to envision a single-track cultural progression. Acculturation is situationally varied, and in the context of adaptation contact and autonomy are not mutually exclusive; not being pristine does not mean being degraded and encapsulated. The preponderance of evidence suggests that hundreds of years of contact did not result in abandonment of autonomy by groups characterized by "flexible egalitarian sharing." Kalahari San have clung to their biocultural well-being through the option of a "bush life-way," now lost to them through environmental degradation.

3. It is important to separate *general* statements (e.g., small-scale societies strain to preserve autonomy) from *particular* observations of a specific response to a specific acculturative pressure (e.g., a specific band establishes a mutually beneficial relationship with a peasant society).

4. The dominance of Eurocentric semantics to which Solway and Lee point with regard to concepts such as independence, isolation, servitude, ownership, and stewardship is apparent in their own implication that "development" is of necessity good and/or inevitable. In an evolving system, adaptation is the goal, and well-being is adaptation with minimal stress. In the human context well-being depends on both material and social technology. Thus the forced demise of hunter-gatherers may be no more of an adaptive failure than the demise of social technology among *developed* societies. The enterprise to which we students of small-scale societies most need to turn our energies is the revision of our conceptual lexicon. We seem to be comparing socks with bananas, and no matter how good or abundant the data on socks and bananas this is an exercise in futility. Is it not worthy of some generalization on human adaptation, given the pressure to hurry small-scale societies along the path of "progress," that so much autonomy and egalitarianism still survive?

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The history of human occupation of the Kalahari is a book but recently opened. Of necessity, today's statements are based on a minimum of research; 50 years hence a more coherent picture will have emerged. Although in its infancy, Kalahari archaeological research has demonstrated the presence of food-producing peoples in a few better-watered areas for some 1,500 years and indicated contact between them and some Stone



Age peoples. Domestic stock (sheep and cattle) and the art of pot-making, possibly transferred from the north through Stone Age peoples, had reached these areas prior to A.D. 500 and the advent of metalworking pastoralists/agriculturalists. Other research has shown that some groups living along the watercourses of the northern Kalahari today speak Khoisan dialects and practise some San customs but by blood are of black rather than San origin.

Rock paintings in fairly early styles at Tsodilo in the northern Kalahari include many cattle: two beasts being either herded or stolen, single beasts accompanied by schematic humans with erect penises, a cow sandwiched between two large mythological animals (equine hippos) with rain falling through them to produce four streams (of milk?) from where its udder should be, a running herd with penis-erect schematic humans and a human disguised as an ostrich, single beasts either isolated or positioned next to other animals (rhino, eland, etc.), and three white schematic cows with grid bodies. Paintings of sheep have not been found, although two white paintings may depict goats.

Excavations at Toteng (near Lake Ngami) and at Tsodilo by Lawrence Robbins and me suggest that microlith manufacture continued until at least A.D. 1550–1650. Also, some San still periodically carve melon knives from ostrich bone and arrowheads from horn and bone, although metal and fencing wire are freely available.

In eastern Botswana (outside the Kalahari) in areas that fell within the domains of the Khami state, descendants of San complain that Batswana still deliberately call them "Basarwa" (Bushman) to differentiate and distance themselves from them even though today they live a settled agro-pastoral existence.

Non-San Kalahari peoples of today include Bakhlagari, Batswana, Bayeyi, and Hambukushu, who have (or until recently had) San clicks in their languages, and Ovaherero and Ovambanderu, who employ no clicks. Although Batswana are relatively recent arrivals in the Kalahari, much points to a long past association between them and San somewhere in southern Africa. Tswana rainmaking involves large snakes, rainbulls, and eland's fat; "let the eland die" occurs more than once in praise poetry; fables in the two languages have marked similarities. Openness, caring for others within the group, sharing and communal eating of meat of cattle and large animals are all reminiscent of San values. Apart from clicks in the language, Batswana and Bakhlagari have some San blood and other physical attributes. Unfortunately, too little research has been conducted in these areas to be dogmatic about relationships between San and non-San, but they appear to exist.

In summary, what is suggested is that (1) at Tsodilo, cattle were important in San culture before or just after the arrival of non-San in the area; (2) the ancestors of today's "black" San were immigrants from the north who adopted San culture; (3) ancestors of San and Batswana intermarried somewhere, sometime in southern Africa; (4) some facets of Tswana and San beliefs and

customs were borrowed/evolved in proximity, with no certainty at this point as to who got what from whom; (5) the manufacture of implements of bone and stone continued for 1,000 years after the advent of metalworking; and (6) today some non-San wish to differentiate and distance themselves from San. Finally, we should remember that during the Early Iron Age in southern Africa, no massive waves of Bantu-speakers surged southward; rather, foreigners trickled into a vast land populated by Stone Age peoples, a few of which in better-watered areas kept domestic stock. Whether these foreigners brought cattle with them or acquired them from Stone Age peoples has not been conclusively demonstrated.

Taken together, the foregoing pose a number of questions: Who dominated whom (if either dominated the other) at first, and what did each learn and acquire from the other? Was the Kalahari, as it now appears, better-watered then than it is today? Who were the ancestors of the "black" San, and what part did they play in culture change? What links were established between differing groups, and how far did these extend? How resilient was each culture to change?

Lee and others have provided invaluable material on particular groups of San as they found them at a point in time, even as Isaac Schapera has done for Batswana. Certainly, not all San have always been pure foragers, but this does detract from their work. Now appears to be the time to build on it, to answer some of the questions just posed, and, using such avenues, to supplement archaeology in turning more pages in the history of the Kalahari.

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Solway and Lee reference Yellen and Brooks in claiming that "despite the abundant evidence of Iron Age settlement elsewhere in northwestern Botswana dating from A.D. 500 or earlier and despite concerted efforts to find the same in the Dobe area, there is *no archaeological evidence of Iron Age occupation of the area until the 20th century.*" This is misleading. Despite the recent publication date of the references cited, the reconnaissances referred to were carried out almost 20 years ago, at a time when it was a priori assumed that there could be no Iron Age presence in the Dobe area. These surveys were focussed on permanent pans and surrounding sand dunes where it was expected that Stone Age hunters and gatherers would have settled, not in the areas, such as the margins of the nearby Aga Hills (ca. 15 km distant), in which soils and other ecological conditions are more favourable to Iron Age occupation. No one with any familiarity with Iron Age settlements has ever spent more than a few hours looking for sites in the Dobe area. Solway and Lee's negative evidence should therefore be taken for what it is—*no evidence at all.* One could, in rebuttal, point out that Yellen and others visited and carried out excavations in the Tsodilo Hills in the early

1970s as well without discovering the extensive Early Iron Age settlements that Wilmsen and I have subsequently located and excavated there.

Likewise, the evidence they present to back up their assertion of "relative continuity" in hunter-gatherer life in the Dobe area for "at least several thousand years" is wholly inadequate. What does it take to initiate a measurable change in hunter-gatherer life? Would we recognize it archaeologically if we saw it? Can the LSA data from Dobe rule out the possibility that pre-Iron Age LSA populations were seasonally transhumant between the delta and the sandveld, resulting in much larger territories than are common today? Can they tell us whether LSA peoples shifted their hunting and foraging strategies and preferences in order to meet an expanded Iron Age demand for wild products? The only published accounts of the fauna excavated from the Dobe region (Yellen and Brooks n.d.) are wholly inadequate to answer these and many other questions. Only 13 identified faunal remains were recovered from Mahopa, none from /Xai/Xai 1, and 2 from /Xai /Xai 2. Indeed, these very data could be used to show that significant shifts *did* occur at Mahopa between the upper levels (2–6), dating back to about 600 years ago, in which buffalo made up 60% of the 5 animals identified, and the lower deposits, dating between 2,000 and 3,000 years ago, in which no buffalo at all were recovered and zebra made up 63% of the 8 animals recovered. Are we looking at reality here—say, the impact of guns, wagons, or iron spears on the hunting of dangerous game? Or are these differences simply an artefact of sampling error? To help readers assess whether "unwarranted conclusions have been drawn from scanty data," I summarize below relevant archaeological data from the *sandveld* in the Tsodilo Hills, 70 km west of the Okavango and a mere 125 km northeast of Dobe. A fuller account of these excavations can be found in Denbow (n.d.).

The site of Divuyu documents the initial Bantu Iron Age occupation of the Tsodilo Hills with dates between A.D. 550 and 730. Excavations from six major units at Divuyu (totalling 27 m<sup>2</sup>) provide an artefactual assemblage that includes 16,376 potsherds, 226 iron beads, bracelets, and tools, 15 copper objects, 9 perforated fragments of ceramic salt strainers, 4 pieces of carved ivory, and 1 backed segment made of chert. The virtual absence of lithics in comparison with metal implements suggests some separation between indigenous, stone-using peoples and entering, iron-using agropastoralists at this time. The material culture at Divuyu thus does not provide strong evidence for joint occupation of the site with neighbouring stone tool-using foragers and pastoro-foragers during the initial stages of contact. This is the sort of picture that Solway and Lee would have us believe characterized Khoisan-Bantu interaction in north-western Botswana up to the time of their visits in the 1960s and '70s, or at least until the 19th or early 20th century.

The faunal assemblage from Divuyu (Turner 1987) represented a minimum number of individuals of 248. Of these remains, 65% were from goats or sheep, 3%

from cattle, 7.5% from fur-bearing animals such as jackal, fox, and caracal, 20.5% from common sandveld animals such as eland, impala, and duiker, and 4% from animals such as lechwe and waterbok, whose habitat is almost exclusively permanent swamp. Remains of at least 18 fish were also identified. The presence of fish and other delta animals at Divuyu is incontrovertible evidence that interregional exchange with delta peoples was occurring and that this exchange included mundane dietary items in addition to luxury goods. Some of the wild game (and perhaps cattle) undoubtedly came from exchange with neighbouring sandveld peoples.

Since there are no copper deposits within 200 km of Tsodilo and the nearest usable iron ores are reported to occur only in the delta region and farther north, these goods must also have been obtained through regional or interregional exchange, perhaps channelled along the river systems and then directed westward into the sandveld at Tsodilo. Alternative copper sources, in historic times controlled by Khoisan, also occur at Tsumeb in northern Namibia.

If Divuyu were the complete story at Tsodilo one might find it more difficult (though certainly not impossible [cf. Schrire 1980]) to argue with Solway and Lee's scenario of intermittent and insignificant contact between foragers and farmers as opposed to the more substantive and systemic linkages proposed by us "revisionists." The Nqoma site (A.D. 850–1090), approximately 1 km south of Divuyu on a lower plateau of the same hill formation, however, provides extremely strong evidence for a more integrated, regional political economy—an economy that attracted and articulated with rather than excluded the autochthonous peoples of the region. The major units of the Nqoma excavation (180 m<sup>2</sup>) produced 23,275 potsherds, 1,912 iron beads, tools, etc., 137 copper artefacts, 10 worked ivory fragments, and 32 glass beads and 13 marine shells from the Arab East Coast trade. In contrast with the 1 formal stone tool recovered from Divuyu, Nqoma yielded 83 backed segments, 31 scrapers, and 119 cores and other utilized flakes. Paralleling this increase in stone tool proportions is an increase in the proportion of hunted game, which made up 34.5% of the minimum of 197 individuals identified at Nqoma. Cattle (32.5%) and goats or sheep (25.5%) were the most important domesticated sources of meat. Fish and delta animals (1.5%) provide evidence for continued exchange in wild products with the Okavango to the east.

These changes in the subsistence economy and material culture of the sites at Tsodilo are indicative of a wider restructuring and transformation of relations within the region—relations which increasingly incorporated indigenous foragers, and perhaps pastoro-foragers, into an integrated regional exchange network. Evidence in the form of pottery and iron, associated with dates identical to those from Divuyu and Nqoma, indicates that this network reached into the heart of the Dobe area. These data can be seen as indicative of important shifts in economic finalities from "production for use," with its finite limitations on demand, to "produc-

tion for exchange," which brings with it the commoditization of wild products for exchange with an agricultural sector. Solway and Lee, in common with many other Marxist, as well as "substantivist," economic theorists, permit no possible non-zero-sum (capitalist?) alternatives to intrude upon their agrarian-vs.-hunter-gatherer typologies of the precolonial African world. Almost by definition, there can be no precolonial incentives for change based upon opportunistic responses to new economic, social, and market possibilities. Instead, we are offered models in which autonomy is achieved and maintained only through a kind of historical apartheid—change occurs only when trade is essential to survival, and such trade inevitably results in relations of "dependency" and consequent "cultural degradation."

Finally, Solway and Lee's presentation is not only ahistorical but condescending and patronizing to the very people it purports to defend from revisionists who would "call into question the claims to authenticity of a number of foraging peoples." Authenticity about what? What I, my colleagues, and many Kalahari residents including the San would like to call into question is the conditions of historical and evolutionary *uniqueness* and *isolation* that have been attributed to them by Lee and others. They are no more unprepared by history to cope with complex decisions involving economic and political alternatives than are rural peoples of this or any other region. For the past thousand years the social construction of identity in the Kalahari (and indeed today between the Kalahari and the Western world) has been based upon a dialectic of interdependence and mutual definition—relations in which both hunters and herders represent themselves as "symbolic transformations of each other predicated upon their economic differences" and thereby "reproduce both their own uniqueness and their continuing interrelationship" (Galaty 1986:114). Perhaps the exhortation to continue to believe in the historical uniqueness of the shrinking Dobe waterhole has more to do with the ideology of intellectual self-definition among some anthropologists than with any need for outside "authentication" of their lives by Kalahari peoples.

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Solway and Lee's major difficulty with the so-called Kalahari revisionists, namely, their alleged projection of a spurious uniformity on a vast and diverse region, is clearly a matter of interpretation. Wiessner (1986:121) concludes a recent article, "Historical studies have revealed that these conditions and attitudes have varied greatly throughout recent history (Gordon 1984)."

The people labelled Bushmen are one of the most heavily commoditized groupings in the annals of academe, but North American scholars, ignoring a large corpus of work in languages other than English, tend to reinvent them. Symptomatic of this is Solway and Lee's

rudimentary map. Even if, not reading German, they had consulted the maps in Schinz (1891) and Passarge (1907), they would have found wagon roads going through the Nyae/Nyae area. Their paper has important historical precedents in the work of Schott (1953, 1955), Kohler (e.g., 1966), Heintze (1972), and Szalay (1983), to mention just a few. To attempt to situate "the Kalahari San in history" while all but ignoring the rich and accessible archival material in both Botswana and Namibia bespeaks remarkable self-confidence. Is this perhaps the legacy of the heady days when history was out and ecological utopianism was in? I would have thought that Wiessner's work (e.g., 1986) would be relevant to the enterprise. Is she ignored because she belongs to the wrong anthropological horde?

Solway and Lee chide me for presenting an account for which less than 20% (?) of the material refers to the !Kung of the Dobe-Nyae/Nyae area. But most !Kung live in the Kaukaveld, northern Namibia (including Ovambo), and Angola, not in the Nyae/Nyae-Dobe area (Guerreiro 1968, Seiner 1913). They blandly assert that the copper miners near Tsumeb were Nama-speaking Heikom without giving sources for this claim, although in all likelihood it is Vedder, whose historiographical skills are controversial (Lau 1981). And what if the distance is 400 km (although my map shows the road distance between Grootfontein and Tsumkwe as just over 200 km)? Schinz travelled it in 11 days with a cumbersome ox-wagon, and even before the First World War items given to Bushmen on farms in Tsumeb district were found in the Nyae/Nyae area, while people from Nyae/Nyae were actively trading with people on and from the Okavango River (Muller 1912, Gordon n.d.).

Solway and Lee suggest that tales of oppression were exaggerated to serve colonial ends and thus by implication need not be taken seriously. But this does not apply where one would expect it to, in near contemporary Afrikaner anthropology, where it would have been a strong justification for apartheid. On the contrary, Afrikaner anthropology treats Bushmen much as Lee does, as people living in a reasonable degree of "primitive affluence" with a superior knowledge of veldcraft, and it is this discourse that provided the ideological rationale for their military recruitment by South Africa. "Revisionists," in contrast, would attribute their ostensible success as soldiers not to their veldcraft skills but to their pariah status within the wider society. And there might be a moral here more important than trying to take the principled high ground by the rhetorical assertion that the so-called revisionists are engaged in "intellectual neo-colonialism that seeks to recreate their history in the image of our own." By the same token it could be argued that Solway and Lee seek to recreate the Dobe people in the *alter* image of capitalist society, contending that Bushmen are not subservient because the "'serfs' [frequently] disappear . . . leaving their 'masters' high and dry."

Given the sorry state of global human rights, I would argue that oppression of foragers by "us" is a critical

issue that we have consistently overlooked as anthropologists. Given that the colonial Germans referred to Bushmen as vagabonds while they applied their genocidal policies to them and that the Nazis were concerned with wandering Jews and Gypsies, I would submit that the way in which the state defines and treats its people of no ostensible fixed address is crucial for understanding genocide and other forms of structural violence.

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Solway and Lee present a convincing, well-argued, and well-substantiated response to those who for the last decade or so have been challenging the premise that post-Holocene hunter-gatherers are "aboriginals." The "revisionists" find this premise too static and argue instead that such peoples are to be regarded as "marginals," interdependent with neighbouring agro-pastoralists and linked through them to a wider, ultimately universal world system. This argument has been presented with increasing forcefulness and buttressed by a growing body of archaeological, ethnohistorical, oral-traditional, and ethnographic evidence. Though the debate has been especially vigorous in the context of the Kalahari hunter-gatherers, it has been extended to other foraging groups as well, for instance, those of the Malaysian tropical forest (Headland and Reid 1989).

I have been sympathetic to the revisionist position in some of my own work on the Kalahari Bushmen (most recently in the context of oral traditions, where I have treated certain legend-like narratives as mythologized accounts of possibly quite early contacts with agro-pastoralists [Guenther 1989:152-59]). I was drawn to this perspective in the mid-seventies through the careful, scholarly study of the historian Richard Elphick (1977), who demonstrated one of the elements of the "interdependent" position—early Soaqua hunter and Khoikhoi herder interaction and interdependence—in his work on 17th-century Cape Khoisan peoples and the first generations of Dutch settlers. Since the archaeologists and anthropologists have appropriated it as an analytical device, however, I have been troubled by the stridency of their discourse and their claim for explanatory monopoly.

I find Solway and Lee's basic point eminently reasonable—that in some regions of the Kalahari and at some times in the history of its various hunting-and-gathering inhabitants people were relatively isolated and autonomous, while in other regions and at other times they were relatively incorporated and dependent. It is a point that is in accord with the fact that these hunter-gatherers frequently and typically live, as do so many others, in marginal lands, that is, at the "deep periphery"—the periphery of the periphery—of agro-pastoral, core-and world-connected neighbours. Given this ultra-peripheral condition, which is tantamount to isolation,

one would expect them to maintain much of their aboriginal autonomy.

In addition to assembling an impressive body of archaeological, ethnohistoric, and ethnographic facts in support of their case against the hasty and overzealous application of dependency and world-system theory to hunter-gatherers, Solway and Lee criticize the discourse within this body of theory. They stress the need to define more clearly, and render less Eurocentric, such basic categories as "autonomy," "dependence," "independence," and "integration" and point to the often subjective, uncritical, and exaggerated ethnographic renderings of the dependence relations of Bushman hunter-gatherers and Bantu-speaking agro-pastoralists.

The one problem with this important article, beyond the control of its authors or editor, is the timing of its release. One of the authors is, in the view of the revisionists, the principal representative of the position they oppose. Their principal spokesman, in turn, is Edwin Wilmsen, who has just published what is probably the most thorough and well-considered expression of the revisionist point of view (Wilmsen 1989*b*). The evidence and arguments presented by Solway and Lee might have gained even greater strength and authority had they engaged their major critic on the basis of his major work. In any case, this cogently argued rebuttal places the ball, for the time being, in the revisionists' court. What with Wilmsen's book and the forthcoming session on the Kalahari forager-pastoralist debate at the 1989 American Anthropological Association annual meeting, there is the stuff here for yet another CA article. This debate deals with one of the central issues, and certainly the most controversial of these issues, in hunter-gatherer studies today.

HENRY HARPENDING AND PATRICIA DRAPER

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We have very little to say about the larger issues to which this paper is addressed. We find the discussion here agreeable, and we find the papers of the revisionists agreeable, but the underlying debate is like shadow without substance. Of course the San are not timeless relics, but it is equally absurd to say that practices like food sharing represent a subliminal consensus about how to avoid outside domination. The !Kung that we know share meat because it would rot if they didn't and, by sharing, they obtain meat in return the next week. They are less enthusiastic about sharing nuts, which don't rot.

The real argument seems to be about the correct image to be projected, or about what anthropologists should tell science writers. Like the devil with a bible, anyone is free to take the data and generate whatever image is desired. There is a certain unwholesome urge among the revisionists to beat up a pristinist, and Lee is often the one chosen. But Lee, in his dissertation and later publications, depicted the Herero presence in the Dobe region in scrupulous detail. Harpending has recently been

working with Herero in the region, and everything that he has found confirms Lee's accounts of the history of !Kung-Herero interaction.

Evidence is lacking for the assertion, at least regarding the Herero, that "without the availability of a reservoir of San labour to replace absentee blacks the Kgalagadi and Herero could not have enjoyed the prosperity they experienced in the 1960s and 1970s." In the 1960s it was common for one or more !Kung families to live in a satellite village attached to a Herero village. In the late 1980s this pattern is unusual; most !Kung live in their own villages. Some but not all of these villages include Bantu-owned *mafisa* cattle along with other cattle owned by !Kung. The Herero seemed to be relieved by this withdrawal—Herero informants felt, probably correctly, that consumption by !Kung dependents outweighed production by !Kung labor. We have no systematic data on the point, but we could not identify anything like a labor shortage among Herero, even in the face of !Kung withdrawal into their own settlements and into Namibia and in the face of the absence of their own Herero children and young adults at primary and secondary school.

There is very little herd management in this part of the Kalahari, in strong contrast to practices among East African pastoralists. Typically in East Africa boys and young men actively herd cattle during the day. Herero must work at lifting water from shallow limestone wells for their cattle during the dry season, but otherwise the cattle are on their own to find forage. Stock return to their home kraals on *their own initiative* in the evening. Gibson (1962) pointed out that a low proportion of Herero men went to South Africa to work in the mines. Today, a scan of men aged 20–40 in our Herero database (Renée Pennington, work in progress), ascertained through reproductive histories of women, shows that 9 of 245 are working in Namibia and 3 in Botswana away from traditional Herero areas. These amount to fewer than 5% of the men in this age-group. Herero disinclination to migrate elsewhere for employment and the modest work requirements of herding suggest that there has not been strong demand for !Kung labor.

Solway and Lee are correct in their complaint that the revisionist literature blurs the distinctions among the various San peoples. The model of Elphick, echoed by Schrire, fits what is known about the people of the Central Kalahari, who speak a Central Bush language. There is not a hint of that kind of alternation between foraging and pastoralism in !Kung ethnography and history collected from old informants. On the other hand, it is perhaps misleading for Solway and Lee to focus so much discussion on the region that is shaded on their map. This happens to be an area (why is it round?) that includes where the Marshalls worked in the 1950s and where Lee and others worked in the 1960s and later. But there are as many !Kung at Ghanzi and between Ghanzi and Rietfontein, to the south, and there are a lot of !Kung to the north, extending into Angola. Referring to their map, the distance between Dobe and Tshumkhwe corresponds to a day's trip, and visiting between the two

places was and is constant and unremarkable. !Kung in Ghanzi often had visitors from /Xai/Xai and intermediate points when we worked there in the 1960s. If the ancestors of !Kung used space as the !Kung do today, then the "influence" of any Iron Age people even hundreds of kilometers away would have been felt daily or weekly.

Harpending asked several hundred Herero and Mbanderu old people in northwestern Botswana (the region north of Ghanzi and west of the Okavango delta on the map) about the history of contact with !Kung. Only one person reported that her family was in !Kung territory, between Tshumkhwe and /Gaam, at the time of the massacre in 1904; the rest said that their families were far west of the !Kung. Herero informants reported that their parents had had Khoisan retainers, but they all said that these retainers were Bergdama and not !Kung. (The term that we gloss as "retainer" could reasonably be translated as anything between "part-time employee" and "slave." This is a rather fundamental problem that casts doubt, in our opinion, on accounts by 19th-century travelers about relations between San and Bantu.)

In 1988 Draper collected life histories from 13 elderly !Kung, ranging in age from the 60s to the 90s. Twelve of them reported having spent their early childhood in the bush, without contact with Bantu. They remember having joined Bantu villages in their later childhood or young adulthood. The single informant whose earliest memories included Bantu villages and cattle, a woman of 65 years, was born at !Goshi, the westernmost water source in the Xangua Valley. Her account agrees with Solway and Lee's, identifying an early Tswana cattle post at !Goshi. The remaining 12 informants were born farther west, mostly in Namibia, and their first memories of contact are of Herero rather than of Tswana. The arrival of a few Herero during the 1920s and 1930s did not trigger a !Kung exodus from the bush. Most !Kung continued to live by hunting and gathering while maintaining trade relations with settled !Kung.

Biological evidence about the history of San-Bantu contact should be useful, and we have two mutually contradictory sorts of biological evidence to offer. First, the !Kung are well known for their low fertility (Howell 1979), but infertility is even worse among the Herero and Mbanderu (Harpending and Pennington 1989), with mean completed family sizes of older women of about 3.5. Low fertility in both these populations is unremarkable since they are adjacent to regions of high infertility in Angola and Zambia already identified by demographers (Frank 1983). Harpending and Draper (n.d.) report that the completed family size of old !Kung women in the late 1980s is under 4, the same level as reported by Howell (1979) and by Harpending and Wandsnider (1982) in the 1960s. Further, Draper asked the oldest !Kung informants about the fertility of their own parents. Using a new indirect technique for estimating the parity distribution of parents of a cohort, Harpending and Draper found the same distribution of completed family sizes among parents of the oldest !Kung, with a mean slightly under 4. All this suggests that low fertility has

afflicted the !Kung for a long time. This has been attributed to mobility, feeding practices, long lactation, low body fat, and other ecological factors. Another possibility is that the !Kung suffer from the same infectious and presumably venereal infertility as their neighbors. The presence of this disorder among the oldest !Kung for whom reasonable estimates could be generated suggests that biological contact between !Kung and Herero is old in the region.

The second line of evidence comes from mitochondrial DNA, genetic material that is transmitted from mother to daughter but not from father to offspring. Vigilant et al. (n.d.) find that mitochondrial DNA in a sample of !Kung implies a branching history of our species in which !Kung occupy one branch and the rest of humanity another. A good world sample is not in yet, and our finding may not hold up, but it does show that the !Kung are truly genetically different from their neighbors. At the level of genetic material they do not look like members of an old, established regional social hierarchy, in the sense that !Kung do not include descendants of downwardly mobile Herero or other members of "higher" classes. The mitochondrial work is concordant with evidence from nuclear markers (Harpending and Jenkins 1973).

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Solway and Lee shed light on a number of important issues in what might be termed the "Kalahari forager-agropastoralist debate." The conclusions that they reach concerning the range of variation in San adaptations in the Kalahari and the differential effects of contact and interethnic interaction are quite important.

The ethnohistorical and ethnographic research conducted in the central portion of southern Africa over the past several decades supports the argument that some San groups were incorporated into patron-client relationships while others chose to forage for a living and still others oscillated back and forth between foraging and food production, depending upon environmental, economic, and sociopolitical conditions (Vierich 1981, 1982*b*; Hitchcock 1978, 1987). There is also evidence that San were involved in wage labor both in the mines and on farms of southern Africa. Virtually every anthropologist who has worked among San has taken note of the fact that they exhibit a wide array of adaptive strategies. These patterns of socioeconomic variation existed at the time contacts between San and Europeans occurred in the 19th century as well. On the one hand, statements are made to the effect that San "have a great aversion to agriculture and cattle breeding" (Holub 1881:349) and that they are not "able to accustom themselves to a settled dwelling place" (Mohr 1876:156), while, on the other, travellers point out that some San kept domestic animals and raised crops (Chapman 1971 [1868], vol. 1:154; Oates 1881:178-80).

It is not unlikely, as Schrire (1980) has argued, that at least some San were pastoralists at some point in the past who lost their herds. Indeed, there is both ethnohistorical and ethnographic evidence to support this suggestion. James Chapman, travelling through the eastern Kalahari in August 1853, was told by the San there that their forefathers had owned cattle but had lost them as a result of extreme cold (Chapman 1971 [1868], vol. 1:109). San in the northeastern and southeastern Kalahari told me that they had lost their cattle as a result of droughts in 1933 and the 1960s.

There is no question that most San today want to have livestock of their own. Some people are getting them, through purchase, payment for labor, gifts, inheritance, or government livestock loan schemes. At the same time, it is important to note that very few Kalahari San households have sufficient numbers of animals to be considered self-reliant pastoralists. There are ecological, economic, and social constraints that must be overcome in order for San to get and maintain access to livestock. Ecological constraints include low-quality grazing, lack of water, predators, disease, and poisonous plants (e.g., mogau, *Dichapetalum cymosum*). Economic constraints include getting sufficient funds to purchase stock; the wages of herders are low (averaging less than \$5-10 per month) and paid irregularly. Even in situations in which San get an animal a year as payment for their services, they are often given males. Many Tswana will not sell cattle to San, saying, "What good is a cow to these people? All they will do is kill and eat it." San livestock is sometimes confiscated by other cattle owners who claim that they have been responsible for stock theft or for the loss of animals they were supposed to be watching over. Sometimes government officials (e.g., Department of Veterinary Services staff) take San animals away on the pretext that they are being kept too close to veterinary cordon fences and as such are a threat in terms of the spread of hoof-and-mouth disease. San livestock owners must also contend with pressure to give them to other individuals within their own groups or to kill them and distribute the meat among group members.

Archaeologists have traditionally held that people living closest to food producers (agriculturalists or pastoralists) will have the greatest degree of dependence on crops or livestock. Data on Kalahari San food production reveals, however, that this is not the case. Those San living on the peripheries of villages, for example, tend to be less involved in their own crop and domestic livestock production than those who live in remote areas. Part of the reason for this is that the people near villages often have alternative means of making a living, such as working for others. In addition, cattle damage is a serious problem for people trying to raise crops.

There are several points in Solway and Lee's paper which need clarification. First of all, the argument that the central Kalahari was, by law, free of large-scale village and livestock development is true only in a relative sense. These is oral-history evidence that suggests that San kept livestock in the central Kalahari in the 19th



century, and today, in spite of the laws, there are nearly 3,000 goats, 275 donkeys, and 60 horses in the Central Kalahari Game Reserve (Hitchcock 1988:A4-76). Secondly, although archaeological investigations have yet to be carried out in the western Kweneng District, the presence of Early Iron Age ceramics on pans and in fossil river valleys in other parts of the Kweneng, in the central Kalahari, and in the Ngwaketse District to the south suggests that agropastoralists may well have visited the area on occasion well before the end of the 1st millennium.

The use of the term *mafisa* to describe one type of relationship between San and Tswana also needs to be examined. The tradition of giving cattle to other people to manage in exchange for their being able to use the products of those animals (e.g., milk, draft power) is well-established in Botswana. The custom of making long-term loans of cattle, known as *go fisa* in Setswana, serves several purposes. It helps cattle owners to spread their animals around and thus reduces risk; it enables them to cut down their herd sizes, thus facilitating management; it provides a means of hiding wealth from other people, including tax collectors and jealous relatives; and it is a means of creating alliances and currying favor. The majority of *mafisa* relationships involve people of the same ethnic group, even sometimes the same family. Judging from interviews in several parts of the Kalahari, it is relatively rare for San to be given *mafisa* animals. Cattle owners usually lend livestock to people who already have some cattle of their own, something that is true for only a minority of San. Also, Herero, Tswana, and Mbukushu cattle owners often state that they consider San bad risks and therefore are reluctant to lend them their cattle. In the eastern Kalahari, less than 1% of the 666 households on which data were collected had *mafisa* animals (Hitchcock 1978:298-99). Even though San sometimes refer to the cattle they are watching over as *mafisa*, this may suggest a more institutionalized kind of interaction than actually exists.

Finally, while it is true that some San could withdraw from their involvement with Tswana and other agropastoralists, they often did so under the threat of punishment. There are numerous references in the ethnohistoric literature and in government reports to the beatings that San received for leaving their "masters," and in some cases people were killed (Tagart 1933, Joyce 1938). The options of changing jobs or of returning to mobile foraging were certainly available, but they involved a fair degree of risk in a complex social and natural environment.

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Though I am not qualified to comment on the specific historical and ethnographic material that Solway and Lee adduce in support of their argument, I am much in

sympathy with their general point of view. My comment focuses on the theoretical issues raised in the final pages of the paper and is more a suggestion than a criticism. I agree that debates about hunter-gatherers have often paid inadequate attention to the precise meanings of key terms such as "autonomy" and "dependency" (though see Ingold 1986a: chap. 9; Myers 1988), but surely the most crucial and neglected term of all is "society." In treating both autonomy and dependency as possible conditions of a *society*, in its relation to other societies, Solway and Lee do not question the applicability to hunter-gatherers of the concept of society itself. Yet I would maintain that this concept is part and parcel of what they would call "agrarian discourse," in which "structures of domination are taken as given." Society, as Levine and Levine (1975:177) put it, "is domination" (see Ingold 1986b:258 for a discussion of this idea).

In characterizing the social life of hunters and gatherers, what we are really attempting to get at is a certain *quality of relatedness*, both among people and between people and their environments. In other words, our aim is to discover the properties of hunter-gatherer *sociality*. Thus, when we examine the relations between hunter-gatherers and their agrarian or pastoral neighbours, we are really concerned with the articulation between one kind of relatedness (or sociality) and another rather than between one kind of society and another. It is only within the framework of non-hunter-gatherer (or, let us say, "agrarian") sociality that hunter-gatherers are seen as existing in "societies." The concept of society carves the world of human beings into mutually exclusive blocks in much the same way as the agrarian concept of territory carves up the country they inhabit. If the latter implies a relation of domination and control over the land, the former implies a relation of domination over people. Hunter-gatherers exist in "societies" for those who would seek to exert their control over them but not for the hunter-gatherers themselves. Their world is not a socially segmented one, for it is constituted by relations of incorporation rather than exclusion, by virtue of which others are "drawn in" and not "parcelled out."

The essential difference between hunter-gatherer and agrarian sociality, I believe, is that whereas the latter is based on domination the former is based on *trust* (see Gambetta 1988 for some exemplary discussion of this concept). Both domination and trust imply dependency, but trust implies acceptance rather than denial of the autonomy of the other on whom one depends. That is why personal autonomy is such a fundamental feature of hunter-gatherer social relations. The peculiar difficulty in characterizing the relations between San and non-San neighbours, which is the central problem of Solway and Lee's contribution, may lie precisely in the fact that on the one side they are framed within a discourse of domination epitomized by such notions as slavery, serfdom, and clientship whilst on the other they are framed within the discourse of autonomy and trust. The actual conduct of these relations seems to be an enactment of the contradictions between the two discourses and to be



marked as much by mutual puzzlement as by common understanding.

Solway and Lee are right to criticize one-sided views that can conceive of the relation between hunter-gatherers and their neighbours only in terms of the domination of the former by the latter. And they are right to point out that according to such views hunter-gatherers can have no history save a "reactive" one—a history of response to oppression. But if we are to accord to hunter-gatherers a history that is truly their own, the subject of that history cannot be "society," nor can it be a record of "social change." As I have argued, the very notion of "society" locks the people into an externally imposed frame that is structured by relations of domination and subordination. So long as history is equated with social change, its motor will be found in the dynamics of these relations. The real challenge of hunter-gatherer studies is to develop a conceptual vocabulary that will enable us to capture the dynamic potentials of a radically alternative mode of relatedness. The work of meeting this challenge has scarcely begun.

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Solway and Lee have made a valiant attempt to show that the "revisionists" have overreacted to the original work of the "forager school" (Lee, Tanaka, et al.), and although I think that both sides have made relevant points the final word has yet to be said on this issue.

The occupation of the Kalahari by Iron Age people was only sporadic between A.D. 600 and 1100. Many of these occupations could simply have been winter grazing camps such as those of Kavango River communities (Jacobson 1987a), which were possibly extended over several seasons by favourable climatic conditions. Subsequent to this there appears to have been no settlement.

Although the Iron Age had been established since A.D. 500 and pastoralists in central Namibia since at least 30 B.C. (Jacobson 1987b), it is simply wrong to assume that the presence of pottery on an archaeological site means that the band living there was directly in contact with farmers or pastoralists (Musonda 1987). Small numbers of potsherds (or other exotic items such as metal) could quite easily have been the result of trade or *hxaro* exchange (Wiessner 1977) in much the same way as the isolated examples of seashells that are often found inland (Wendt 1980). The archaeological data therefore need to be critically evaluated.

The creation of a class of serfs or an underclass results from the demand for land and labour. This is the dominant theme of southern African history for the last 300 years, and it serves equally well to explain the pre-European past. The environment in which foragers lived (i.e., Dobe and southwards) inhibited permanent settlement by food producers, thus resulting in no direct demand for land, which, had it been at all suitable for food production, would have been permanently settled, if not

in prehistory then in the historical past. As Solway and Lee point out, foragers came and went at will, whilst outsiders stayed only intermittently.

It is always overlooked that a similar spatial dichotomy existed in southern Africa between farmers and pastoralists, also as a result of environmental conditions. Areas suitable for farming were settled by communities with social, political, and settlement patterns very different to those of communities in areas suitable only for a nomadic pastoral way of life, although there was trade and intermarriage between them (Hall 1987). There is also the question here of whether autonomous groups of forager bands were found living in what for the food producers were unsuitable areas and their degree of acculturation relative to their "advanced" neighbours. This problem vexes South African archaeologists, and it is for this reason that the models developed for the Kalahari are so important.

Obviously, one is not dealing with "pristine" foragers who could serve as direct models for, say, the Pleistocene. They may have been involved in sub-continental trade or exchange networks, but it is certainly wrong to consider them all as having been subordinated to a dominant mode of production. The social reality of contact between modes was as variable as the land, the motive force behind prehistoric economies.

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Solway and Lee's important article raises issues that reach far beyond research in the Kalahari. It is therefore a valuable contribution not only to hunter-gatherer studies but to all of anthropology. The "revisionists" are not entirely wrong in some of their recent assertions about the Basarwa (or San)<sup>1</sup> any more than Lee was wrong in his characterization of Dobe in the early 1960s. As Solway and Lee mention, historically there have been Basarwa who formed a disenfranchised underclass. A point missed by the "revisionists," however, is that at the same time there were primarily hunter-gatherers. The Basarwa have been robbed not only of their history, as pointed out by Solway and Lee, but of their diversity. The economic diversity among Basarwa groups can be seen by contrasting those of Dutlwe with those of nearby Kutse.<sup>2</sup> Kutse Basarwa are full-time hunter-

1. Both the term "Bushman" and the term "San" are derogatory words, and, although far from perfect, the term "Basarwa" is more neutral, as well as being the one preferred by the government of Botswana (e.g., see Cashdan 1983:64; Silberbauer 1989). The controversy over terms seems to me to show the sensitivity and concern of the researchers for the people they study and the lengths to which anthropologists will go to insure the dignity of those with whom they work.

2. I use the spelling currently preferred by the government. I am most grateful to the government of Botswana for granting permission to work in the Kalahari, to the people of Kutse for their hospitality, and to the Fulbright Foundation and Old Dominion University Research Office for funding.

gatherers who are not locked into any patron-client relationship with Bantu people. Government-distributed mealie-meal flour and a limited amount of cultivated melons supplement gathering activities but have by no means replaced them. Many Basarwa own a few goats, but 90–100% of their meat comes from wild game. Thus, within the relatively short distance that separates Dntlwe and Kutse there are considerable differences. The fact that the Dntlwe Basarwa are intimately tied into Bakgalagadi agropastoralism has no bearing on the fact that Kutse Basarwa are primarily (though not exclusively) hunter-gatherers. Other Basarwa adaptations have been documented by Hitchcock (1982) for the Nata River and Cashdan (1979) for the Botletli River. This diversity should not be surprising to anthropologists working in the area, since one of the few consistent pan-Basarwa traits is a general flexibility of culture (e.g., Barnard 1988b on kinship; Vierich 1981, 1982 on subsistence; Kent and Vierich 1989 on mobility and spatial patterning). Instead of ignoring diversity or viewing it as a challenge to one's own view of a group, one should attempt to explain it. I believe that much of the diversity that we observe may be the result of different patterns of mobility. There are groups that have been sedentary for hundreds of years and groups that have been so for less than a decade, and the amount of Bantu influence seems to me greater in those that are more sedentary (Kent 1989b).

Considering what was going on in Namibia and South Africa during this period and the enormous pressures to change, it would be much more difficult to explain why the !Kung had remained the same in the period between Lee's initial fieldwork and later studies (e.g., Biesele et al. 1989). Finally, one must demonstrate that cultural assimilation accompanied trade as is assumed by some "revisionists." That trade does not automatically mean assimilation is obvious from the fact that most Americans own Japanese-made televisions, cars, VCRs, and other electronic products but few would consider themselves much influenced by Japanese culture.

It is paradoxical that anthropologists, dedicated to the study of diversity, should so often conclude that other anthropologists are wrong when they report that "their" group is different. I hope that Solway and Lee's paper and the discussion it generates will demonstrate that no one has been wrong in the characterization of any one particular group of Basarwa and that there is much to learn from the differences among them.

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The significance of this essay lies in its effort to go beyond the sterile debate over opposing models, "isolate" vs. "interactive," that has dominated the study of the San and other hunter-gatherers for the last decade (cf. Headland and Reid 1989), in its attempt to examine the

particular historical circumstances of two San groups, and, most significant, in its opening up of new research questions. The issue, Solway and Lee insist, is not whether the Kalahari San lived in splended "isolation"—"we take it as given that all societies are involved in economic exchanges and political relations with their neighbours"—but rather the actual history of the contact situation, "treating the impact of trade as problematic rather than as given." I am in complete agreement with this view. My critical comments are directed at the level of success of these efforts.

While stating that the nature of contact is the subject of their study, Solway and Lee frame the question solely in terms of whether the San retained their "economic autonomy" vis-à-vis their neighbours in each historical period. As a result, we are left with a very shallow understanding of San history and society. For example, we learn that during the last three decades of the last century the San experienced direct and disruptive European trade in the course of which wildlife was depleted, guns were widely distributed, miscegenation occurred, and the Dobe area became the nexus of long-distance cattle-trek routes. The three-decade-long period is remembered by the San as "a time of intense social activity and economic prosperity." Yet in their effort to cast this generation-long era of "intense irruption" only in terms of whether it entailed a "basic restructuring of relations of production," the authors do not explore the multiple social, political, and even ecological implications of such interaction and dismiss it as short-lived.

What is a "basic" restructuring of relations of production? In what way was this new source of wealth, employment opportunities, miscegenation, and movement of people and cattle along established routes not "basic"? Were other animals besides elephants diminished by the European hunters and gun-carrying San? What did such possession of guns entail in terms of the daily diet of people (fertility rates?), their ability to defend themselves, inter-group power struggles, the rise of richer/more powerful individuals in the community, or the ethic of sharing? Were cattle transported along these trek routes exchanged for local services? How was this intensive three-decade period, which could be described by San almost a century later, represented in the evolving mythology and symbolic order of the people who experienced it?

Such questions address the issue of *how* rather than *whether* such changing historical circumstances and experiences of contact transformed San society. The issue, in other words, is not whether 30 years of intensive contact with white hunters and others dramatically destroyed existing relations of production among the San (a formulation that grants these nexuses of relations permanency and internal stability) but what were the mechanisms (social, cultural, political, and economic) that reproduced or transformed a given social reality at that particular time.

Again, I agree with Solway and Lee that interchanges do not necessarily destroy an existing way of life. Peo-

ple's choice, and capacity, to reproduce their social and cultural patterns in the face of forces (internal and external) impinging on their lives and the transformation of those social patterns are both active historical processes that call for analysis. To use Solway and Lee's own phrase, one must "do justice to the complexity and fluidity of the situation."

Another set of problems emerges from the definition of "autonomy" and thereby the nature of the historical process only in economic and ecological terms. Even if one has no quibbles with the authors' criteria for "self-sufficiency," one is hard-pressed to see the significance of the distinction between the "autonomous" residents of the single camp in Dobe (among the nine encountered by Lee and other researchers in the '60s) said to have lived mainly by hunting and gathering and their "employed relatives" ten kilometers away. Is the mere fact that people *could* leave their employers and retreat to the bush sufficient to explain the emerging social, cultural, and political patterns that such prolonged interchange entails? What about those women married to Herero men? Where did they and their children belong? Was access to *mafisa* cattle only an issue of economic concern, or did it entail the emergence of internal differentiation, a restructuring of political relations? Clearly, Solway and Lee are aware of the limitations of their economic view, and their more specific discussion of the *mafisa* system and the nature of San "serfdom" is not contained within this narrow framework. One can only hope that this essay will stimulate a new and more sophisticated discussion of the San social formation, a discussion that has too long remained under the shadow of ecological and economic models.

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Solway and Lee raise several contentious issues. First, they agree with studies that are critical of the depiction of "hunter-forager" societies as isolated and timeless and that advocate situating them in wider historical, social, and political-economic contexts. They argue that these contexts and the structured connections they manifest are socially constructed and historically specific. Thus, hunter-foragers insert themselves or are integrated into these contexts in diverse ways that vary from one concrete situation to another; furthermore, contact does not automatically entail the domination and exploitation of peoples that practice hunter-forager modes of existence. At issue is how to conceptualize these instances of articulation in theoretical terms that simultaneously inform both subsequent research and praxis.

Second, they object to the tendency to represent hunter-foragers as having a distinct social order that sets them apart conceptually from "nomadic herders" or "sedentary peasants." Such representations originate in

the assumptions of simple economic determination and linear progress that have provided the underpinnings for virtually all cultural evolutionist arguments since the late 18th century (Meek 1976). As many have noted, hunter-forager societies exhibit diverse social and economic forms; they vary from highly mobile to quite sedentary, from broad-based subsistence production to highly specialized economic activity, and from relatively egalitarian social relations to chieftainships. The diversity of their sociopolitical forms does not mirror precisely the variability of either the forces or means of production or the way in which labor processes are organized. In fact, the same range of sociopolitical forms occurs among nomadic herders and farming communities, as recent discussions about the evolution of cultural complexity make abundantly clear. This situation directs attention toward the Marxist analytical concept of a mode of production; it raises questions about the significance of diverse forms of primitive communism or the kin-based, communal mode of production (Marx 1965 [1857-58], 1983).

The third is whether human groups, regardless of their social relations of production, can establish and maintain social relations that do not involve domination and exploitation. Solway and Lee argue that under some conditions they can and have; they indicate that relations of domination and subordination are not natural to humanity but socially constituted in certain kinds of society under historically specific conditions. The opposing view is that social relations that do not involve domination and exploitation are rare at best and difficult to sustain in any event; consequently, domination and exploitation will ultimately emerge and prevail in every society and interaction, whether they are natural or social constructions. The question is whether "the will to power" is an unavoidable and ineradicable feature of human existence and, hence, a characteristic of all social groups or whether it is typical only of the dominant segments of those societies shaped by diverse forms of class struggle. Thus, once again, the claims of Hobbes, Nietzsche, and Weber are pitted against those of Marx and Engels.

Finally, they challenge the denial of the primitive and the counterclaim that all societies are essentially the same. With Diamond (1974) and others, they see the primitive as a projection from and a commentary on civilization that tells us what we are not and what we could be. These critiques tell us that the quality of everyday life in other societies is different and that societies change. Thus, they afford a glimmer of hope in today's dangerous, fragmented, and crisis-ridden world; in the long run, these images may be anthropology's most significant contribution to humanity. However, in contrast to postmodern commentators, Solway and Lee recognize that primitive societies exist in real historical contexts textured by particular political, economic, and social relations and by culture. They argue that power relations, if they exist, need to be addressed, not ignored or treated as part of human nature.

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Ten years ago, smothered in rejections of my first revisionist critique of the San literature (Schrire 1980), I turned for advice to Nancy Howell of the Harvard Kalahari Research Group. She suggested that it was worth, at most, a short note to the editor of CURRENT ANTHROPOLOGY. Since Meyer Fortes had urged me to write a book instead of an article, I persisted, and then watched a substantial body of revisionist work emerge, the most gratifying of which is perhaps Howell's (1988) still unpublished piece on the Tasaday and the San (see Bower 1989, Lewin 1988). Howell, who spent 22 months in the Kalahari, now insists that the group's depiction of San isolation was contrived by consciously and unconsciously ignoring such !Kung as failed to conform to expectations "because we didn't come half the way around the world to see . . . people behaving as a rural proletariat" (1988:7). Strong words these, yet the revisionist opinions, and indeed, Howell's too, have until now gone apparently unnoticed by critics such as Lee. Here, at last, he speaks, only to construe a decade of revisionist scholarship as the work of neocolonialists who have created a uniformity that renders the San "degraded cultural residuals" living under the domination of invaders, with no history of their own.

These accusations are unworthy of our scholarship and of his. Where is domination in the oft-quoted words of one of our number: "The old notion of these people as passive victims of European invasion and Bantu expansion is challenged. Instead of toppling from foraging to begging, they emerge as hotshot traders in the mercantile world market for ivory and skins. Rather than being victims of pastoralists and traders . . . they appear as willing agents, . . . brokers . . . and hired shots" (Gordon 1984:196)? Accusations that we imposed uniformity on the San conflict with our studies of particular archaeological sites and of specific court records and documents.

Solway and Lee's case rests on the premises that Dobe is different, isolated geographically by a dry ring, and that there is *no archaeological evidence of Iron Age occupation of the area until the 20th century*. Both premises are false. First, geomorphologists point to changes in climate in the Kalahari, and none has established the invariance of the barrier around Dobe (see Shaw and Cook 1986). Furthermore, even if the barrier existed, it could be crossed in a couple of days' trek by foragers carrying water in ostrich eggshells. Secondly, although evidence of an Iron Age presence below the ground at Dobe is cited by Solway and Lee, it is interpreted as evidence of trade rather than occupation. The distinction rests on the number of sherds present and the position of one bone. Reserving judgment here might be acceptable if this were the only Iron Age site in the Kalahari, but it is not. It is one of 34 sites in Botswana, and its significance lies not in its particular count of potsherds and bones but in the fact that it duplicates a

pattern of irrefutable archaeological evidence for the association of early Iron Age and Late Stone Age elements throughout the Kalahari (Denbow and Wilmsen 1986) that suggests extensive movement of people, artefacts, and food through trade, marriage, barter, theft, or plain good fortune. It is this pattern that belies the uniqueness of the Dobe case and that illustrates, in its repetition and consistency, how much more reliably archaeology reveals the past than do any number of ancient !Kung elders.

I am not mollified by Solway and Lee's closing words equating those who would relegate hunter-gatherers to splendid isolation with those who would integrate them into a wider capitalist world. Evidence, hard field and archival evidence, denies the splendid isolation of the Dobe !Kung and confirms that they were integrated into the wider world some considerable time ago, all the "liberal" rhetoric in the world notwithstanding.

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Views about the nature of modern hunter-gatherer societies have reflected the changing orientations of anthropological theory and altering relations between prehistoric archaeology and ethnology over the past 200 years. Periods marked by a strong faith in the regularity of human behavior and in rationality as its primary determinant have been characterized by the belief that modern hunter-gatherer societies provide living examples of Paleolithic life. This view was embodied in the "theoretic history" of the Enlightenment, the unilinear evolutionism of the 1860s and 1870s, and the neo-evolutionism of the 1960s (the period which also saw the publication of *Man the Hunter* [Lee and DeVore 1968]). In intervening periods anthropologists have been preoccupied with the diversity, particularity, and contingency of human behavior, as a result of which they have had serious doubts about the extent to which modern hunter-gatherers accurately represent the nature of pristine societies. In the early part of the 20th century diffusion was seen as a complicating factor; today, as Solway and Lee point out, dependency and world-system theories play a similar role. The influence of the prevailing particularism is evident in the work of the leading archaeological thinkers of the 1930s and 1940s. Even when espousing an evolutionary point of view Childe (1942) argued that societies that had remained hunter-gatherers must from the beginning have had a different, and more conservative, superstructure from those that evolved into more complex ones, while Clark (1939) consistently preferred direct historical analogies based on European folklore over cross-cultural ones for interpreting European prehistory. The diversity of reasons that have been advanced both for supporting and for rejecting the proposition that modern hunter-gatherer societies approximate prehistoric ones reinforces Solway and Lee's con-

tention that powerful ideological considerations are involved in this debate.

My own recent analysis of 17th-century Huron society strongly supports Clastres's (1977) argument that small-scale egalitarian societies have powerful built-in mechanisms that actively oppose the development of social and political inequality. In the case of the Huron, I have been able to show how positive and negative sanctions were combined to pressure individuals to live up to ideals of generosity and individual freedom deeply embedded in Huron ideology and social practice (Trigger 1990a, b). This research has convinced me that Clastres was at least metaphorically correct when he asserted that "the history of peoples without history is the history of their struggle against the state" (1977:185–86). It seems clear that these arguments apply to hunter-gatherer societies as well as to the low-population-density swidden agriculturalists that Clastres and I have been studying.

Because of this, I would agree with Solway and Lee that it is premature for archaeologists to reject the idea that aspects of modern hunter-gatherer societies such as food sharing were also characteristic of prehistoric hunter-gatherer peoples. Contrary to the view of the more extreme dependency theorists, these are forms of behavior that are likely to be reasonably resistant to change. On the other hand, archaeologists may have more reason to be concerned about the impact that contact with non-hunter-gatherers may have had upon the demography and foraging patterns of hunter-gatherers. Binford (1980) has argued that differences in foraging patterns between hunter-gatherers of low and of high latitudes reflect age-old adaptations to these regions. Yet it is unclear to what extent the foraging patterns of subarctic peoples represent pristine adaptations to these environments or have been reshaped in recent centuries by a universal readaptation brought about by trapping for the fur trade and the growing availability of supplies (including foodstuffs) at trading posts. It is also debated whether such changes in subsistence scheduling as may have occurred brought about significant changes in social organization or whether the flexibility inherent in prehistoric boreal-forest subsistence patterns preadapted indigenous societies for the new economy (Francis and Morantz 1983). I believe that there are no a priori answers to these questions and that a definitive understanding of the nature of late prehistoric foraging patterns in the boreal forest and of the changes that may have been brought about by the fur trade depends on archaeological data. While these answers may not be quickly forthcoming, archaeology has a demonstrated ability to provide information about prehistoric settlement and subsistence patterns and is slowly developing a body of middle-range theory that permits statements to be made about relative degrees of economic equality in prehistoric societies. What we know about small-scale societies suggests that archaeologists would be unwise to indulge in radical revisionism for its own sake. But it is equally obvious that dependency theory's challenge to the significance of ethnographic data collected in modern societies provides archaeology with a new

and vitally important role in explaining cultural variation. Ethnographic data have ceased to have an ensured significance that is independent of their historical context. By working together, archaeology and ethnology may eventually not only provide a better understanding of human history but also gain a sounder understanding of the general nature of human behavior.

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Solway and Lee's paper is a most interesting and valuable contribution to studies of the history of the San. In accounts of early travellers and explorers, the differences in life-styles of San living in various regions of southern Africa and in their relations with neighboring groups are striking. The approach taken by Solway and Lee is an important step toward explaining some of these differences. Here I would like to add some evidence from the structure and nature of San exchange relations that supports the view they present of the !Kung of the Dobe area—that these !Kung maintained a considerable degree of autonomy until recent years, although they were fully engaged in interactions with surrounding agropastoralists and other groups of San.

As I have shown (Wiessner 1981, 1982, 1986), the !Kung of the Dobe region were part of a broad regional network of delayed reciprocal exchange called *hxaro*. The *hxaro* network provided flexibility in social relations of production and access to land. Two features of the system suggest that it allowed !Kung to tap the resources of surrounding populations while preserving a certain degree of autonomy. The first is that individuals maintained *hxaro* partnerships with people up to 100–200 km away as well as with people closer at hand. These long-distance ties were not necessary to provide alternative residences in the face of environmental failure, and the !Kung themselves say that they maintained them "to keep up with what was going on in other places." They mentioned that, in the past, distant *hxaro* ties had given them access to desired trade goods, such as metal and beads. These goods would either be passed on from partner to partner across the Kalahari or obtained during visits to partners who lived closer to the source. Thus, through *hxaro* exchange, !Kung participated in the broader trade network and yet maintained some freedom to choose their degree of involvement with the groups that produced or controlled desired trade goods.

Secondly, !Kung engaged in *hxaro* relationships with other San only, mostly with other !Kung. Exchange of goods with agropastoralists such as the Tswana, Herero, or "Goba" took the form of more balanced and immediate trade. *Hxaro* relationships allowed !Kung to visit partners in other areas, utilize their resources, and obtain products that were produced or controlled by agropastoralists without incurring obligations to them. For example, !Kung who lived primarily by hunting and

gathering visited partners who worked for agropastoralists and remained with them for days, weeks, or months to drink milk. It is probably for this reason that agropastoralists frequently complained that when they employed one San, 10 or 20 shiftless relatives came along to visit or stay. The impression of shiftlessness came from the fact that, through the *hxaro* system, these visitors could obtain goods indirectly from agropastoralists but remain independent of their control.

*Hxaro* ties were important not only for groups that depended heavily on hunting and gathering but also for individuals or families employed as labourers. Most !Kung who worked in subsistence or wage labour kept up ties with !Kung who were still subsisting primarily from hunting and gathering. These were usually kin in groups in which the labourer had some land claims. Frequent visits and exchange of goods kept claims to land active and allowed labourers to remain within the protective network of !Kung social relations. Should they feel exploited by their employers or simply tired of working, they could easily rejoin autonomous groups, as Solway and Lee mention.

It might be added that there is some intriguing evidence that the *hxaro* network may not have been the only system that united !Kung in a region. In the past, young men were initiated together with those from other areas to create a common bond between men from different places. Furthermore, !Kung were divided into named groups that may have been part of a larger clan system (Lee 1979). Although older !Kung can describe the initiation and named groups, they can no longer tell how they functioned because the importance of these social institutions has dwindled within their lifetimes. However, these institutions probably created ties that permitted further self-sufficiency of !Kung in a region.

In short, the accounts of early travellers can tell us something of the living conditions of San in different areas. Archaeology can provide evidence for interaction and exchange. However, archaeological remains and early reports can only give limited information on the nature of exchange in the past. As Solway and Lee point out, it is the nature of exchange that is important to understand, for "exchange is a fundamental part of human life and appears in all cultural settings." Certainly the structure of both internal and external exchange relations is not the same for all groups of San. Understanding the structure of different San exchange systems and how they functioned within a regional perspective, before environmental degradation made subsistence by hunting and gathering unsustainable, may provide an additional source of information on the degree of self-sufficiency or dependence of San living in different parts of southern Africa.

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I welcome this contribution; we should all be relieved that San-speaking peoples have now been granted his-

tory by one of the foremost deniers of that history. Whereas a cornerstone of Lee's previous work was that San-speaking peoples were isolated (sometimes, semi-isolated—whatever that may be) quintessential foragers, we now read that the status of San peoples is a product "of long interaction between foragers, farmers, and herders," that it may be taken as "given that all societies are involved in economic exchanges and political relations with their neighbours," and specifically that 70% of San peoples at Dobe were engaged "in a mixed economy of foraging, herding, and farming" while all the rest were retainers on cattle posts when Lee worked among them.

Still in January 1989, according to Lee (1989:118), Dobe was "one of the last areas in Africa (if not the world) where people hunt and gather for a living. It was the persistence of this ancient way of life that first attracted" him to that place in 1963. In 1984, he still thought of his work there as "a race against time [to observe] a foraging mode of life during the last decades of its existence" (p. ix). In 1979, "the vast distances of the Kalahari [still shielded Dobe from] African or European contact until recent years" (p. 33); San people there were still "on the threshold of the Neolithic" (1972:342), "stripped of the accretions and complications brought about by agriculture, urbanization, advanced technology, and national and class conflict" (1974:169). From the beginning, Lee (1969:47) wished "to show that the Bushmen exhibit an elementary form of economic life"; this "required a population as isolated and traditionally oriented as possible" (Lee 1965:2).

Why were the San mixed economies, political relations with neighbors, and exchanges among diverse peoples that we are now told are ubiquitous in the region not seen before? Howell (1988), a "revisionist" member of the Harvard Kalahari Project, provides the answer: "We could have stayed near home and seen people behaving as a rural proletariat." Konner and Shostak (1986), also members of that project (not cited as revisionists), say something similar. These workers point out that, like all research, that of the project was guided by a paradigm, and, as paradigms tend to be, this one proved flawed.

Now, under threat of paradigm change, Lee, in good Kuhnian fashion, prefers to tinker with the paradigm rather than face the flaws. Those of us who reject the paradigm and have moved on to a fuller understanding of the position of San-speaking peoples in the social formation of southern Africa are labelled "revisionists" and accused of erecting straw men by equating the internal dynamics of social formations with unequal social relations and projecting "a spurious uniformity on a vast and diverse region." Part of Solway and Lee's error here lies in their use of "social formation" as a synonym for "culture"; they are therefore unable to appreciate that a social formation is forged in the ideological arena of social and property relations whereas "culture" is an intellectual organizing principle of anthropology. If they understood this they would also understand that their "revisionists" are investigating how current Kalahari social and property relations came to be as they are, not how



one "culture" (or its members) resisted or embraced another.

The very idea of Lee scolding others for holding uniformitarian views could be amusing, coming as it does from one who grounded his program in "the uniformitarian approach to evolutionary reconstruction" (1968: 343; 1979:434) and found in the Kalahari "a way of life that was, until 10,000 years ago, a human universal" (1979:1), but it is not. Solway and Lee seriously misrepresent those they cite as "revisionists," none of whom projects uniformities of any kind onto the Kalahari or anywhere else. Howell and Konner and Shostak present autocritiques of the paradigm applied to the specific locale of their (and Lee's) research; they speak of no other Kalahari area but do draw analogies to intellectual approaches brought to bear on peoples thought to be isolated in other parts of the world. Schrire insists on careful attention to the history of individual places; Gordon posits environmental constraints (not all of them valid) on the spread of pastoralism. Denbow and I, both jointly and separately, document ecological, economic, historic, and political diversity during the past two millennia. My recent portrayal of the political economy of the region (1989a) shows how far we are from uniformitarianism and what we gain in knowledge from that distance.

Solway and Lee's misrepresentation of their critics is based in part on misreading of published work. They say, for example, that I acknowledge that Kalahari foraging "provides an extraordinarily rich and meaningful existence for those who practice it." But in the paragraph cited (Wilmsen 1989b), concluding a paper in which I document the centuries-old structure of institutionally congruent social concepts and complementary economic systems of San- and Bantu-speaking peoples in the Kalahari and argue that these systems have persisted in the face of varying political forces through continual modification in relation to each other, I say nothing at all about a rich and meaningful forager existence in the present. Again, they chide Gordon for uncritically conflating all of northern Namibia (including Dobe-Nyae/Nyae), then say that they place Lee's Dobe-Nyae/Nyae oral histories in the context of regional histories encompassing all of central and southern Africa which make no mention of, let alone present data on, Dobe-Nyae/Nyae; in fact Solway and Lee simply reiterate assertions of timeless sameness.

Along with my "revisionist" colleagues, I am convinced that it is necessary to place local histories in regional contexts and applaud efforts to do so. Here, however, we read again that the waterless desert kept Dobe far from the turmoil of 19th-century Africa until 1925—despite the long interaction between foragers and farmers that we are assured was characteristic of the history of the entire region and the averral that all peoples have political relations with their neighbors. It is risky, say Solway and Lee, to assume that contemporary patterns of contact (that old concept that allows us not to think of integrated social formations) were characteristic of all prehistory (and history?), but at the same time they use archaeological materials—at first dismissed as a few

fragments of pottery and iron—to support a picture of continuity during the last couple of millennia, more or less. Contemporary oral-historical accounts are, for them, adequate to reinforce their view of "at least several thousand years." A more realistic picture of forager self-sufficiency may be found in Wilmsen and Durham's (1988) documentation of differential production relations and Hausman and Wilmsen's (1985) account of their consequences for child growth.

But then, what are we to make of a place where some people (San-speakers) participate only indirectly in an economic system for which they are primary producers? Where a San "forager" could just say—after European merchants had gone away and left him unemployed—"Let's go to the Tswana, bring their cattle here and drink their milk"? How does a "forager" know how to care (in both senses) for cattle? What sort of pastoralist would entrust his animals to a forager? How is it that the effects of all this on San society remained limited, even when San bore the responsibility for the productive enterprise (on this see Passarge, who was at Nxai/Nxai during 1896–98, not—as Solway and Lee seem to believe—in 1907, when his report, first published in 1905, was reprinted) and many remained on the peripheries of villages and were still primary producers? *Mafisa* is not the simple, voluntary arrangement they suppose but a political relation with, in addition to benefits to both parties, well-defined obligations backed by strong coercive powers (Lee [1989] elsewhere offers a more realistic but still oversimplified discussion of Dobe *mafisa* arrangements). The less said about their benign view of serfdom the better, but Motzafi's (1986) vivid portrayal of the degradation borne by women in Botswana's subordinated minority underclass (San-speakers included) stands as eloquent rejection of their apologist stance on subservience.

Despite its flaws, this paper marks a significant move toward fuller comprehension of the social history of the Kalahari for Lee (Solway has consistently recognized that history); this can only lead to greater understanding of the region. I, for one, hope that the authors will continue in this direction.

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With a landmass of ca. 1.5 million square miles, southern Africa spans 19 degrees of latitude (compared with 23 degrees for the United States). It straddles the Tropic of Capricorn and thus includes both temperate and tropical regions; its environments range from Mediterranean to desert to tropical lowland. Prior to ca. 2,000 years ago, the sole inhabitants of this vast and diverse expanse were Khoisan peoples who lacked pottery and metal and were dependent solely on hunted and gathered resources. Archaeologists term their culture "Late Stone Age," and associated microlithic industries include small crescent or lunate forms. About 2,000 years ago Iron Age peoples appeared from the north; of Negroid



stock, they brought with them pottery, copper and iron implements, goats, cattle, and domesticated plants, including millet and sorghum. Given a varied environmental backdrop, interactions between Iron Age immigrants and indigenous Late Stone Age peoples were complex and varied from region to region. In places the Late Stone Age life-style disappeared rapidly, while in others hunting and gathering remained a viable and sole means of subsistence into the 19th century. In some areas Khoisan peoples acquired goats and developed an independent pastoral life-style. The "Black Bushman" groups of northeastern Botswana document an instance in which indigenous culture proved dominant. Where they retained their separate identities, Late Stone Age and Iron Age peoples undoubtedly developed varying kinds of relationships. Taken together, ethnographic, historic, and archaeological data demonstrate the impossibility of simple generalizations, and the history and prehistory of individual subregions must be approached on a case-by-case basis.

After 20 years of archaeological research, the prehistory of the Dobe and neighboring /Xai/Xai San is well understood. A sharp line of demarcation separates the vegetated dune-and-valley system characteristic of this region from the better-watered Okavango swamp margins to the east. This boundary, delimited today by the westernmost extension of the Northern Kalahari Tree and Bush Savannah with Mopane, marks the limits of riverine-adapted peoples such as the Bayei and the Mambukushu. It has an archaeological reality as well. To the east, extensive work by Denbow and Wilmsen (1967) in the Tsodilo Hills and other margin sites demonstrates the presence of an Iron Age tradition as early as the 6th century A.D. Excavation confirms the presence of domestic goat, sheep, and cattle, sorghum and millet, metal smelting, and relatively large numbers of ceramics.

At Dobe, /Xai/Xai, and other waterholes to the west, however, the situation is very different. While many Late Stone Age sites have been discovered, wide-scale survey by a number of trained archaeological observers has uncovered no evidence of Iron Age occupation. Excavation at /Xai/Xai by Wilmsen (n.d.) and by Yellen and Brooks (Yellen and Brooks n.d.) both there and at other sites shows a Late Stone Age tradition with an earliest date of ca. 3,600 B.P. that continues into the 19th century. Materials consist almost solely of lithics and burned animal bone. Because the sequence begins well before the appearance of Iron Age peoples in southern Africa, one can assess the impact of this immigration through examination of change in the archaeological record. With the advent of Iron Age peoples along the Okavango margin, small amounts of pottery and metal appear in the western Late Stone Age sites, and this clearly indicates either direct or indirect contact. The effect of these Iron Age peoples, however, is minimal. In contrast with the situation in margin sites, both metal and pottery are extremely rare. The diverse stone tool assemblages remain unchanged over time, suggesting continuity in the kinds of activities people conducted. In sum, the archaeological evidence indicates that the ap-

pearance of Iron Age peoples to the east had little effect on the Dobe-/Xai/Xai foraging life-style.

Archaeology of very recent sites also provides relevant information. In 1975 and 1976 I excavated a series of Dobe San camps, the earliest of which dated to 1944 (Yellen 1987). Although Herero pastoralists were in the region at that time and !Kung undoubtedly had access to goats and cattle, faunal analysis indicates a minimal presence of these species in the Dobe !Kung diet prior to 1962. The fauna reflects a hunting-and-gathering mode of subsistence, and not until the mid- to late 1960s does the picture begin to change. Thus archaeological data from both older and more recent sites support Solway and Lee's Dobe analysis.

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Solway and Lee are to be commended for their attempt to sort out the shouting match that has plagued Bushman studies for over a decade. Differences in archaeological/historical construction, in the form and content of ethnography, and in the discourse of theoretical explanation and interpretation have reached a boiling point in the comparative study of hunters and gatherers, and those of us who have worked elsewhere no longer have any idea what we should take seriously, let alone accept.

No one can attribute pristinity to the Bushmen. One might be able to present a better case for an unchanged existence for some other hunter-gatherers, but even in such special cases the issues are far from clear. While Aboriginal Australians and possibly some of the Arctic Eskimos might be such cases, even here the pristinity of a hunting-and-gathering existence is dubious if not moot. Australia has the virtue of being a land in which hunting-and-gathering societies were dominant throughout the continent, though regional differences existed in the intensity of exploitation and the kinds of internal variability between and among societies. However, the Aboriginal cultures in Arnhemland and Cape York possessed rudimentary knowledge of how certain root crops could be replanted, and in some cases they had knowledge of different societies in the Torres Straits area as well as coastal New Guinea. Yet, the pristine hunting-and-gathering society, an anthropological fiction and ethnographic invention, has dominated our thinking and in most cases distorted any real theoretical contribution to knowledge about the internalization of historical processes in societies on the periphery.

Solway and Lee stress that the hunting and collecting structure and the egalitarian ethos are or can be regenerated in the "bush" when patterns of exchange or forms of domination change. Thus, they note, "When cattle were withdrawn, as they often were, the bush was there to fall back on, and that same bush beckoned as an alternative if the responsibilities of keeping cattle grew too onerous." This assumption also occurs in the Abori-

ginal Australian literature—the idea that once cattle or sheep ranching declined the “bush” would be restored and a traditional way of life could be resumed. Interestingly enough, this assumption is shared by Aborigines and whites in central Australia for different reasons. Whites feel that Aborigines can “go bush” and thus not be a burden on them, and Aborigines feel that the “correct” way of life has to be maintained and that can only be done by reverting to the “bush.” But in reality the “bush” has changed radically. It can no longer sustain what it might have sustained in past times, and therefore it is no longer a guarantee or even an opening on the past. It would be helpful if Solway and Lee had shown how these various social and economic transformations have changed the “bush” so much that there may be no means of returning to anything. Environmental degradation can drastically limit the possibilities for change, whatever the mode of production. Many anthropologists (including myself) who have worked with various versions of hunter-gatherers have wrongly succumbed to the temptation of the eternal “bush” as a means of possibly preserving their pristinity.

One of the critical contributions of Marxism to anthropology is its ability to deal with history, structure, and ethnography within a unified context. History is not simply relations with neighbors and possibly world systems but change induced by those relationships in the internal structure and logic of particular societies and the impact of changing structures on future events. The dialectic permits us to move beyond the internal/external contrast and directs our attention to emerging social formations that embed particular societies in a regional matrix. It may be too late to rethink the comparative ethnography of the Kalahari along such lines, but given the vast amount of work by archaeologists, historians, and social anthropologists we may be able to develop some informed speculations that focus on the dynamic dialectic between evolving social formations within the context of encroaching political and national hegemonies.

## Reply

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First our thanks to all the respondents for adding their rich and varied input. Their efforts indicate that we agree at least on one thing: that San history is a topic of importance.

Many of the commentators find our general position reasonable and offer useful suggestions and criticisms to strengthen and/or modify our arguments. Trigger, Patterson, Ingold, Yengoyan, and Bicchieri focus in varying ways on the communal logic of the relations of production underlying the adaptation of San and other foragers both prehistorically and in the recent past. (Trigger and

Patterson would extend this analysis to include some non-foragers such as low-density horticulturalists.) Crucial to this communal logic is the ability of these societies to reproduce themselves while limiting and inhibiting the accumulation of wealth and power. Their comments, reinforcing a point previously made by Clastres (1977), add a crucial component to our argument: that such societies have social and political resources of their own and are not simply waiting to assimilate the first hierarchical model that comes along. Nevertheless, we take seriously Trigger's caveat that because “ethnographic data have ceased to have an ensured significance that is independent of their historical context” interpretation of the archaeological record assumes even greater importance in the solution of these problems.

In a similar vein, Ingold urges us to reconsider the use of the term “society,” which he sees as characteristic of agrarian discourse and as obscuring the distinctive mode of sociality that characterizes foraging peoples. This comment strengthens our case, which rests in part on the evidence for the retention among some Kalahari foragers of their own cultural specificities and forms of social reproduction.<sup>1</sup> We return to this point below.

We see Yengoyan's observation that among the Australian Aborigines foraging relations of production can be regenerated in the “bush” as another indication of the existence in these societies of an internal dynamic apart from the dynamic of their articulation with capitalism and the state. He also notes, as we do, that environmental degradation can foreclose this option. Bicchieri observes that evidence of hunter-gatherers' resistance to becoming “like us” has confounded writers of “premature epitaphs” and advocates of “progress.” He asks rhetorically whether it is not worthy of comment, “given the pressure to hurry small-scale societies along the path of ‘progress,’ that so much autonomy and egalitarianism still survive.”

Barnard plays a doubly useful role in the discussion by attempting to frame the terms of the debate within a wider discourse and adding some observations drawn from his own extended Kalahari field experience. He distinguishes three broad avenues of inquiry among Kalahari researchers: the regional historical study of larger systems, the ethnographically specific study of unit cultures, and the focus on economic pluralism and dependency. (We would add that some researchers work in two or all three.) These approaches are not necessarily antagonistic, and he sees our paper as an implicit attempt to reconcile them. He enjoins us not to lose sight of the diversity of culture-contact situations or of the historical continuity observed in some cases.

Harpending and Draper offer further concrete support for a degree of autonomy for 20th-century !Kung in their finding that of 13 elderly Kung interviewed in the 1980s, 12 had spent their childhoods hunting and gathering in the “bush.” They are correct that many !Kung live in areas of greater contact and that Dobe !Kung were

1. Nicolas Peterson has recently made a similar point.

in contact with them, but the distinction between *contact* and *domination* is precisely our point.<sup>2</sup>

For Campbell, who has devoted more than three decades to the study of Botswana's prehistory, history, ecology, and culture, the question of prehistoric domination in the Kalahari remains open: the evidence is not yet in. With others, he acknowledges the diversity of Kalahari San cultures. He states further that San and Tswana have had a "long past association . . . somewhere in southern Africa." We do not doubt it. But he also adds that the Tswana are recent arrivals in the Kalahari, and therefore the presence of mythological themes common to Tswana and *some* San languages cannot be invoked as evidence for the integration of *Kalahari* San with Tswana. We strongly agree with him that there are a number of San peoples who are much better candidates for "regional integration" than the Dobe !Kung, for example, the numerous "Black San" of the Okavango, Botletle, and Nata Rivers.

As one of Barnard's economic pluralists, and having worked among the proletarianized farm-Bushmen of Ghanzi, Guenther says that he was initially attracted to the revisionists' banner but became disenchanted with their "stridency" and "claim to explanatory monopoly." He finds reasonable the view, given the diversity of Kalahari San cultures and the vast area they occupied, that some groups at some times would have been "relatively isolated and autonomous."

Yellen offers a thoughtful overview of southern African prehistory, in which the diversity of environmental settings and economic interactions between Stone Age and Iron Age cultures is emphasized. His point that different ecological zones in the northern Kalahari reveal different archaeological pictures supports our thesis of historical variation of San integration and contradicts Schrire's assertion of a "pattern of irrefutable archaeological evidence" of Iron Age elements throughout the Kalahari. Jacobson provides helpful archaeological information from an adjacent area, Namibia, noting that in the period A.D. 600–1100 Iron Age settlement of the Kalahari was "sporadic" and "subsequent to this there appears to have been *no settlement*" (emphasis added). If this suggests a long period of Iron Age *absence*, is it possible that for some hundreds of years the foragers of parts of the Kalahari were hunters in a world of hunters after all? The implications of this hiatus should be explored. Jacobson's views on trade seem eminently sensible to us: why invoke integration with Iron Age peoples to account for small quantities of exotic goods on Stone Age sites when such materials could as easily be accounted for by Wiessner's *hxaro* trade?

Kent and Motzafi-Haller join Jacobson and ourselves in raising questions about the impact of trade. What exactly does it mean to the people involved? These commentators agree with our caution against attributing profound transformative powers to "things." We suggest that foragers' trade with non-foragers can be assimilated

into preexisting patterns, that the meaning and value of "things" will be different in different social contexts, and that the values in one social context can be translated into the values of another; this may be an aspect of what Ingold refers to as a different kind of sociality.

Several commentators direct our attention to Wiessner's (1982:61–84; 1986) elegant and persuasive analysis of *hxaro*, a system of exchange in which externally derived trade goods are transformed from one scale of values into another—from "more balanced and immediate trade" to delayed reciprocity. Thus it is encouraging that Wiessner concurs with our general position. Her data add weight to the argument that reciprocal exchange is more than adequate to account for the presence of exotic goods in Dobe area prehistoric sites. If this is what Denbow and Wilmsen mean by a regional system of interaction, then perhaps our positions are not so far apart.

Motzafi-Haller urges us to go beyond a strictly economic understanding of !Kung autonomy to explore the lasting social, ideological, and political impacts of the 19th-century European presence. She raises important questions, some of which have been dealt with in our paper and elsewhere (Lee 1979) while others should be the focus of future research.

Hitchcock suggests that the loan-cattle arrangement at Dobe may not be *mafisa*, despite its being so termed by the San. He points out that *mafisa* relations tend to occur between relative social equals and rarely between Bantu and San, although San herding of Bantu-owned cattle is widespread. One of us (Solway) found corroboration for Hitchcock's position in Kweneng, where *mafisa* is non-coercive and almost exclusively between related Kgalagadi and where San herd Kgalagadi cattle but in non-*mafisa* arrangements. Does the fact that payment of a female calf to the *mafisa* holder is obligatory in Kweneng and elsewhere but optional at Dobe indicate a variant of *mafisa* or a different kind of cattle-holding system? We thank Hitchcock for raising an important question for further research.

We take seriously his concerns regarding San servitude. He and others have accurately drawn attention to instances of repression, coercion, and violence in San/non-San relations. While not denying this type of relationship, we hold that it cannot be generalized. We have also observed relationships with a different quality—with less coercion. (Harpending and Draper also express concern about what conclusions can be drawn from 19th-century reporting of San servitude.)

Hitchcock is probably correct about the presence of Iron Age materials in the prehistory of western Kweneng. Oral traditions of San and Kgalagadi both emphasize a long association. We hope that archaeological research will be undertaken in the area, and we find it curious that none has taken place given the intensive investigations that have been carried out in the north.

Some of the critics are still not engaging with the point we are making. Were there parts of the Kalahari in which San subordination to black domination was early, thoroughgoing, and persistent? Yes. Were *all* or even the vast majority of San so subjugated? No. Thus it is point-

2. The mitochondrial-DNA evidence for !Kung's marked distinctiveness is intriguing, but at this early stage we do not quite know what to make of it.

less to accuse us, as Denbow does, of maintaining the San in a kind of "historical apartheid" into which no "non-zero-sum" economic alternatives can intrude.

Wilmsen, Denbow, Gordon, and Schrire raise questions about our historical methodology and knowledge. We are accused of ignoring a large body of archival material (Gordon), but in a paper of this kind we could present only summaries of more extensive historical research (Solway, in particular, was able to include in the paper only a fraction of her historical sources). We agree with Gordon that there is more to be gleaned from the German and Afrikaans sources.<sup>3</sup> We are also accused of privileging the words of a few "ancient !Kung elders" over the "hard facts" of the archaeological record (Schrire). However, as our paper and the contradictory opinions expressed by our commentators confirm, the archaeological "facts" do not speak for themselves, and the written texts must be historically and culturally located as well. Gordon implies that we find hidden or conspiratorial agendas in the colonial documents when they appear to exaggerate the harshness of San servitude in order to serve colonial ends. This allegation is unfair and untrue; it would be poor scholarship to accept these or any other documents at face value without placing them in historical and social context.

We acknowledge the antiquity of trade in the region to which Wilmsen, Denbow, and Gordon point, but the historical record shows that patterns of trade were uniform neither in time nor in space. The fact that ivory was exported from southern Africa in the 16th century (Wilson 1975:78) should not blind us to the fact that trade increased by a quantum jump after 1850 with the arrival in the Kalahari of unprecedented numbers of European traders and missionaries. African chiefdoms competed amongst themselves to benefit from this trade, and their power increased. This is not to imply that stasis characterized earlier periods or that all was a "zero-sum game" before European "contamination" (Denbow), but it is to suggest that patterns of trade shifted through time and that the intensity of trade depended on proximity to European centers. Areas such as the Kweneng, close to the Transvaal border, were more permanently involved in resource extraction than were areas such as Dobe, despite the presence for a time of European hunter/traders in the latter.

In order to portray Lee as the staunch upholder of hunter-gatherer pristinity, Wilmsen chooses his quotations from Lee's oeuvre rather selectively. He has chosen to ignore, for example, passages in which Lee specifically disavows "pristinity" and discusses !Kung cattle ownership and agriculture in the past (e.g., Lee 1976:18) and in which, as Harpending and Draper remind us, he has "depicted the Herero presence in the Dobe area in scrupulous detail." Other passages overlooked include those in which Lee urges caution in the use of hunting-and-gathering models for evolutionary reconstruction (e.g., 1979:2, 433-37)—a point that Barnard emphasizes

here. Alternative readings of Lee's position are possible; in at least one recent synthesis, he is located among the revisionists (Haraway 1989:194-97, 227).

Almost a decade ago Lee coedited a book (Leacock and Lee 1982) one-third of which was devoted to exploring the historical and contemporary interactions between foragers and farming, herding, and market societies. Leacock and Lee (1982:1-20) argue that foraging societies can only be understood as the product of a triple dynamic: the internal dynamic of the foraging relations of production, the dynamic of their interaction with farmers and herders, and the dynamic of their articulation and incorporation within the capitalist world system. The difference between us and the revisionists is that they emphasize the operation of the second and third dynamics at the expense of the first, affording minimal reality to foraging as a distinct mode of life, what Ingold has here called a "radically alternative mode of relatedness."

It is heartening to see Schrire and Gordon attempting to rescue the San from being simply passive victims of outside forces. Rather than "toppling from foraging to begging" the San in Schrire's view could follow a number of alternative pathways, including Gordon's "agents . . . brokers . . . and hired shots." We would suggest, following Chapman (1971 [1867]), that the list be expanded to include hunter-gatherers.

The revisionists appear to be united in their appeal to regional studies and to political economy, and we are much in sympathy with this view. But when we borrow from political economy we should be aware that we take over weaknesses as well as strengths. Theory in political economy requires both a state and a market, and it has been notoriously unsuccessful in dealing with societies that lack either or both. It has been far easier to recreate these societies in the image of our own, thereby obviating the need to account for their non-hierarchical, communal, and non-exploitative aspects.

The !Kung San of the Dobe area in the 1960s were convinced that their ancestors were a distinct people who lived by hunting and gathering in the last century. And while that view, like all oral traditions, was a cultural construction, most observers in the 1950s and '60s were struck by their strong sense of themselves and their retention of vital and viable systems of meanings and practices. The impression of cultural identity and cohesion was reinforced by the fact that most of the institutions of cultural and social reproduction were in place—including language, exchange systems, ritual practices, the kinship and name-relation system, and the continuing ability to feed themselves by hunting and gathering. At the same time, several important sociocultural subsystems were clearly not functioning. The last men's initiation ceremony (*choma*) had been held around 1960. (The women's menstrual initiation dance, however, continues to be performed.) The landholding name groups that had flourished in the last century had ceased to function by 1963 (Lee 1979). Dispute-settlement mechanisms and land-tenure systems were in a state of flux and had begun to come under the control of the Tawana headman, backed by the colonial state (Lee 1979). Never-

3. In collaboration with Mathias Guenther, one of us (Lee) is currently reanalysing the works of Passarge, Schinz, Wilhelm, and others.

theless, the !Kung certainly *believed* that they were a people—beleaguered, in the throes of change, and in an increasingly dense multi-ethnic setting, but still with a corpus of institutions and a sense of themselves.

!Kung identity and the institutions of social and cultural reproduction that support it are perhaps the most eloquent testimony against the claims of the revisionists that the Dobe !Kung are to be seen primarily as a component of a rural underclass dominated by tributary power and merchant capital. To accept that view is to miss a crucial component of the !Kung reality. It is true that the European frontier washed over them and receded between 1870 and 1900, but when the Europeans left, life went on. Later, *mafisa* cattle waxed and waned in importance, and life went on. Either the !Kung survived the withdrawal of European and Bantu resources or they perished. Since they survived for periods and in places without these external resources, we have to conclude that there was *something* there for them to fall back on—some means of reproducing themselves culturally and physically that was their own and not dependent on outside intervention. That degree of self-sufficiency, a portion of which was still observable in the 1960s, was the basis for the view that a hunting-and-gathering way of life had persisted in the Dobe area.

What we are arguing for here, and what has been our theme all along, is not a vision of hunter-gatherers impervious to all change. Our extensive treatment of the Kweneng case demonstrates our awareness of historical specificities (and also our attempt to break the tyranny of the Dobe case over the anthropological imagination). Rather, we are seeking a more complex and subtle understanding of how people have or do not have the capacity and will to absorb and process new elements, ideas, experiences, opportunities, and still retain internal coherence and the capacity for self-reproduction.

We continue to be puzzled at the revisionists' reluctance to grant foragers identity and historical agency. What is it that disqualifies them? Why must they be full-fledged members of an oppressed underclass (or "hotshot traders") before we allow them historical visibility? In some cases (e.g., Kweneng) the development of a regional hierarchy was the salient historical fact, but does that constitute the only legitimate history for foragers? By the same token, why is the analytical and political project of understanding the laws of motion and change generated in part from foragers' internal dynamics and economic logics—the distinct mode of sociality (In-gold)—regarded (in some circles) as ethnographic romanticism and greeted (by some) with such condescension and derision? We encourage students of foraging peoples and the peoples themselves to address these questions.

A number of the commentators have mentioned the increasingly shrill and strident tone of some of the participants in this debate. We have consciously tried to avoid this here in the hope that less heat will generate more light. While we squabble, the peoples of the Kalahari are facing increasingly serious political and economic problems. We could be more effective as allies in their struggles if we could overcome this stumbling block.

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