

unreliability as a reporter. There are good grounds for arguing that he *never even reached Lewisfontein or Karakobis*. Passarge's 1904 map of the Nyaenya-Dobe area (pl. 1) shows Schinz's route passing through Gam on the southern margin of the area, 65 km south of Qangwa-Lewisfontein.

One could of course argue that I am nitpicking: What if a few Europeans weren't where they were supposed to be if the overall picture of mercantile penetration and devastation is accurate? This picture is the cornerstone of Wilmsen's (1989a) account of foragers as an impoverished rural underclass the roots of whose dispossession go back over 1,000 years. Close examination of his sources reveals that the evidence for this position is anything but irrefutable. By conflating accounts of trade from all over the Kalahari he makes these accounts appear by implication to apply to the Dobe area when the historical record reveals nothing of the sort. As Solway and I have noted, no one is denying for a moment that *some parts* of the Kalahari were the sites of intense and sustained mercantile activity; but this is not news, having been noted 25 years ago (Lee 1965). The question is: was the Dobe-Nyaenya area such a site? The historical record, including the data assembled by Wilmsen himself, answers emphatically in the negative. Wilmsen's (1989a: figs. 3.4, 3.7, 3.9, 3.11) overlays of place names or putative trade routes are applied to maps on which the original historical documents show a virtual blank. Readers are encouraged to consult the maps and accompanying information in the sources themselves to evaluate the level of geographical knowledge and commercial activity in this part of the interior during the period in question (e.g., Andersson 1856, 1861; Baines 1864; Chapman 1971; Galton 1853; McKiernan 1954; Reise 1859; *Neueste deutsche Forschungen* 1867; Herero-Land 1878; Vedder 1938). Whereas Wilmsen (1989a: fig. 3.5) depicts the Nyaenya-Dobe area as the centre of a network of trade routes in the mid-19th century, he presents not a single document for any of these routes, nor could we find any. Passarge himself does not help the case when he writes (1907:118, my translation),¹ "There do not seem to be any trade routes through Bushman land, rather the trade went on between two close tribes." And Galton, writing in 1853, at the beginning of the colonial trade era in northern Namibia, notes of the Herero peoples immediately to the west of Nyaenya-Dobe that apart from the north-south axis "the Damaras have no communication whatever with any other country, a broad land dividing them from the natives to the east, and the sandy tract by the seashore bounding them to the west" (1971 [1853]: 199).

The Nyaenya-Dobe area in the period 1850-1900 was far from the entrepôt that Wilmsen and Denbow portray. In fact, with the exception of a few years in the 1870s, the sources indicate that it was rather a backwa-

ter, bypassed by most of the major trade routes. This is the picture strongly conveyed by Schinz (e.g., 1891:357-62) and Passarge (e.g., 1904:407-15; 1907:119, 131-33). The !Kung San traded when they could, defended themselves when they had to, and during the long intervals between trading expeditions went about their business. These early fragmentary accounts offer glimpses of a relatively resilient and opportunistic hunting and gathering adaptation not in isolation from the wider world but in contact with it. In broad outline this is the picture that a dozen pre-Wilmsen ethnographers documented in the 1950s, '60s, and '70s.

Even in the absence of evidence to support the case that the Nyaenya-Dobe area was the centre of mercantile activity one could fall back on the weaker but still potent argument that the mercantile activity elsewhere in the region was sufficient to transform it. Here we return to the "Coke Bottle in the Kalahari" syndrome (Solway and Lee 1990:109-10): any level of mercantile activity, however small, has power and reach sufficient to penetrate all the social formations to the point where they are "sucked dry of commoditizable wild animals" and "selectively destroyed," but surely it is of interest whether a trading expedition arrives once a week, once a month, once a year, or once a decade? And whether the value of goods taken annually is \$100,000, \$10,000, or \$100? Or is the mere presence of any mercantile activity sufficient to transform hunter-gatherers into serfs, wage slaves, or members of a rural underclass? Is the question of historical and cultural specificity no longer of any relevance in the age of post-modern political economy? These are the broader questions raised by the paper under review.

Finally, I wonder what the connection is between the basic weakness of the historical case and this paper's extraordinary rhetoric.

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Wilmsen and Denbow describe the debate between Fritsch and Passarge as the first great Bushman debate. This, through no fault of theirs, is not accurate. Some 70 years earlier, Dr. John Philip, the superintendent of the London Missionary Society in southern Africa, and Donald Moodie were the main protagonists in a considerably more vehement argument on the South African past which covered a wide range of issues—and indeed led to a clutch of libel suits—but which had as one of its main foci the origin and status of the Bushmen within what was then the Cape Colony. It was, I would contend, this controversy that set the agenda for most subsequent discussions of pre-industrial South African history (Ross n.d.).

Philip (1828) argued in essence that the Bushmen against whom the Dutch colonists waged a genocidal

1. "Handelstrassen scheinen durch das Gebiet der Buschmänner nicht bestanden zu haben, vielmehr erfolgte der Austausch von Stamm zu Stamm."

war in the 18th and early 19th centuries were people forced to take up a predatory way of life because their previous pastoralist existence had been destroyed by colonial conquest. Moodie (1841), in contrast, adduced a variety of quotations from the earliest records of the Cape Colony to show that “‘considerable numbers of these people [the Bushmen] existed in their present state previous to the occupation of the country by Europeans’ and that they were, as they still are, the scourge of every people possessing cattle.” It was an argument with direct political consequences. Philip was attempting to show that the actions of Europeans towards the autochthonous peoples of southern Africa had been and still were essentially unjust. If this was the case, then a new, more humanitarian course of action was required. Indeed, he managed to persuade the influential British Parliamentary Committee on Aborigines to recommend such a change of policy. Moodie, on the other hand, was employed by the Cape government to find evidence which would rebut such views and justify the continuation of the *status quo ante*. Perhaps for the first time (at least among the colonists) but certainly not for the last, South Africa’s past was contentious precisely because it could be mined for arguments about the country’s present and future.

With this in mind, it is instructive to inquire into the hidden agenda behind the Fritsch-Passarge exchange. It would be naïve to believe that Germans writing in 1907 on the Omaheke (as the Herero called the desert into which they had been driven by German colonialists a year earlier) would have been motivated exclusively by disinterested scientific concern. I must admit that I cannot see whether there are deeper ideological disagreements to be found in the exchange or whether it was merely impelled by Fritsch’s pique at having been ignored by a man whom he considered his client. It may well be that I have failed to see through the code in which they wrote, or it may be that on fundamentals they agreed. What is certain, however, is that Passarge was writing within the discourse of German colonialism. To me, at least, he seems to be foreshadowing the “Thank God the Germans came” line of argument used notably by Heinrich Vedder (Lau 1981), arguing that colonization saved the Bushmen from destruction—though he does admit that it had been the introduction of firearms which had first caused the problem and wonder whether Afrikaner settlement near Ghanzi would not exacerbate the problem (1907:119, 123).

Passarge, then, was certainly not a naïve observer. Exactly how his political and personal concerns may have influenced his ethnography is, however, not something that Wilmsen and Denbow make clear. I take it that the promised translation of Passarge’s works concerning the Kalahari will address this issue and provide more information about a fascinating, if somewhat repellent, man (see his comments on colonial policy in South Africa, with their great dislike for any form of humanitarianism [1908:341], or his justification for the maintenance of slavery in Cameroon after the German colonial conquest [1895:526–27]). It may also explain why he chose to

adopt a tactic which is most unusual for an ethnographer, namely, describing the area of his fieldwork as atypical. To Passarge the Kalahari was “a region created for hunting peoples” except (at least according to his map) for the small area to the west of the Okavango swamps where he, and later so many others, did fieldwork. In his map of the economic potential of southern Africa he designated precisely that area, along with the environs of Lake Ngami, as suitable for pastoralists (Passarge 1908:170). Wilmsen and Denbow have not provided such basic source criticism on Passarge, merely commenting on the length of time that he spent in the Kalahari. This would no longer be seen as a sufficient justification for the work of any ethnographer. The fact that Passarge supports their position does not absolve Wilmsen and Denbow from a responsibility towards his work which they assume with some relish with regard to that of their opponents.

In the absence of such cross-examination of their key witness, I am not convinced by Wilmsen and Denbow, except that I have no reason to doubt that the inferences they have drawn from the archeological data are relatively solid. Nevertheless, I think that they are probably right in their basic contentions. These contentions derive, however, in the first instance from their examination of written texts produced in the early years of colonial penetration, as indeed is the case for virtually all discussions of foragers in southern Africa from John Philip on (e.g., Elphick 1977, Schrire 1980, Parkington 1984). The logics of analogy and back projection, justified or otherwise, which run from these into the more distant past are complex and are made even more so by the need to take into account the activities of the Dutch in the Cape and the arrival of firearms and European traders in the Kalahari, to take only the case at hand and that which I know best. Building the arguments which are necessary requires more than the piling up of favourable quotations.

In principle, the same procedures can be used to build analogies from the ethnographic present (of Richard Lee, for instance) to the distant past as from early colonial times to the centuries which preceded them, though of course the results would necessarily be somewhat more uncertain. It is not necessary to claim that the Zhu were isolated in order to make statements about them which have some potential relevance to the study of southern Africa before the introduction of domestic stock more than 2,000 years ago and of the rest of the world even longer ago. Indeed, as this exchange has shown, it is counterproductive to do so. What we surely need now is to move beyond the sharply polarized positions that have been taken and ask whether it is possible, given the Zhu (and other Bushmen) were not isolated groups of foragers when they were investigated but also given that they acquired a considerable proportion of their sustenance from hunting and gathering, to draw any general inferences about this way of life from their study, and, if so, which? Such an investigation must necessarily take into account the modern, historical, and archeological evidence of interaction with pastoralists, agricultural-

ists, and colonialists, if only to discount it. It is surely illegitimate to extrapolate directly from the present to the past, except in the ways in which historians and archeologists always do, and would be so even without the problems caused by the interaction with pastoralists and others. Rather, descriptions of the behaviour of men and women in some ethnographic present, whether Wilmsen's, Lee's, Passarge's, or Philip's, though valuable in themselves as evidence for the time at which they were created, can only be used heuristically for other periods, as potential generators of ideas which need to be tested in some way or other when applied outside the very immediate temporal and spatial context in which they were made.

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It is difficult to know how to respond to an article that is so negative in tone, destructive in intent, and singleminded in its attack on a group of scholars, particularly Richard Lee. Wilmsen and Denbow claim to take the intellectual and moral high road and close by saying that they will continue to engage evidence in a dialectic of theory and practice while others, presumably Lee and myself, are concerned with personalities and positions. Yet their article reads like an inquisition. Does one respond in like tones? Does one "wince" at every laboured attempt to harness 19th-century sources to their polemic, "cringe" at every insult hurled in Lee's direction, "behold" as they penetrate layers of ideology and inadequate theory to see truth in the Kalahari where all before them have been tainted by "overtones of Tönnies," mired in "postwar biological humanism," etc.? Are we to accept that they have cornered the market on "actuality" while others have simply written allegories about invented people?

It is disconcerting to see one's words come back as parody, quoted out of context and with all qualifiers removed. There are numerous examples, but one will suffice. In their closing paragraph Wilmsen and Denbow paraphrase Lee and me as saying that "providing historical detail to their histories robs 'foragers' of those histories." This is indeed "obscurantist rubbish," but we never said it. The original passage reads: "imposing agrarian discourse on hunter-gatherers . . . robs the latter of their history" (Solway and Lee 1990:122). Their reading misses both its point and its spirit. We were talking about the theorizing of history and not about the addition of more historical detail, which is something we welcome.

Wilmsen and Denbow chide us for not understanding "history as historicity, the process through which social formations realize their transformations." Our case-study periodization is not, however, simply a chronicle of events but a model in which the latter phases represent systems of regional articulation. They also accuse

us of subscribing to a simplistic ecological determinism in which San dispossession is attributed to environmental degradation. Looking to the Southern Kalahari case, San dependence on Bantu-owned water sources has been a critical factor, but, as we note, this has more to do with changing property and power relations than with the changing water table (Solway and Lee 1990:114, 119).

The use of 19th-century sources by Wilmsen and Denbow demands mention. In an age when "ethnographic authority" is anything but taken for granted (Clifford 1983) and ethnographic style and method are themselves subjects for study, it is surprising that Wilmsen and Denbow grant so much ethnographic authority to Passarge. Our firsthand observations about contemporary drought-relief feeding and borehole drilling in the Kalahari are dismissed as "quasi-quantitative" while Passarge's descriptions are presented as "considerably ahead of his time." His use of terms such as "clan," "band," and "headman" goes virtually unquestioned. Contemporary ethnographers would acknowledge these terms' histories of competing definitions and would situate their usage within an anthropological discourse. I also question the manner in which MacKenzie's words are generalized to the whole Kalahari. MacKenzie's observations of the contestation between Tswana leaders over their peripheries were written from the powerful and wealthy trading capital of the Ngwato chiefdom. This was the mercantile crossroads of central-southern Africa (Parsons 1977:113), and the political and economic fortunes at stake were significantly greater here than in Ngamiland, by comparison a political backwater.

Wilmsen and Denbow's paper is ambitious in its attempt to draw broad conclusions from vast regions and periods of prehistory from archeological evidence. The arguments offered are intriguing but highly speculative and at times contradictory. For example, we learn that the whole Kalahari has been enmeshed in hegemonic relations for over 1,000 years, with European incursion representing continuity with a long line of power and profit seekers: "the dominant centers of the hardveld . . . were extending their economic interests into the western sandveld; . . . this was the beginning of hegemonic domination from the east that was consolidated by about A.D. 1000." Yet at another point in the text we find that "Khoe and San peoples, far from being icons of aboriginality, developed and controlled the means of production and trade over large parts of the Kalahari interior in earlier centuries, only being subordinated as 'Bushmen' in the 19th century." Aside from the obvious discrepancy between these statements one has to question whether the archeological evidence is sufficient to suggest that any group of people was able to consolidate hegemony over the whole Kalahari 1,000 years ago.

A more general concern is the underlying assumption about cultural difference. I completely agree with Wilmsen and Denbow in their plea for the abandonment of a reified view of closed cultures in favour of an analysis of continuously shaped social formations. But in rejecting the idea of closed cultures how does one account for