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On the Myth of the "Savage Other"

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Headland and Reid (CA 30:43–51) are to be congratulated on raising an issue which is arguably central to the praxis of anthropology, that of the consequences of our labels for those defined as "Others." While their critique is well founded, they do not present us with an alternative analytical mode. We need to know more about both the sociocultural milieu that spawned the myth of the "savage isolate" and the concerns inducing the critique. My own work on that settler-labelled lumpen group known as the Bushmen arose out of a concern for the consequences of our imagery of "pristine savages" (Gordon 1986, n.d.*a*). But then, I was privileged to grow up in an area in which Bushman stories were an important part of local folklore, and the discrepancy between local wisdom and academic wisdom, coupled with a commitment to develop a sustained critique of the policy of apartheid, made my choice of topic obvious. What fuels the fires of other critiques?

My approach was to look at the material, economic, social, and ideological (iconographic) roles of foragers in the wider society, taking as a point of departure the observation that the term "Bushman" originally meant "bandit" or "outlaw." Hobsbawm's (1969) *Bandits* provides potentially interesting analytical prospects. An alternative approach that might be fruitfully explored is that of Roux (1964), who adopted Toynbee's notion of an external proletariat in dealing with the complexities of South Africa. After all, what is apartheid but the ultimate material manifestation of this accent on *difference*? I would be interested in learning where and how Headland and Reid propose to go beyond their critique.

Viewing foragers not as isolates but within a specific regional context is a recent fashion, but it is not a novel approach. One of the bases for Fritsch's (1906) attack on Passarge's *Die Buschmänner* was precisely that it did not treat the Bushmen as integral parts of Herero, Tswana, and Ovambo society. The interesting question is why Fritsch's critique was forgotten while Passarge's book went on to achieve the status of a minor classic. In the South African anthropological discourse in the thirties the key issue was whether one should study African cultures as distinct, isolated entities or whether, following Radcliffe-Brown, one should analyse black and white as part of the same social system (Gordon n.d.*b*). No prizes will be awarded for guessing which approach provided important ideological rationales for the policy of apartheid. In short, a strong case can be made that the issue is central to anthropology itself. But to consider questions of this nature we will have to go beyond a listing of a few isolated case studies and a listing of disparate explanations. With Diamond (1964), we will fearlessly have to confront "the uses of the primitive." One reason we do not engage in exercises of this nature is, I think, that we may not like our own conclusions. We will need a historicized political economy of anthropology. It is no accident that these "traditional hunter-gatherer groupings" are probably the most heavily commoditized peoples in the annals of academic discourse. Many scientists and others have a vested interest in maintaining the ideological status quo. Wilma Stockenstrom said it beautifully in her novel *Expedition to the Baobab Tree* (1983:92): "When I see the little people I know they are dream figures that really hunt and really provide me with food."

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Headland and Reid are correct in saying that modern hunter-gatherers are not a single group totally isolated from the outside world and preserving the way of life characteristic of their Palaeolithic ancestors. Each of these peoples has experienced a long history of social and cultural development. In my book on the Australian Aborigines (Kabo 1969) I attempted to show that their culture was continuously developing, though essentially without external influence, throughout their long history—thus challenging the "stagnationist" portrayal of hunter-gatherer societies as incapable of independent development (cf. Lévi-Strauss's analogous concept of "cold" societies). This is not to deny that hunter-gatherers developed at different rates, that the development of a particular society might occur in significantly greater isolation than that of other societies, and that the production of food was never characteristic of many of these societies. It has long been obvious that in the course of the history of mankind rates of social and cultural development have varied. The claim that all hunter-gatherers have experienced "acculturation" to some degree is as rash as the claim that none have. It is necessary to clarify in each case the exact nature of the external influence, how general and how intensive it was, how it affected the life of a given society, and upon what specific areas of that life it touched. The results of such an analysis may contribute to the reconstruction of much earlier stages of social and cultural development (Kabo 1986).

The fact that the Aborigines of northern Australia borrowed tools from the Indonesians did not change the fundamentals of their life; the new tools fitted extremely well into the former system of socioeconomic relationships. Once again this shows the need for a concrete and complete analysis of every situation and the importance of avoiding hasty generalizations.

The idea that many hunter-gatherers have long been in contact with more developed societies and borrowed extensively from them (including elements of agricultural production) is not in itself new. The symbiosis of Pygmy and Bantu has been recognized for a long time. What is far more important is clarifying the influence of this symbiosis on the social relationships and the economy of the Pygmies—what exactly was altered in their social system and what was preserved and why.

It cannot, however, be argued (as Headland and Reid tend to do) that isolation has never existed. For thousands of years the Tasmanians, the Australian Aborigines, and many Stone Age groups lived under conditions of partial if not total isolation, maintaining rather sparse populations in extremely harsh environments. It is typical that the clearest example of an isolated people—the Australians and Tasmanians—is not considered here. Headland and Reid do refer to the Australians' sporadic experiments with crop cultivation, which were essentially insignificant; the Australians remained until colonization typical hunter-gatherers.

Abundant references to the conclusions of others cannot take the place of a thorough, theoretically and methodologically well-grounded analysis of the facts.[1]

1. Translated by Atalanta Gillett.

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Headland and Reid have a just cause but do not serve it well with vagueness, sweeping generalisations, selective use of evidence, and partiality in its interpretation.

"Isolated" is not restricted in its meaning to a state of hermetic closing off of all contact whatsoever. The end-paper map of my Bushman Survey report (1965), surveyed and drawn by me, shows ≠xade Pan as a little over 100 km from cultivated lands at Tsxobe and Metseamanong. I described G/wi participation in exchange (p. 50) and alliance (p. 76) networks which stretched beyond the Kalahari. The commodities which moved across the network were indicated, as were their place and importance in G/wi life. I speculated (p. 82) on Tswana influence on their G//ana co-residents and later (pp. 121–22) described sporadic migration to European-owned ranches. In *Hunter and Habitat* (1981), relationships with non-G/wi are discussed at several places; a reading of pp. 60–63 and 138 et seq. should ameliorate the stringency of the view of pristine isolation that Headland and Reid attribute to me.

While I am obviously responsible for what I publish, Headland and Reid perhaps overlook the anthropologist's use of others' knowledge. I had access to Tswana, Kgalagadi, German, and British sources as well as G/wi informants. Historical data led to the clear conclusion that the G/wi, G//ana, and others were the only permanent inhabitants of the central Kalahari for at least 150 years, and the contemporary view was that one would have to be very odd to want to go there because so many had perished in the attempt. It wasn't *terra nullius*, but neither was it Bourke Street on a Saturday night. Headland and Reid rely on Keesing (1981), who evidently attaches some weight (p. 115) to the assertion by Curtin et al. (1978:292) that this was the region where the Khwe acquired sheep and cattle; this contradicts ecological and linguistic sense as well as the hard facts of animal husbandry. Their date of "perhaps early in the second millenium A.D." is, furthermore, unaccountably late, as Bantu-speakers had been herding sheep and cattle farther east in southern Africa for several centuries before that and had been in contact with Bushman in that more easterly location.

This is not to deny either the existence or the antiquity of the trade routes of Denbow and his collaborators.

Interpreting the relativistic concept of isolation as if it were used in an absolute sense is semantic vandalism. What I wrote is not whatever Schrire may mean by "depict[ing them] as quintessential isolates." In their eagerness to rid anthropology of one myth Headland and Reid are in some danger of fabricating another, viz., that all of us who lived among hunters and gatherers lied and continue to do so.

Keesing's 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, 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 (perhaps periodically completely) self-sustaining. In both continents they were held in low esteem by others and were politically powerless to compete with pastoralists, cultivators, and industrialists for land and other resources which the latter wanted. Where they had to compete because others had entered their domains, they were overwhelmed and ceased to live autonomously, found the consequent social and cultural changes distressing, and became despised, disadvantaged members (or adherents) of the dominant society, unable to operate their earlier mode of production.

In calling the G/wi "hunters and gatherers" I meant that they obtained nearly all of their sustenance by hunting (which included scavenging from other predators, trapping, catching, and simply picking up) undomesticated species of animals for meat and other products, gathering (picking, digging up) fruits and other parts of uncultivated indigenous plants for food and fluids, and drinking water found in naturally occurring (sometimes slightly improved) depressions after rain during and shortly after the wet season. The exceptions were meat from wild animals that I shot and uncultivated indigenous plant food that I gave to people in part-recompense for the time they gave me. On my parents' farm my father and I hunted antelope for meat and gathered various local fruits and plants; Kgalagadi, Herero, and Tswana herders, cultivators, and mixed farmers do the same: all of us I term "part-hunters and/or part-gatherers." Our modes of production may have been comparable, but the modes of distribution and cultural meanings of what we brought home differed quite widely.

To imply that because hunters and gatherers rapidly learn the technology of herding, cultivation, and industry they must have practised these in the past is Lamarckian and offensively condescending about their learning ability. When teaching astro-navigation to grandsons of women and men who encountered vehicular use of the wheel only in adulthood I was not impelled to hypothesise that their ancestors had formulated either a calculus of spherical triangles or a numerical ephemeris.

Parenthetically I report a matter of nomenclature: there was "Bushman" and then that infelicitous blunder "San," and now the Botswana government uses "Basarwa" in reference to the Bushmen of that country (Tswana-speakers will note the promotion from "Masarwa"). As the new term will be recognized by many Basarwa as applying to them, it is perhaps preferable to the neutral (but conceivably sexist) English or the pejorative Nama one.

Headland and Reid might try their lances on another myth, viz., that the !Kung are the stereotype from which recklessly to generalise to all other Basarwa and other southern African Bushmen. Many Basarwa and Bushmen share certain genetic characteristics, a heritage of hunting and gathering, and languages with a high incidence of click consonants. Some of these languages are related, but Traill (1978) identifies five separate language families. Among Basarwa there are radical differences in systems of belief, values, and social, economic, and political organisation. Some of these are of long standing, others the result of contact with non-Basarwa. Botswana is not ecologically uniform, and the knowledge and techniques needed for hunting and, especially, gathering vary greatly. The !Kung are not more numerous, widespread, typical, or significant than many other Basarwa. I agree that there are excellent descriptions and commentaries about them, but it is sloppy anthropology to appoint them or other Basarwa as reigning stereotype. In the fifties and sixties they were decidely not "intimately tied into continentwide cultural matrices." Certainly there were numerous similarities in material culture, but this is scarcely what anthropology thinks of as constituting all of culture and society.

Headland and Reid seem to have relied on commentators' quaint and uninformed interpretations of what we who worked in the Kalahari and in Australia wrote and would perhaps have been better served by consulting the latter. Plenty of us have written on the place of motor vehicles, involvement in national politics, tinned food, or welfare payments and wages in Aboriginal life. None of us, either in the Kalahari or in Australia, have been so barbaric as to mistake Basarwa or Aborigines for representatives of Tylor's state of savagery. "Other," perhaps; I found myself very "other" in relation to G/wi who knew so much that I did not. As I slowly learnt, the distance lessened, but I remained the pupil and they the masters. I was the savage, and I really can't see that any of us had any kinky motives for seeing it that way.

The G/wi did not "remain in their 'primitive' state" because they were "kept there by their more powerful neighbours." They had (and, as I reported, some took) the opportunity to join their neighbours. The others firmly expressed their preference for staying where and as they were and continuing to "eat no domestic foods" and did so until bore water was provided by the state and more powerful others came and made their preference impossible.

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On Assessing Nutritional Status in the Papua New Guinea Highlands

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Studies of the human ecology of the Papua New Guinea highlands usually deal with single groups of people on a small scale and in great depth. These features are so characteristic that such research has been labelled the New Guinea Syndrome (Mikesell 1978:8). Review papers like Dennett and Connell's (CA 29:273–81) are important because they bring into focus issues that for those immersed in the minutiae are blurred.

A major issue, as Allen comments (p. 282), is how to "investigate situations in which human physiology, plant physiology, long- and short-term climatic variation, human cultural beliefs and practices, economic change, and the provision of health services, to list only some of the factors which we have to deal with, are involved." The temptation, at least for the geographer,is to try and do everything (the geographer as polymath!). But, Allen asserts, not even large multidisciplinary cross-sectional studies are the solution; what is required is long-term interdisciplinary studies in a few select locations. Dennett and Connell's excellent review may at least help clear the underbrush preparatory to the expensive data-collecting exercises spanning one or two decades that Allen suggests. For example, it reveals the important problem of equating nutritional status with anthropometric measurements in the context of human ecology. The implication is that it must be clear what theoretical approach is being taken and what is to be measured or recorded before monitoring starts.

Various comments on Dennett and Connell's paper refer to the adaptiveness or otherwise of stunting. It is apparent that there is a broad range of opinion on morphological adaptation and its association with morbidity and mortality and on the measurement of such morphological characteristics, adaptive or otherwise. The crux of the problem is equating a measurable degree of dysfunction/loss of function (Pacey and Payne 1985) with a set of anthropometric measurements representing nutritional status. In effect this means that the charts/standards used for anthropometric measurement, whether the Harvard Standard Scales (Stuart and Stevenson 1959) or the National Center for Health Statistics (1976) growth charts, have to be calibrated to the degree of dysfunction/loss of function, as measured by rates of death, morbidity, or other factors, in particular populations. Such calibration will produce a set of scales unique to the population being studied and eventually obviate the use of international standards. Of course, the international standards could also be calibrated with sets of cut-off points, each set unique to a particular population.

In citing Heywood's (1982) and our (Crittenden and Baines 1985) work on the functional significance of malnutrition, Dennett and Connell (p. 275, esp. n. 2) appear to miss the point of using international standards as a reference rather than a measure to be attained. Of course there are problems with the Harvard standards, and "debate continues as to whether one set of charts can be used internationally or whether national or local charts derived from the local population should be used in different places" (Gracey 1987:173). The standards were, however, generally recommended (and modified) for use in the Pacific and South-East Asia (World Health Organization 1979) and in Papua New Guinea (Department of Health, Konedobu 1980), and they allowed us to compare the Nembi with previously studied Papua New Guinea groups. As work progressed, the Harvard standards were also used for comparisons over time. There is, as Heywood (1983) points out, the additional problem of assessing the genetic potential for growth of different Papua New Guinea populations. Again this is a matter of calibration. It is not a question of rejecting the Harvard standards as invalid; they are as invalid or valid as any international standards in that context (Gracey 1987:176).

The WHO growth chart is based on the data from the U.S. National Child Health Examination Survey (National Center for Health Statistics 1976). The NCHS charts were chosen for this purpose because they are based on large samples examined in the 1960s and 1970s by well-trained staff using standard techniques and because the data are cross-sectional and amenable to statistical analysis (Gracey 1987, World Health Organization 1978). Data from work in the Southern Highlands previ-