CURRENT ANTHROPOLOGY Volume 30, Number 1, February 1989

Hunter-Gatherers and Their Neighbors from Prehistory to the Present[1]

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It is widely assumed that modern hunter-gatherer societies lived until very recently in isolation from food-producing societies and states and practiced neither cultivation, pastoralism, nor trade. This paper brings together data suggesting a very different model of middle to late Holocene hunter-gatherer economy. It is argued that such foraging groups were heavily dependent upon both trade with food-producing populations and part-time cultivation or pastoralism. Recent publications on a number of hunter-gatherer societies establish that the symbiosis and desultory food production observed among them today are neither recent nor anomalous but represent an economy practiced by most hunter-gatherers for many hundreds, if not thousands, of years. Psychological and political reasons for Westerners' attachment to the myth of the "Savage Other" are discussed.

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1. An earlier version of this paper was read by Headland at the Fifth Annual Visiting Scholar's Conference, Southern Illinois University, April 15–16, 1988. We thank the following for written critical comments on earlier drafts: Alan Barnard, Matthias Guenther, Janet Headland, Susan Hochstetler, Karl Hutterer, Richard Lieban, Carol McKinney, and William Scott. We feel a special debt of gratitude to Bion Griffin and Agnes Estioko-Griffin for their substantial input over many years and to Leslie Sponsel for detailed comments on several earlier versions. Richard Crawford, Ronald Edgerton, Pedro Gil Munoz, Rudolf Rahmann, and John Slonaker assisted us in our archival research. We had help in translating certain documents from Hella Goschnick, Marianne Finkbeiner, and Hartmut Wiens (from German) and Charles Peck, William Scott, and Martha Shirai (from Spanish).

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The present paper was submitted in final form 18 v 88.

Westerners today commonly think of tribal peoples in general, and hunter-gatherers in particular, as primitive and isolated—incomplete, not yet fully evolved, and outside the mainstream. This view has been supported throughout this century by the writings of explorers, adventurers, missionaries, government agents, journalists, and, until very recently, anthropologists. Tribal peoples, and especially nomadic foragers, are often described as "fossilized" remnants of isolated late Paleolithic hunter-gatherers who have just emerged, through recent contact, into the 20th century. "Modern foragers tend still to be viewed in most of the current anthropological literature as sequestered beings whose very existence is due to the fact that they live beyond the reach of the trade routes of foreign powers. They are depicted as quintessential isolates, whose world was merely glimpsed in passing by explorers, and who remained remote until anthropologists penetrated their lives" (Schrire 1984:2).

The literature is full of recent "discoveries" of "isolated" tribal groups. Stereotyped descriptions of such peoples are found in popular writings such as Burroughs's *Land That Time Forgot* (1963 [1918]) and Gibbons's *The People That Time Forgot* (1981) and in anthropological works such as *Primitive Worlds: People Lost in Time* (Breeden 1973). Redfield's 1947 classic "The Folk Society," which idealizes tribal systems as "isolated," helps through its reprintings (most recently in Bodley 1988) to keep the myth alive in anthropology classrooms. Other anthropological examples are Huxley and Capa's (1964) *Farewell to Eden*, describing their visit to some Indians in the Amazon as "a trip that was to take us back thirty-five hundred years in time" (p. 13), and the 1984 educational film on the Mbuti pygmies titled *Children of the Forest* (see review by Morelli, Winn, and Tronick 1986). Schebesta's 1947 work on the Philippine Negritos is called *Menschen ohne Geschichte* (People without History), and the author of a 1981 book on the "Auca" of the Ecuadorian rain forest calls them an "isolated" people whose "way of life has changed little since their ancestors migrated from Asia across the Bering Strait" (Broenniman 1981:17).

Perhaps the best-known case, made famous by some 20 ethnographic films produced in the 1970s by Napoleon Chagnon and Timothy Asch, is that of the Yanomamo, a horticultural people of the Amazon. In the

third edition of what is probably the most widely read anthropology book in the United States today, Chagnon (1983:1) continues to portray these "fierce people" as living in pristine isolation from Western influence at the time of his initial visit to them in 1964—and this despite the fact that American missionaries have been working with the Yanomamo in his area since 1950 (pp. 3, 9). He even calls them "our contemporary ancestors" in the final sentence of his book (p. 214). (For a contrastive view of Yanomamo prehistory, see Colchester 1984; see also Ramos 1987.)

These works and many others perpetuate a view of tribal peoples as having lived until relatively recent times in isolation from their neighbors. There is, however, conclusive evidence that this "isolate model" is incorrect—that most, if not all, tribal peoples have typically been in more or less continuous interaction with neighboring groups, often including state societies, for thousands of years. We will call this view the "interdependent model" and support it with recent ethnographic descriptions of several hunter-gatherer societies traditionally considered "isolated" and "primitive."

We are not the first to question the myth of the primitive isolate. Spielmann (1986:305), for example, criticizes anthropologists for their "unrealistic and misleading" tendency to analyze egalitarian societies as closed systems, and Wolf (1982:18) points to anthropology's "mythology of the pristine primitive." It is part of what Strathern (1987) refers to as the "persuasive fictions of anthropology." Our argument here is in fact influenced by recent writings of several anthropologists who began to challenge it at about the same time as we did (e.g., chapters in the volumes edited by Leacock and Lee 1982, Francis, Kense, and Duke 1981, and especially Schrire 1984). More generally, our model was inspired by the writings of Roger Keesing, Frederick Dunn, and Karl Hutterer, who describe the prehistoric world as one in which tribal peoples have been in intense interaction with one another for a long time. Keesing calls the isolate model "the mosaic stereotype" and critiques it in detail (1981:111–22). He proposes instead a "systemic view" of the prehistoric tribal world in which simple tribal societies, complex societies, and even states coexisted and evolved together. He believes that most prehistoric foraging groups were parts of complex regional systems tied together by trade, exchange, and politics—that "for several thousand years the 'environments' of most hunters and gatherers have included surrounding agriculturalists, pastoralists, and in many cases kingdoms and empires" (p. 122). What we are calling the isolate model is a view of "a world that never existed" (p. 114). It continues, however, to be taught to anthropology students and to the public.

Case Studies

THE PHILIPPINE NEGRITOS

The Philippine Negritos, some 25 ethnolinguistically different groups numbering in total about 15,000, are hunter-gatherers in various stages of culture change. Most practice minor desultory cultivation and intense trade of forest products with non-Negrito agricultural populations. Two models of their prehistory may be proposed. The older and more generally accepted "isolationist stance" (to borrow a term from Gordon 1984:220) is that the first human inhabitants of the Philippines were some type of Pleistocene *Homo sapiens* that evolved some 20,000 years ago into the Negrito found in the archipelago today (Solheim 1981:25; Rambo 1984:240–41; Omoto 1985:129–30; Bellwood 1985:74, 113); that their original languages were not Austronesian; that they were "pure" hunter-gatherers; and that they had at most only infrequent contact with the Austronesian-speakers who began migrating into the Philippines around 3000 B.C.[2]

This isolate model is reflected, for example, in the report of a psychological anthropologist who studied the Ayta in western Luzon in the late 1930s that these Negritos, living "an isolated life in the equatorial rain forests, where millennia slip away with so little change . . . are probably living the way our own ancestors did some hundred thousand years ago" (Stewart 1954:23) and that "nowhere were the Negritos known to have agriculture" (p. 24). The anthropologist Eder (1978) describes the recent past of the Batak Negritos of Palawan Island in a similar framework, assuming without evidence that they "once lived in self-contained isolation" (p. 55), that "in the closing decades of the nineteenth century" they were still "isolated . . . from all but sporadic contact" with outsiders (1978:ix; see also 12), that they "began cultivating rice only during the latter part of the 19th century" (1978:58), and that trade of commercial forest products "to obtain desired consumer goods . . . may also have begun at this time" (p. 58). Warren (1984:3) also assumes that the swidden cultivation he observed among the Batak in 1950 was "obviously newly acquired from their neighbors." Fox (1953:175) noted that the Ayta Negritos "are today all shifting cultivators" but believed that they "were once able to live without recourse to cultivation" (p. 245), judging that their "association . . . with cultivated plants must be reckoned in a few hundred years—*excepting perhaps the taro and yams*" (p. 27, emphasis added). And Reynolds (1983:166) has recently stated, "For thousands of years, the Negritos in the tropical forests of Southeast Asia had managed to maintain a traditional life by withdrawing from prolonged contact with non-Negritos." Rai's (1982) ethnography presents Agta Negritos in northeastern Luzon as "relatively isolated" in pre-Hispanic and early Spanish times, with only "marginal" and "peripheral" trade with outsiders until the last two or three centuries (pp. 139–40, 145–46, 152, 154) and formal trade "at most only as old as the beginning of this century" (p. 156). He

2. The latest archaeological and linguistic evidence favors the hypothesis that the original homeland of Proto-Austronesian was Formosa and that a group speaking a daughter language of Proto-Austronesian arrived in the northern Philippines from Formosa around 3000 B.C. (Pawley and Green 1973:52–54; Blust 1978:220; Harvey 1981; Scott 1984:38–39, 52; Bellwood 1985:107–21, 130, 232). For recent opposing views on the location of the homeland, see Solheim (1984–85) and Meacham (1984–85).

surmises that "the Agta may have been practicing some degree of horticulture for the past two centuries" (p. 166).

Negritos, then, according to the isolate model, were pure hunter-gatherers with a near-Pleistocene economy throughout most of the Spanish era and perhaps even into the early part of this century.

We propose a more complex *interdependent* model that better represents the history of the Negritos in the late prehistoric period. Symbiotic interaction[3] with outsiders probably began soon after the first Austronesian-speaking people began migrating into Negrito areas—for some populations as early as 3000 B.C. For the proto-Agta groups in northeastern Luzon it may have been somewhat later but was likely well established by 1400 B.C., when humans who were probably not Negritos were cultivating rice in that area (Snow et al. 1986).

The Agta are the least acculturated of all Philippine Negritos (see Griffin and Headland 1985 for bibliography and Headland 1986, Reid 1987, 1988*a* and *b*, Headland and Reid n.d.). Called Dumagat by outsiders, the Agta ethnolinguistic groups of eastern Luzon typically reside in small nomadic camps in the rain forests of the Sierra Madre. The most salient activity of Agta men is hunting wild pig, deer, and monkey with bow and arrow. Among the Casiguran Agta, in a typical year about a quarter of the households cultivate tiny swiddens, averaging only one-sixth of a hectare in size. Rice is the main staple, wild starch foods being part of only 2% of meals (Headland 1987). Almost all of this rice is acquired by trading wild meat, minor forest products, or labor with neighboring agriculturalists; less than 5% comes from their own small fields.

Proponents of the isolate model would claim that these Agta bands were until recently almost completely separated from non-Agta farming populations, since even during Spanish times very few non-Negrito people lived in that inhospitable area, with its rugged mountains, stormy weather, and rough seas. They would argue that the Agta's involvement in agriculture, desultory as it is, is a recent "contamination" resulting from contact with farmers and the pressure of shrinking hunting territory. Negritos have been widely described as "people without cultivation" even into this century (e.g., Borrows 1908:45–46). Estioko-Griffin and Griffin (1981:55), for example, present the agricultural practices of the Agta they studied in the 1970s as "new," with the more acculturated Agta only "in their second or third generation as part-time marginal [swidden] farmers." They state that Agta cultivation practices are still little known and that in the traditional Agta system there was a "lack of use of cultigens" (p. 61). The ethnohistorical, archaeological, linguistic, and botanical evidence fails to support these views.

Ethnohistorical evidence. Early reports substantiate beyond question that the Agta were making swiddens and that symbiotic relationships with nearby farming communities were well established throughout the Spanish period. When Dean C. Worcester, U.S. Secretary of the Interior of the Philippines, made a quick steamer trip down the east coast of Luzon in 1909, he depicted the Agta on the remote northeast coast as primitive and untouched: "In this region, and in this region alone, the [Agta] Negrito . . . has had little or no contact with white men or with Christian [i.e., non-Negrito] Filipinos" (Worcester 1912:833). It is clear, however, that he failed to grasp the significance of the many trade items he found in their abandoned lean-tos: coconut shells, clay pots, metal fishhooks, metal arrowheads, bolos, and commercial cloth (p. 841). Furthermore, one of his photographs "taken [in these Agta camps] on the northeast coast of Luzon" (p. 837) shows a wooden mortar for pounding corn or rice, a small clay pot, and a tin can.[4] In 1909 the Agta bands in this area were probably the most remote and "primitive" hunter-gatherers in the Philippines, but the trade goods just mentioned show that they were certainly not independent of other Filipinos or of agriculture.

A number of 18th-century reports make clear that the Agta were involved in intense symbiosis, including patron-client relationships, with Christianized farmers and trading forest products for rice, tobacco, metal tools, beads, and pots (AFIO MS 89/60 1745; Santa Rosa 1746, cited in Perez 1928:87, 94, 106 and 1927:294). It is clear from many other records that this system was widespread by the 19th century (see, e.g., Semper 1861:252, 255–56; 1869:51–52; de Medio 1887, quoted in *Report* 1901:391; Platero n.d., quoted in *Report* 1901:391; Segovia 1969 [1902]:103; *Eighth annual report* 1903:334; Garvan, March 12 1913, in Worcester 1913:105–7; Lukban 1914:2, 4, 6–9; W. Turnbull 1929:177, 237–38; 1930:782, 783; Vanoverburgh 1937–38:149, 922, 928; Lynch 1948; Amazona 1951:24; Tangco 1951:85; and Schebesta 1954:60, 64). Likewise, there is solid evidence that the Agta were making swiddens of their own by the 1740s (AFIO MS 89/60; Santa Rosa 1746, cited in Perez 1928:87, 88, 92–93, 96), in the 19th century (Semper 1861:252, 255–56; de Medio 1887 and Platero n.d., cited in *Report* 1901:390–91), and in the early years of this century (Worcester 1912:841; Lukban 1914:2; Whitney 1914; Turnbull 1930:32, 110, 782, 794; Vanoverburgh 1937–38:922, 927; for English translations see Headland 1986).[5]

Archaeological evidence. The archaeological evidence establishes that extensive international trade in forest

3. At least seven types of symbiosis are recognized (see, e.g., Sutton and Harmon 1973:184): mutualism, cooperation, commensalism, amensalism, competition, predation, and parasitism.

4. This photograph, taken on August 30, 1909, is in the Worcester Photographic Archives of the Museum of Anthropology, University of Michigan, File No. I-Z-1. It shows another trade item, a small clay pot to the right of the mortar, that was cut from the reproduction published by Worcester (1912;837).

5. Eder (1987:23, 45–46, 48–49) cites a number of archival references showing that the Batak Negritos also engaged in interethnic trade and some agriculture during Spanish times. Endicott (1983:224–26; 1984:30) cites 19th-century references indicating that trade, labor barter, and occasional horticulture "have long been regular features of the economies of the nomadic Semang (Negritos)" in Malaysia (p. 30). Brosius (1983:138; see also 139–40) indicates that the Ayta Negritos had been making swiddens "for a very long time, almost certainly prior to the arrival of the Spanish."

products has been going on throughout much of insular Southeast Asia for at least the last thousand years and that nomadic forest peoples, including Negritos, have been the collectors and primary traders (Hall 1985:1–2, 21, 23–24, 226). Dunn (1975) argues that such trade in Malaya, mostly to China, began in the 5th century A.D. Rambo (1981:140) agrees, saying that Malaysian Negritos may have evolved into specialist forest collectors for maritime traders as early as 5,000 years ago. Hoffman (1984, 1986) argues that Chinese sailors were trading for forest products in Borneo before the 5th century. Their arguments dispel any suggestion that Paleolithic people were living isolated in the jungles of these islands on the eve of the Europeans' arrival.

Hutterer's (1974, 1976, 1977, 1983) description of extensive prehistoric trade in the Philippines supports our interdependent model for these islands. He and others (Fox 1967; Landa Jocano 1975:145–53; Scott 1981; 1983; 1984:63–84) review the evidence for trade between the Philippines and China by at least the time of the Sung dynasty (A.D. 969–1279), with Negritos having intense symbiotic relationships with outsiders at that time (Hutterer 1974:296). Mindoro, in the central Philippines, was part of the international Asian trade routes by A.D. 972 (Scott 1983:1) and "was itself the central port for the exchange of local goods on a Borneo-Fukien route" by A.D. 1270 (p. 15). According to Scott, "the total impression is one of continual movements of rice, camotes, bananas, coconuts, wine, fish, game, salt, and cloth . . . to say nothing of iron, gold, jewelry, porcelain, and slaves" (p. 24).

Looking specifically at the Agta areas of northeastern Luzon, archaeological studies indicate that there were non-Negrito populations here long before the Spanish era. Peterson (1974*a*, *b*) excavated what was almost surely a non-Negrito habitation site in the center of today's Agta area that he dates at 1200 B.C. or earlier and considers "incipient agricultural." It has yielded pottery, mortars, and evidence of the reaping of grain (1974*b*:131, 161, 162, 225, 227). Another archaeologist presents evidence that humans were living in another part of this area by the end of the Pleistocene and by 5000 B.C. were using "grass reaping blades" (Thiel 1980). These blades should probably be associated with a Negrito population; the brass needle found at the same site in an archaeological level dated 2000 B.C. and a burial cave dated 1500 B.C. are probably not Negrito.

The evidence is solid that people were cultivating rice in northeastern Luzon by 1400 B.C. (Snow et al. 1986). This site is also on the western edge of today's Agta area and just a few kilometers from Thiel's. It is probable that the ancestors of today's Agta were interacting with these farmers by the middle of the 2d millennium B.C. Finally, recent archaeological research establishes that there were ceramic manufacturing cultures in northeastern Luzon as early as around 3000 B.C. (Snow and Shutler 1985:1). The archaeological record, then, suggests that rice-farming populations and Negrito hunters were living within a day's walk of each other in northeastern Luzon for at least the last 3,000 years.

Linguistic evidence. Our interdependent model proposes that these Agta hunters carried on intense interethnic relationships with Austronesian-speaking farmers at the earliest periods. The linguistic support for this view has been outlined elsewhere (Headland 1986:17–19, 174–78; Reid 1987, 1988*a*, *b*; Headland and Reid n.d.) and will be only briefly reviewed here.

All Philippine Negrito groups speak languages that, like those of their non-Negrito neighbors, belong to the Austronesian language family. These Negrito languages are, for the most part, unintelligible to their agricultural neighbors; they are not simply dialects of those neighbors' languages as has frequently been suggested. They are neither aberrant nor distinctive as a group among Philippine languages. Now, since Austronesian-speaking people did not begin migrating into the Philippines until around 3000 B.C., and since the ancestors of today's Negritos had lived in those islands for thousands of years before that time and therefore presumably spoke languages that were not Austronesian, the question is when and under what circumstances they gave up their original languages and began speaking Austronesian ones.

At some time in the prehistoric past, the ancestors of today's Negritos must have established some type of contact with the Austronesian-speaking immigrants in the course of which they lost their own languages and adopted those of the newcomers. In order for a language switch of this magnitude to have occurred, more was probably involved than trade. There must have been periods of intimate interaction long enough for bilingualism to develop and then for the original Negrito languages to be replaced. The linguistic data suggest that all this happened a very long time ago. While it is theoretically possible for early Negritos to have abandoned their original languages in the space of three or four generations, the degree of language differentiation that has subsequently taken place could not have occurred in such a short period of time. This divergence implies a period of independent development of well over a thousand years in the case of the Negrito languages that are today most similar to their non-Negrito sister languages and of many thousands of years in the case of those that are least similar.

Our hypothesis, then, is that well over 1,000 years ago, and quite possibly 3,000 years ago, the ancestors of today's Negritos were interacting with non-Negrito speakers of an Austronesian language. This interaction was so intense that the Negritos adopted the language as their own. Later these ancient Negritos separated themselves from their non-Negrito neighbors but retained the language they had borrowed from them. Over time, through the normal processes of language change, separate dialects and finally separate daughter languages developed. There is no other plausible explanation for the linguistic facts. For example, some Negrito languages have retained archaic features, such as case-marking particles and verbal affixes, that are not found today in most other Philippine languages but existed in some very early daughter languages of Proto-Austronesian. These archaic forms indicate that these Negrito languages were

first learned when such forms were still present in the protolanguage spoken by the non-Negrito people with whom they were then in contact. (For details see Reid 1987, Headland and Reid n.d.)

Botanical evidence. The reason that prehistoric Negritos attached themselves so readily to non-Negrito farming populations was, we suggest, a critical nutritional need. As one of us has argued elsewhere (Headland 1987), tropical rain forests are not the food-rich biomes they are sometimes assumed to be. While faunal resources are usually sufficient there, these may not provide sufficient lipids to supply the nutritional needs of humans in the absence of wild plant starches. The late Pleistocene human populations of the Philippines seem to have been living in areas that were then wooded savannas, not rain forest (Thiel 1980; see Scott 1984:14, 142 for a review of the evidence). The prehistoric Agta probably did not move into the rain forest before they had at least seasonal access to cultivated starch foods.

We propose, then, that the symbiotic relationship we find today between tropical forest hunter-gatherers and farmers evolved long ago as an adaptive strategy for exploiting the tropical forest. This aspect of our model accords well with Rambo's (1988) "adaptive radiation model" for the ethnogenesis of Southeast Asian Negrito culture: that Negritos evolved culturally into what they are today as they moved into the forest to collect wild products to trade with agriculturalists and overseas traders for tools and starch food.

The accumulation of evidence, then, leads us to favor the interdependent model for the history of the Philippine Negritos.[6] Some bands possibly did live seasonally far from and independent of non-Negrito farming populations, but even these groups moved at times to locations in which they could trade with farmers. Most Negritos, however, interacted intensely with their Austronesian-speaking neighbors to the extent that they not only learned the languages of those neighbors but actually adopted them as their own. The interdependence of Negritos and farming populations observable today has existed much longer than most scholars have thought. There is no question that the ancestors of the present-day Agta were at one time Paleolithic hunter-gatherers. What we are arguing is that this Stone Age life-style ended long ago, probably by the middle Holocene, and that prehistoric Negritos probably moved into the Neolithic at more or less the same time as their neighbors.

6. To advocate the isolate model would require hypothesizing either that the Negritos were not the original inhabitants of the Philippines but rather immigrated there concurrently with the various groups of Austronesian immigrants some 5,000 years ago or that the homeland of Proto-Austronesian was the Philippines. The latter hypothesis would imply that there had always been both Negrito and non-Negrito peoples in the islands, both groups having evolved biologically from some earlier type of *H. sapiens* or perhaps even *H. erectus*, and that their earliest language was Proto-Austronesian. To our knowledge, no one has seriously proposed either of these hypotheses.

THE SAN

Since the appearance of the 1980 film *The Gods Must Be Crazy*, millions of moviegoers have been convinced that the San Bushmen are the sweetest, most innocent, and most contented people on earth—still lacking, in this age of airplanes and Coke bottles, any knowledge of property, money, or the outside world. Other powerful media continue to perpetuate this myth. A 1985 article in *Newsweek* (January 28, p. 66) depicts the San as untouched until, "early in this century, they encountered Civilization." In a recent human ecology text (Campbell 1983) this view is reinforced: "San lifestyle probably changed little over the course of hundreds of thousands of years" (p. 124). In accord with this is another, more recent *Newsweek* article reviewing the latest scientific theory on modern man's common ancestor, a woman they are calling Eve who lived about 200,000 years ago and "probably [lived] much like today's Bushmen in southern Africa" (January 11, 1988, p. 51). Johnson and Earle (1987:38–54) make no mention of the !Kung San's involvement with outsiders or with food production, describing them as pure foragers and asserting that "until the mid-1960's, the San were relatively isolated from the outside" (p. 38). Konner and Shostak (1987:11) extend this date another decade, saying that "the !Kung San . . . were subsisting primarily by traditional methods of hunting and gathering into the 1970s," and suggest that their life-style may be "relevant to the interpretation of some aspects of human adaptation during the paleolithic period of human evolution." (For a review of many other references describing the San in isolationist terms, see Hitchcock 1987.)

When Richard Lee first described the !Kung San in the 1960s, he too presented them in terms of the isolate model. The !Kung were in fact popularized through Lee's writings and the Marshalls' (e.g., Thomas 1959) as the classic example of "real" hunter-gatherers because of their apparent isolation and independence of food production. But it was Lee himself who later discovered that "the !Kung were no strangers to agriculture and pastoralism" (Lee 1979:409; see also Lee 1984:135). He found that the !Kung had been doing no planting at the time of his first visit (1963–64) simply because of a drought; on his return (1967–69) he found that 51% of the men planted fields (p. 409; see also 1976:18; 1981:16).[7] Wiessner describes, too, the way some extremely acculturated !Kung groups may return to what appears to the outsider to be a completely unacculturated state—a "common occurrence" among them (1977:xx). This observation is supported by Guenther (1986). According to Wiessner "it was impossible . . . to infer anything about degree of acculturation of a family from current lifestyle." Gordon (1984:219) states the problem clearly: "It is not that Lee is wrong in his representation of reality. Indeed he has shown himself to be

7. This is a figure much higher than for Casiguran Agta men, of whom 24% did some minor cultivation for themselves in 1983, an average year (Headland 1986:483).

quite flexible on the issue of contact and interaction. . . . the problem lies in how others interpret Lee's statements."

In fact, Lee's 1984 book on the !Kung shows how closely tied the Dobe !Kung were to food producers when he first encountered them in 1963. The 466 Dobe !Kung were then living in nine camps, eight of which were within a 20-km radius. What students often fail to note is that there were then living within that same area 340 blacks and thousands of livestock. In eight of the nine camps, the !Kung were living *with* black herders, for whom they worked part-time as herders. Only at one camp, Dobe, were !Kung living with no non-!Kung or livestock, and even these "frequently visited" the blacks at Mahopa, only 10 km away, "to ask for some milk" (pp. 16–17, 123). In 1963 trucks were passing through the Dobe area—"about one truck every six weeks" (p. 18)—and a minority of Dobe !Kung men had worked in the mines at Johannesburg (p. 138). In spite of this, Lee sometimes overemphasizes the "relative isolation" of the !Kung (pp. vi, 129). It seems an overstatement for him to claim that the Dobe !Kung were living "almost entirely by hunting and gathering" (p. vi) when he found them or that "by 1960 the !Kung still remained hunter-gatherers without herds or fields" (p. 119, but see p. 135, where he acknowledges that most !Kung had practiced both herding and agriculture in the past). And he continues to reject the thesis of Schrire (1980) and Wilmsen (1983) that !Kung society had been fundamentally altered by interaction with herders many hundreds of years ago (p. 130).

From Silberbauer's (1981) description of the neighboring G/wi San, they seem as close to the archetype of the "isolated" hunter-gatherer society as one could hope to come. Brooks (1982), however, casts doubt on this characterization. She points (personal communication, 1986) to a statement by Tanaka (1976:100) that the same G/wi, whom Tanaka studied only a year after the period represented by Silberbauer's study, "do keep herds of goats and donkeys." According to Wilmsen (1983:17), "Accumulating evidence overwhelmingly renders obsolete any thought of San isolation even before European colonial intrusions into their native arenas. Early Iron Age agropastoralist economies were active in all parts of the Kalahari and its surroundings at least for the past millennium. . . . To ignore this is illusion."

Schrire (1980), who believes that the San have been practicing sporadic pastoralism for hundreds of years, reviews a good deal of evidence that contradicts any theories about the existence of pure hunter-gatherers anywhere in southern Africa. Denbow (1984:178) shows that "foragers and food producers have been enmeshed in networks of interaction and exchange for 1,000 years longer than was previously suspected. Over 1,200 years ago these networks reached into the heart of the Dobe !Kung area" (see also Denbow 1986, Denbow and Campbell 1986, Denbow and Wilmsen 1986). Volkman (1986) presents the San as having long practiced a mixed economy that included crop planting and animal husbandry as well as hunting and gathering. Finally, after reading Gordon's (1984) startling descriptions of the intense interaction between African herders and Kalahari San in the last hundred years, it is hard to believe that the groups described by Silberbauer and Tanaka were as isolated and "untouched" as they seem to have thought.

These groups are indeed "hunter-gatherers," but in the sense of Leacock and Lee (1982*a*:4, 7–9)—not because they are isolated primitives who eat only wild foods and not because of their mode of *subsistence* (i.e., hunting, fishing, gathering) but because of their unique foraging mode of *production*, characterized by sharing, communal ownership of land and resources, and egalitarian political relations (Lee 1981). Today's hunter-gatherers engage in minor food production and eat traded starch foods, "but their relationship to their environment continues to be predatory and opportunistic" (Keesing 1981:512). Above all, as Guenther (1986) points out, they manifest flexibility and adaptability, as the same bands may move sequentially over a generation or two from serfdom to food production to mining to pure foraging to employment as mercenaries as they adjust to ecological and political changes in their environments.

As Parkington (1984:172) says, "We know now . . . that all hunter-gatherers in southern Africa have shared the landscape for at least 1500 years with pastoralists or agriculturalists." Wilmsen (1983:16) cites a wealth of data to support this view for the Kalahari and says that "in the nineteenth century, the !Kung homeland was already laced by a network of trade routes supplying local products to the European market." Denbow (1984:188) points out that, though anthropologists like Lee, Silberbauer, and Tanaka have tried to find independent foraging groups to study in the Kalahari, "in fact there has probably been no such thing here, in an historical or processual sense, for almost 1500 years." The recent reviews by Hitchcock (1987) and Denbow and Wilmsen (1986) on the issue support the idea of hundreds of years of San interethnic symbiosis. We may accept Vierich's (1982:213) proposition that "if the hunting and gathering way of life has survived in the Kalahari, it is not because of isolation."

THE CENTRAL AFRICAN PYGMIES

Moving north to central Africa, we find Campbell (1983:32–33) describing the Mbuti pygmies as until recently "independent forest groups." For him, "there is no doubt of [the] ability [of the Mbuti] to survive without [trade]." Turnbull, of course, argued a quarter of a century ago that the Mbuti were not economically dependent upon farmers because they could and sometimes did live independently on wild foods (1963:35; 1965:34; but see Vansina 1986:436). Indeed, he maintains this position today (1983, 1986), despite the failure of anthropologists to find a single case—either ethnographic or in the archaeological record—of a pygmy group living independently of village farmers anywhere in Africa and the evidence that the African rain forests would not provide sufficient wild foods to sustain hu-

man foragers for long periods (Hart and Hart 1986, Headland 1987, Bailey and Peacock n.d.).

Cavalli-Sforza (1986) paints a somewhat less isolationist picture of pygmy life. While he suggests that pygmies (albeit imperfectly) represent Upper Paleolithic living conditions (p. xxii; see also pp. 378, 422, 424, 425), he does acknowledge that "there probably are no Pygmies living in complete isolation" (p. 369) and "seem to be no Pygmies who have truly zero contact with African farmers" (p. 422; see also p. 362). He argues, however, that they "continue living in an economic system presumably similar to that of our earlier ancestors" (p. xxii), "have not, or only very recently, adopted farming as a major source of food" (p. 18), "live, or presumably lived until a short while ago, exclusively as hunter-gatherers" (p. 20), and "live still basically unaffected by contact with the modern world" (p. 422). Although he points out that Bantu farmers "probably made early contacts with Pygmies . . . 2000 years ago or earlier" (p. 362), he minimizes the effect of those contacts on pygmy culture and feels that pygmies "retain substantial independence" even today (p. 362).

In contrast, Bahuchet and Guillaume (1982) argue for a long history of interethnic trade between the African pygmies and their agricultural neighbors. Concerning the Aka, they call into question "the widespread image of pygmies living confined and isolated in their forest cocoon," saying that "the linguistic affiliations of Aka, and the long process of differentiation, imply the existence of ancient contacts which must have been more extensive than mere occasional exchanges of material goods" (p. 191; see also Bahuchet and Thomas 1986, Bahuchet 1987).[8] Morelli, Winn, and Tronick (1986:744) go a step farther to propose that "forest living for the Mbuti may be a relatively recent phenomenon" (after they were forced into the forest by warring tribes).

OTHER HUNTER-GATHERER GROUPS

Recent evidence suggests that—with the possible exception of the arctic and subarctic peoples—most late Holocene hunter-gatherer societies were not isolated at all but engaged to some degree in interethnic trade with neighboring societies and, in many cases, part-time food production. There is some evidence of intense trade, at least in Europe, during the late Pleistocene. The archaeologist Olga Soffer, referring to Cro-Magnon peoples, has recently been quoted as saying, "You have something like a prehistoric Hudson Bay Co.," with elaborate networks of exchange between clans (*Newsweek*, November 10, 1986, p. 71). Soffer (1985) argues for much more complexity in social organization among Upper Pleistocene hunter-gatherers than has heretofore been recognized. For the Holocene, Wobst (1978) cites several references to widespread interregional trade among "late paleolithic hunter-gatherers" on several continents. McKinley (n.d.) has a book in press to be titled *Stone Age World Systems*, and Gregg (n.d.) is editing a collection of papers on interaction in small-scale societies. Both volumes will emphasize the worldwide extent of the interaction model we propose here. Several papers in a volume edited by Francis, Kense, and Duke (1981) show the complexity of long-range trade networks in Amazonia in prehistoric times. The papers collected by Mathien and McGuire (1986) describe prehistoric networks linking Mesoamerica and the Southwest. Schrire (1984:14–17) and Speth and Spielmann (1983:20) review the writings of others on the idea of more general interethnic trade in North America long before the arrival of Europeans, including Eskimo interchanges across the Bering Strait. For insular Southeast Asia in particular, Dunn (1975:120–37) reviews evidence suggesting that inland-coastal trade was established on the Malay Peninsula by 8000 B.C. and that by 2000 B.C. Malayan forest peoples living far inland may have been tied into overseas trade networks. And Hoffman (1984, 1986) dispels any idea that the hunter-gatherers in the interior of Borneo were independent "wild people of the woods," arguing that these "Punan groups . . . arose initially from the demand for various jungle products desired by Chinese" more than 1,000 years ago (1986:102). According to Hoffman, "it is time for anthropologists to stop thinking of Borneo as though it were another New Guinea" (p. 103).

We should not, then, continue to consider the "hunter-gatherers" of the last 2,000 years or so as isolated or as people who eat no domestic foods (Coon 1971:xvii), practice strict "Pleistocene economies—no metal, firearms, dogs, or contact with non-hunting cultures" (Lee and DeVore 1968:4), live in patrilocal bands (Service 1971), or have no agriculture of any kind (Murdock 1968:15). As Lee and DeVore have stressed, such definitions would effectively eliminate most, if not all, of the foraging peoples described over the last century as "hunter-gatherers." Even prehistoric Australian Aborigines evidently practiced various types of simple plant cultivation, including burning, seed planting, replanting of wild yam tops, fertilization, and irrigation (Campbell 1965).

Explaining the Persistence of the Isolate Model

A French journalist who visited an Agta Negrito band in the northern Philippines for a week in 1979 reported that there was "no evidence that the tribe practiced any kind of agriculture" (Evrard 1979:38) and described their fear of his mirror, tape recorder, and camera, "obviously the first they had ever seen"—considering himself "the first white man to intrude upon them" (p. 39). A 1981 report on these same Agta by the Commissioner to the Non-Christian Tribes for Cagayan Province (appointed by the governor and given that title in the late '70s) describes

8. Berry et al. (1986:26) make the same argument for the Biaka pygmies. A brief review of other such linguistic references may be found in Cavalli-Sforza (1986:367–69). In this light, Turnbull's (1983:21) argument that the Mbuti recently "lost their own language and adopted those of the immigrant peoples" is unacceptable.

them as a "Newly Found Tribe" of "cannibal[s] in the upper Sierra Madre" and even quotes one Agta as saying that "the most delicious meat is the liver of human beings" (Cortez n.d.). He describes them as "the most primitive, wild, fierce and dangerous group . . . a generation from the Stone Age" and speaks of their having no clothes, being fond of eating raw meat, and being ignorant of days, weeks, months, and years; their children, he says, are "unwanted and unloved," and "idolatry and adultery are supreme." These are the same Agta among whom one of us had been living since 1962. They have, of course, long been quite used to white people, cameras, and mirrors, they love and care for their children, and they have interacted with outsiders for many hundreds of years.

Ethnocentric and racist statements such as these still appear in print, and the prejudice they reflect continues to be widely held (for summary compilations of examples, see Headland 1986:445; Headland and Reid n.d.; Hoffman 1986:2–4, 8, 46, 57, 95–96; Rosaldo 1982; Guenther 1980). While few if any anthropologists today would accept any part of the 19th-century evolutionary theories of Tylor and Morgan or of Frazer's creation of "an atmosphere of romantic savagery" (Strathern 1987:256), many lay people continue to believe in the anthropological fiction that Tylor and Morgan codified—that human peoples evolve culturally from savagery to barbarism to civilized status. Implausible as this viewpoint is in the light of new archaeological, linguistic, archival, and ethnographic data, it continues to overshadow recent scientifically sound analyses based on these data.

Some anthropologists have recently attempted to explain why this myth of the "Savage Other" persists. Pandian (1985:63), who reviews anthropology from the perspective of the history of Western thought, concludes that "the psychological needs of people are met by the symbol of the wild man." Fabian (1983:164) takes a more political position, showing that anthropology tends to view contemporary tribal cultures as if they were separate from us in time and place. He sees this as a political use of anthropology that maintains and reinforces a relationship between dominant and dominated societies. He views what we call the isolate model as an ideological tool for exploitation and oppression—for "intellectual imperialism." Dove (1983:85) discusses the persistence of the belief that swidden cultivation is primitive and wasteful and that swiddeners (no less than hunter-gatherers) live in isolation, "completely cut off from the rest of the world," and, with Fabian, sees the reason as political: "These myths . . . have been used since colonial times to justify the exploitation of a . . . vulnerable peasantry by . . . [a] more powerful urban and governing elite" (p. 96).

Behar (1987) shows how the Spanish colonizers of northern Mexico emphasized the savagery of local hunter-gatherers as a justification for driving them off desired lands or enslaving them. Many Spanish settlers, in their petitions to authorities in Mexico City and Spain, described the wildness and brutal nature of the Amerindians and proposed genocide as a solution. Rosaldo (1978:242) notes the same situation in the Philippines and sees "the dominant motive . . . [as] control"; colonizers view indigenous lifeways as dangerous to the goals of "civilization" in that they threaten the establishment of roads and towns in frontier areas. Guenther (1980:135) reviews the 18th- and 19th-century pejorative attitudes and destructive actions of European colonists against the San in southern Africa and accounts for the persistence of negative stereotypes as an "ideological mechanism . . . [that] justified the denial of land, freedom and life to the Bushman." Volkman (1986) reports that the Namibian government continues to treat the San in the same way, making political decisions for them based on their "primitiveness." Finally, Taussig (1987) shows how the colonial representation of the Colombian Indian as Wild Man led to the torture and killing of Indians by colonists in the early years of this century.

Sponsel (1985:96–97) suggests that anthropologists in particular perpetuate the isolate model because of the high value they place on the "primitiveness of the culture studied," "the traditional in 'primitive' culture," "cultural purity," and the depiction of the people as "our contemporary ancestors." On the same theme, Martin (1986:420) says that the folklorization of ethnographic inaccuracies is the result of "exoticism" in anthropology. Ramos (1987) believes that this is why the Yanomamo are so famous today, at the same time espousing Fabian's political explanation (pp. 298, 299). Rosaldo (1982), focusing on the Philippine Negritos, suggests that they are mythologized as "utter savages" to make them more fascinating "objects of scientific value." He is probably right in saying, "Had Negritos not existed perhaps they would have been invented" (p. 321).

Wobst (1978:304) argues that anthropologists "reinforce the overwhelming ethnographic stereotype that hunter-gatherers articulate exclusively with local variability, and that regional and interregional process among hunter-gatherers is a symptom of degeneration and culture contact." It is his view that "all hunter-gatherers in the ethnographic era were intimately tied into continent-wide cultural matrices" (p. 303) but that "the literature is remarkably silent" (p. 304) on this because anthropologists have done a kind of "salvage ethnography" on them, trying to reconstruct the "ethnographic present—the imaginary point in time when the studied populations were less affected by culture contact." In short, Wobst says, anthropologists have filtered out behaviors involving interaction between hunters and their surrounding nation-states, and therefore "the ethnographic literature perpetuates a worm's-eye view of [hunter-gatherer] reality." Cowlishaw (1987) shows for Australian Aborigines that anthropologists have denied their history and authenticity by focusing on the "traditional" in their cultures.

Wolf (1982:14) blames functionalist anthropology, with its static view of cultures, for misleading anthro-

pologists into treating tribal cultures as "hypothetical isolates." We suggest that the more ecologically oriented neofunctionalists of the 1970s have made the same mistake. As Mintz (1985:xxvi–xxvii) explains,

> Cultural or social anthropology has built its reputation as a discipline upon the study of . . . what are labeled "primitive" societies. . . . [This] has unfortuantely led anthropologists, . . . occasionally, to ignore information that made it clear that the society being studied was not quite so primitive (or isolated) as the anthropologist would like. . . . [thus giving the impression] of an allegedly pristine primitivity, coolly observed by the anthropologist-as-hero. . . . One anthropological monograph after another whisks out of view any signs of the present and how it came to be.

Conclusion

The historical and philosophical reasons for Western civilization's fascination with savagery may be more complex than all of these suggestions combined. As we learn from Stocking (1987), this Western world view of the Savage Other probably evolved from an 18th-century Victorian anthropology, and aspects of this view continue to be fed by both anthropological writings and popular works today.[9]

We have argued that small indigenous societies are as fully modern as any 20th-century human group, that many hunter-gatherer groups have been involved in minor food production for thousands of years, and that many of these latter were also participating in interethnic and possibly international trade long before the 16th-century European expansion. The foraging societies we know today remain in their "primitive" state not because they are "backward" but because they are kept there by their more powerful neighbors and because it is economically their most viable option in their very restricted circumstances. Westerners have chronically failed to understand such societies because they continue to see them as fossilized isolated hunters rather than as "commercial foragers" carrying on a life-style not in spite of but because of their particular economic role in the global world in which they live. Until this anthropological bias is corrected, our image of hunter-gatherer culture and ecology will remain incomplete and distorted.

9. An example of this was the worldwide excitement created in 1971 when a group of scientists claimed to have found a lost Stone Age tribe of Tasaday cavemen in a dense rain forest in the southern Philippines—a story that, according to several 1986 reports, may have been a hoax (see e.g., *Newsweek*, April 28, 1986, p. 51; *Asiaweek*, August 31, 1986, pp. 60–61; *Anthropology Today* 2[6]:23–24; see also the official position of the University of the Philippines Department of Anthropology [University of the Philippines 1988]).

Comments

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Headland and Reid do a good job of increasing our appreciation of cultural variability among hunter-gatherers and airing justifiable analytical concerns. Having applauded the substance of their contribution, I would like to turn my attention to its "reprimanding" tone, which is typical of the contemporary wave of criticism directed at past studies of simple human collectives. Criticisms of hunter-gatherer cultures as pristine isolates have become so pervasive as to command the attention of the "Research News" section of *Science*, in which the "very simple but persuasive model of hunter-gatherer life" is challenged and Irven DeVore acknowledges the error of viewing such societies as pristine (Lewin 1988). This debunking should be directed more at media images of the modern noble savage than at the paradigms that became part of the anthropological scene in the sixties. As a participant in the Ottawa symposia on band organization (1965) and cultural ecology (1966) and the Chicago symposium "Man the Hunter" (1966), I find it difficult to dismiss them as having fostered the idea of hunter-gatherers as "primitive isolates." I feel that, while present in the studies of simple societies of the last several decades, the "affluent savage isolate" is receiving far too much press relative to the total ethnographic and ethnological context.

While the data base generated and utilized by Headland and Reid is fundamentally good, their treatment of the Western "Savage Other" myth and their claim to be the rightful heirs to evolutionary-adaptive theory are questionable. In their use of teleological language they display the very Eurocentrism they decry in others. What we need are pliable categories of ecological adaptation that refer to population/space ratios. On such a basis one would postulate that many thousands of years ago some small-scale societies ran out of the space necessary to subsist by food collection and had to shift to the more laborious and less reliable food production.

We must perceive variability and predictability—change and resistance to change—as intrinsic and complementary tendencies of human adaptiveness and, therefore, hold that culture change and the attendant variability are universals. We must study rates and forms of change, not argue over its existence, and accept the fact that biocultural viability implies the coexistence, not the mutual exclusiveness, of "identifiable units." At a more specific level, it is important that we acknowledge the difference between material and social need-resolving technologies so that, noting the ease with which material cultural elements can cross societal boundaries, we can marvel not at the interdependence and "impurity" of small-scale societies but at their persistence.

In the analysis of human adaptiveness, useful categories and labels are rooted (within the confines of finiteness and relativism) in concepts such as integrated change, probabilism, and change meeting change. For instance, the advent of storage in material technology and of complex kinship forms in social technology should be recognized as indicators of an overall trend from simple to complex, not as revolutionary inventions that made civilization possible. Headland and Reid should have put less stress on chastising the proponents of unpalatable views and more on demonstrating the presence, historically and prehistorically, of more interdependence of food collectors and food producers than had been thought. I favor synthetic approaches couched in positive terms, as exemplified by the writings of Barnard and Ingold, in which criticism is offered in a manner that engenders constructive dialogue rather than polemics.

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This is a good article that challenges the isolationist view of hunter-gatherer societies. Despite a huge amount of evidence to the contrary, there remains the tendency, deliberate or unconscious, to see recent hunter-gatherer lifeways as representative of an ancient, uninfluenced and unchanging past. But clearly this model has also been assumed to apply to many small-scale horticulturalists. Diamond (1988), for example, stresses the extreme isolation of parts of New Guinea due to the difficulty—at least for Europeans—of travel. This may well explain why the Dani did not have face-to-face contacts with Europeans until the 1938 Archbold Expedition, but it does not mean that they were not influenced by their neighbors, an impression he conveys in discussing variations in materials, art forms, and language: "New Guinea shows linguists what the world used to be like, with each isolated tribe having its own language, until agriculture's rise permitted a few groups to expand and spread their tongue over large areas" (p. 31). In all fairness I must point out that Diamond, not an anthropologist, considers village isolation to have been generated by intergroup warfare rather than simply the difficulty of travel. The point is, however, that cultural and linguistic diversity in New Guinea is due to interdependent relationships among often hostile neighbors who have forced upon each other a degree of social isolation that otherwise would not have existed. These and similar types of relationships tend to generate tribal boundedness (Fried 1975) and may have led some anthropologists to assume, incorrectly, that the particular groups they were studying after hostilities ended had little to do with surrounding peoples. Whatever the causes of warfare, groups appear to have known much more about the world beyond their villages than reports suggest. For instance, the Dani are said to have had an "obsession with cowrie shells" that were obtained from distant areas.

If in 1988 scholars can still write that there were "isolated" horticulturalists until 50 years ago, how much more isolated hunter-gatherers must appear to some! Naive romanticism, the value of emphasizing the primitiveness of a people, the theoretical neatness of closed-system analysis, and an emphasis on salvage ethnography to gather information on an assumed aboriginal past in which time is collapsed into the ethnographic present are among the reasons given by Headland and Reid for the isolate model's appeal. In regard to the last of these, while historical research has demonstrated ethnological misrepresentation, sometimes it has not gone far enough. Once aboriginal baseline sociocultural systems were reconstructed through ethnohistorical techniques, scholars, particularly nonarchaeologists, treated them as if they extended indefinitely into the past. The intergroup trade and warfare evident in the archival/ethnographic accounts were often assumed to be post-Western-contact phenomena stemming from the introduction of new technologies. For example, Subarctic Algonquian and Athapaskan Indians who live in less productive regions have often been treated as if they were immune from the effects of trade prior to European influences. In fact, however, there is archaeological evidence of widespread dissemination of ideas (Wright 1987:9–11). Even before direct European contact, various peoples later designated Cree traded a variety of materials with the Nipissing and Ottawa, who in turn exchanged them for horticultural products obtained from the Huron and Petun. Complex trade chains and middleman systems extended throughout most of the eastern Subarctic (Bishop 1986), and similar systems existed among the prehistoric Athapaskans of British Columbia (Bishop 1987) and other inland Athapaskans (Rubel and Rosman 1983). Thus, there is no evidence that Subarctic peoples were "possible exceptions" to the interdependent model. Indeed, certain features of their sociopolitical organization can best be explained in terms of the intensity and regularity of intergroup relationships (Bishop 1983, 1986).

I have only one criticism of this article: it does not carry the argument far enough. At one point Headland and Reid refer to Soffer's attribution of ranking in the Upper Paleolithic in part to involvement in trade. This to me is significant because it demonstrates that societal complexity does not simply depend upon food abundance (Bishop 1983, 1987) or, contrary to Testart (1982, 1988), on storage. Moreover, given that involvement in politically and sometimes economically motivated exchange was important to hunter-gatherers, then if Soffer's argument is correct it undermines the view that most Holocene hunter-gatherers were egalitarian in the ways outlined by Leacock and Lee (1982*b*:7–13). In fact, the only conclusion that can be reached is that the majority of hunter-gatherers during at least the last 12,000 years were socially stratified. The only exceptions may have been groups such as the Paleo-Indians that began to occupy new areas for the first time. Within the last few

centuries, overexploitation of environmental resources for a capitalist market system and economic and political dependency have levelled and atomized traditional hunter-gatherer social systems, generating the apparent egalitarianism of many groups. It has also, I suggest, created the illusion of isolation.

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Headland and Reid defend a thesis that is both inherently plausible and empirically well-supported. To a linguist such as myself their defense seems almost superfluous, since the linguistic evidence for their position is abundant and has been known for decades.

The distribution of click languages is a case in point. The use of clicks as phonemes is rare in the world's languages. Apart from some marginal examples in men's secret language among the Lardil of Mornington Island, Australia (Dixon 1980:66), phonemic clicks are restricted to the Khoisan languages of southern Africa (including Hatsa and Sandawe of Tanzania) and to a small number of Bantu languages belonging to the Nguni and Sotho groups (Greenberg 1966). According to Ruhlen (1987), the Niger-Kordofanian language family (to which the Bantu languages belong) contains some 1,064 languages. Since the Bantu and Khoisan languages belong to distinct language families and the only Bantu languages with clicks are those in closest geographical proximity to Khoisan, it is generally inferred that the clicks in languages such as Zulu or Xhosa are indicative of prehistoric linguistic borrowing by horticultural/pastoral Bantu-speakers from hunting-gathering Khoisan-speakers. So apparent is this conclusion in considering the distributional evidence that it commonly is treated as an inference without need of special justification (e.g., Greenberg 1966:66).

The earliest explicit statement I have been able to find regarding the origin of the clicks in Bantu languages is that of Junod (1927:30 n. 2), who notes that "the Zulu clicks are, as is generally believed, of Hottentot origin." Junod's statement may seem to offer hope for the isolationist position, since the Hottentot, after all, are not hunter-gatherers but pastoralists. This fact, however, simply raises the issue of long-standing hunter-gatherer–food-producer contact in another guise. Nama (= Hottentot) generally is regarded today as a Central Khoisan language, most closely related to such "Bushman" languages as Hai.n//m, Kwadi, and G//abake (Bleek 1929, Greenberg 1966, Ruhlen 1987). How can this linguistic fact be reconciled with the cultural differences between the Nama and other Central Khoisan-speakers unless we assume either (1) that the Nama were originally hunter-gatherers who adopted a pastoral mode of life from the Bantu or (2) that the Nama were non-Khoisan-speaking pastoralists who adopted a Khoisan language? The first of these views is generally favored, but either view implies intensive prehistoric contact between hunter-gatherers and pastoralists.

Acceptance of the foregoing thesis does not, of course, imply anything about possible contact between other Khoisan-speakers and sedentary non-Khoisan-speaking groups: one need assume only that the pre-Nama acquired pastoralism from ancestral Nguni- and Sotho-speakers and that the latter acquired clicks from the pre-Nama. Other Khoisan-speaking groups could have remained relatively or even completely aloof from Bantu influences in the premodern period.

Even this weak form of the isolationist position, however, breaks down in considering the Pygmies of equatorial Africa and the Southeast Asian Negritos. Murdock (1959:48) maintained that the economic symbiosis of hunting-and-gathering Pygmy and horticultural non-Pygmy peoples in central Africa goes back "about 2,000 years." Whatever the time depth for such contact, the Pygmies, unlike Khoisan-speakers, do not speak languages belonging to a distinct family. Rather, their languages are genetically varied, and this variation correlates geographically with the distribution of their symbiotic "patrons" (e.g., Benga, spoken by Pygmies of the Atlantic equatorial littoral, is a Northwest Bantu language located among other Northwest Bantu languages spoken by non-Pygmy horticulturalists, while Aká of the Ituri rain forest is an East Sudanic language bordered on the north and east by other East Sudanic languages spoken by food producers. There are basically two positions that can be defended in relation to these distributions: (1) that the Pygmies have always spoken languages such as they now speak but have undergone physical and cultural changes that set them apart from their food-producing linguistic relatives and (2) that the Pygmies have always been physically and culturally distinct from their sedentary neighbors and were once linguistically distinct from them but have everywhere adopted the languages of their patrons. The latter alternative is almost universally accepted. Stated differently, the isolationist view of hunter-gatherer–food-producer relations is almost universally rejected (albeit sometimes implicitly) in discussions of the languages of the African Pygmies (see Gusinde 1955, Murdock 1959, and the culture-historical implications of the linguistic classifications in Greenberg 1966 and Ruhlen 1987).

The Negritos of Southeast Asia fall into three major groups: (1) those of the Philippines, (2) those of the Malay peninsula, and (3) those of the Andaman Islands. Only the latter (viz., the geographically most isolated) speak languages that are not clearly related to those of neighboring non-Negrito peoples. All Negritos in the Philippines speak Austronesian languages, and all Malayan Negritos speak Austroasiatic languages of the Mon-Khmer group. If, like the African Pygmies, the Southeast Asian Negritos have always been physically and culturally distinct from their sedentary neighbors and were once linguistically distinct from them (as is generally assumed), when and how they adopted the language of their "patrons" become matters of some interest. Reid (1987*a*) has recently addressed this issue in relation to the Philippine Negritos. Omitting less significant details, there are four broad possibilities: (1)

there was *one* borrowing early in the contact period, (2) there was *one* borrowing late in the contact period, (3) there were *many* borrowings early in the contact period, and (4) there were *many* borrowings late in the contact period.

The languages of the Malayan aborigines all belong to the South Mon-Khmer group of Austroasiatic languages (Ruhlen 1987). Within this group they apparently form a highly discrete unit ("Aslian"). Such a distribution is most simply explained by a single language replacement soon after Proto-Mon-Khmer had differentiated into North, East, and South groups. The situation in the Malay peninsula is complicated by the fact that the hunter-gatherers of this area represent more than one distinct physical type, only one of which is Negrito. Moreover, the Malayan aborigines have not within recorded history been in contact with Austroasiatic-speaking "patrons." Rather, contact has been overwhelming with Malays, a fact reflected in the large and probably growing number of Malay loanwords in various of the Aslian languages (Benjamin 1976).

The languages of the Philippine Negritos exhibit a very different pattern of relationship: although all are Austronesian, they belong to different major subgroups within the Philippine group of Austronesian languages. This observation eliminates Alternative 2; if there had been a single borrowing late in the contact period, all Negritos in the Philippines would today speak closely related languages. If there had been one borrowing early in the contact period, no Negrito language would subgroup closely with the language of a non-Negrito group. Since this is contradicted by cases such as that of the Negrito speakers of Sinauna Tagalog (Reid 1987), Alternative 1 can also be eliminated. If Alternative 4 were true, the Negrito languages of the Philippines could exhibit considerable diversity but would *always* be closely related to the language of a neighboring non-Negrito group. This has been a persistent view of the languages of Philippine Negritos throughout much of this century, apparently first propagated by such writers as Schadenberg (1880) and Kern (1882), but one that has more recently been modified in the direction of Alternative 3. The recent work of Reid suggests that the adoption of Austronesian languages by Philippine Negritos not only has occurred repeatedly (and in some cases perhaps serially) but has varied considerably in time.

Needless to say, none of these conclusions fits well with an isolationist model of primitive hunter-gatherers. Languages are not replaced without intimate and intensive contact. Some linguists view the wholesale replacement of one's native language by a foreign language with skepticism, and we must keep in mind that the language-replacement model is predicated on the assumption that the hunter-gatherers in question are not only culturally but also racially distinct from their sedentary linguistic relatives. Where such an assumption is not made, as with the nomadic Punan and Penan of central Borneo, a different conclusion is reached: it is not language but rather culture that has been replaced (Blust 1972, Hoffman 1986).

I have so far considered only the claim that hunter-gatherers have on the whole had little contact with food producers in the premodern era, but the case of the Punan and Penan raises another issue. Headland and Reid argue that there are few ethnographically attested instances of "pure" hunter-gatherers—that at least marginal food production is practiced by nearly every human society. In some cases this situation may reflect the fact that a hunting-and-gathering population is descended from food-producing ancestors. Many anthropologists, no less than laypersons, evidently find it difficult to believe that a food-producing society could "revert" to a hunting-and-gathering economy. This attitude—which often flies in the face of considerable evidence to the contrary—almost certainly owes its strength to the depth and significance of the idea of progress in the Western world. The Moriori of the Chatham Islands lost horticulture and were at the time of European discovery living almost exclusively by hunting, fishing, and gathering (Skinner 1923). While examples such as this might be attributed to limitations imposed by a changed environment, such an explanation is often not available in the tropical world.

Hoffman (1986) documents and then dismisses as unfounded the common belief that the Punan and Penan are Neolithic survivals of a state of culture once more prevalent among Austronesian-speaking peoples in Southeast Asia. The linguistic evidence on this point supports him entirely: Proto-Austronesian-speakers ca. 4000 B.C. built houses with ridged roofs and floors raised on houseposts, cultivated rice, millet, and a variety of tubers, domesticated the dog, pig, and chicken, wove cloth on a simple back loom, probably made pottery, and may have had some knowledge of metallurgy (Blust 1976). If they are not a physically, culturally, and once linguistically distinct people who, like the Negritos of the Philippines, adopted Austronesian languages, the Punan and Penan are hunter-gatherers whose ancestors were food producers. The same holds true for other physically "Indonesian" Austronesian-speakers who have been characterized as "Neolithic survivals," for example, the Mentawei islanders (Schefold 1986). Anthropologists who choose to address ethnological questions cannot afford to ignore the evidence of language: whether they like it or not, ethnology and linguistics are as inextricably interwoven as language and culture.

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The points that Headland and Reid make concerning prehistoric trade apply equally to the Inupiat and Yup'ik Eskimo in western Alaska. The Inupiat traded blubber, thongs, and sealskins for Chukchi reindeer hides from Siberia. People from both Siberia and Alaska travelled to trade fairs on the Anadyr River in Siberia and at present-day Kotzebue. The mutual intelligibility of the Inupiaq language in Barrow with Kalaallit in Greenland suggests

a recent separation between these Eskimoan dialects. The Inuit lived in mobile populations, travelling great distances both to subsist and to trade. Their precontact culture had evolved within the previous 1,500 years and was still evolving. They participated in the commercial whaling of the 19th century and worked for wages during the Nome Gold Rush (Foote 1964). Finally, the federal government's introduction of reindeer onto the Seward Peninsula in 1894 was a temporarily successful attempt to turn the Inupiat and Yup'ik into pastoralists.

The Polar Eskimo may be the only group to fit the isolation model. When a party of English and southern Greenlanders contacted them in 1814, they believed themselves to be the only people on earth (Gilberg 1984). Their culture, however, was probably a local adaptation to the effects of the Little Ice Age (from about A.D. 1000 to 1700) and could not be considered representative of that of Pleistocene foragers. They had lost their knowledge of the kayak and bow and arrow. Interestingly, a group of Inuktutut Eskimo in Canada heard about them and set out to reintroduce them to Inuit cultural traits such as the kayak.

Thus, even though no evidence exists of their producing food, the Inupiat and Yup'ik were involved in long-distance trade and used a pastoral product, reindeer hides, to make clothing. How recent a development was all this? Probably some trade occurred in the past. Ray (1975:99), however, argues that the trade in reindeer hides did not begin until after 1788, when the Chukchi and Russians reached a truce in their century-old conflict. Only then could the Chukchi have raised enough reindeer to trade across the Bering Strait. The European goods that followed made long-distance trade significant; necessities could be found reasonably close to most groups in Alaska. The early Russian explorers found obvious trade goods wherever they went. By 1833 the native trade was extensive. When the Russian-American Company tried to establish a northern trading post, it could not compete with the trans–Bering Strait native traders.

I cannot, however, think of a scholar who assumed that these people were isolated. In addition to Foote and Ray, Burch (1975: chap. 7) has made clear the effects of contact on Inupiat social organization. If northern anthropologists have erred, it has been through emphasizing, as Spencer (1959) did, what they believed to be the remaining "traditional" characteristics, leaving the *impression* of isolation.

When Headland and Reid demonstrate that several societies believed to be isolated were not, they perform a valuable correction. When they attempt to explain the intellectual history behind these mistakes, they do an injustice. They draw upon the writings of a few anthropologists and several missionaries, journalists, and government officials to condemn the discipline as a whole. Their version of the intellectual history of the isolationist model is also faulty. They recognize but do not give credence to the two reasons anthropologists wanted to find isolated foragers or at least sift out the "traditional traits" from the modern: (1) to record these cultures in the belief that they were disappearing and (2) to find analogies useful for understanding the evolution of human society. Anthropologists were looking not for the exotic but for the pristine. The editors of *Man the Hunter* clearly stated these purposes in their introduction (Lee and DeVore 1968). These research strategies were legitimate at the time that they were formulated. Only through experience have we learned that forager cultures were not quaint but viable adaptations to existing conditions. Only hindsight makes clear the contribution of these research strategies to the repression of indigenous people and to the misunderstandings of laymen. Headland and Reid suggest different, political, and psychological motivations, but, by Ockham's Razor, we need no Other explanation.

I am left wondering about their ultimate epistemological point. They draw no conclusions about existing interpretations of foraging societies except to imply that they must be understood through their commercial relations with the larger world. Does this mean that we must analyze all of a foraging group's culture through its role in the world economy? Are we left with no data that might provide useful analogies with the past? Headland and Reid need to clarify these points in their reply.

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Headland and Reid mar a thought-provoking paper by overstating their findings and by disregarding well-known ethnographic and theoretical challenges to the isolate model.

Conclusions ignore much and are strangely worded. For instance, it is clear that the authors did not consider Alacaluf, Aleut, Anaktuvuk, Andamanese, Aranda, . . . Wakashan, Washo, Wintun, Wiradjuri, and Yahgan when concluding that "the foraging societies we know today remain in their 'primitive' state . . . because they are kept there by their more powerful neighbors and because [being "commercial foragers"] is economically their most viable option. . . ." The categorical wording tells us that this is a theory in need of testing, not a conclusion in the ordinary scientific sense.

Headland and Reid open with an inexplicably truncated history. They trace criticism of the primitive isolate model of foragers back only to Dunn's 1975 monograph and works published since 1981—to "recent writings of several anthropologists who began to challenge it at about the same time as we did." This breathless announcement of a new model ignores anti-isolationist studies published decades earlier.

Early this century Parker and the Seligmanns, who otherwise drew upon each other's expertise, clashed as to whether at least some Veddas were isolated, pristine primitives. The Seligmanns acknowledged Parker's ethnohistorical evidence of Veddas' having had over 1,500 years of external trade (Seligmann and Seligmann

1911:420–21), but they emphasized Stone Age technology (pp. 18–24, 415) and some Veddas' use of rockshelters (pp. 33, 35, 81–87, 420, etc.) in arguing that Veddas were aboriginally primitive. Parker admitted that he might be wrong in supposing "retrogression of the present Forest Vaeddas" (1909:111), yet archaeological and linguistic data led him to favor that view over the isolate theory (pp. 111–12). Surely pertinent to this debate, Veddas exhibit close economic, social structural, and other similarities to South Indian swidden cultivators, e.g., Kāṇikkār, rather than South Indian foragers (Krishna Iyer and Kunjan Pillai 1935:219–29, 231–35, 238; Krishna Iyer 1937:1–79). Indeed, when the Seligmanns mentioned Bailey's introduction of swidden cultivation to one small Vedda group in 1855 (p. 234), they ignored his statement that the other "wild" Veddas already had it (Bailey 1863:281–82). Among other case studies warranting mention are Bose's report on Birhor's having been driven by Indian neighbors into "specialized dependence on the jungle" (1956:6), even their nomadism being "guided by the needs of trade" (p. 5); two reconstructions of devolution since 2,500 B.C. of the simpler, nomadic Tupí-Guaraní, Arawak, and Panoan tropical-forest groups under pressure from competitors (Lévi-Strauss 1952, Lathrap 1968); Morris's accounts of Malapaṇḍāram, "forest traders" from ancient times, who, like other such people in India and Southeast Asia, have previously, incorrectly, been "described as if they were isolated hunter-gatherers" (1977:225; 1982); and Peterson's (1978*a*, *b*) descriptions of what may be a several-thousand-year-old "tandem system" comprised of Agta and their farming neighbors. Peterson, with the others, specifically rejects the common assumption that a culture may be studied "as if it existed in isolation" (1978*b*:348).

Kroeber, who questioned the isolation of Philippine Negritos years earlier (1919), devoted his 1945 Huxley Memorial Lecture to a sweeping survey of the "interwoven set of happenings and products" one finds in Eurasia and Africa, the ancient Oikoumenê (1946). He says of the Vedda, Bushmen, Sakai, and Aeta (1946:18), "To infer, as is frequently done implicitly, that these peoples still are basal 'primitives' through having somehow remained such automatically for the last ten thousand years, completely insulated or in a geographical vacuum, seems contrary to the probability of our total experience of history."

Other surveys bear mention. Service (1962) treats a shorter time scale than Headland and Reid, but he (1962:66), Gardner (1966), and Woodburn (1980) propose looking at cultural environments; they hold that many foragers have relatively unstructured systems because of centuries of external pressure. Curiously, Service's second edition is cited by Headland and Reid only to be dismissed in a phrase over Service's characterization of less disturbed foragers.

Several kinds of theoretical literature have been disregarded. (1) Headland and Reid cover ecological anthropology selectively. Had they looked at classics by Birket-Smith (1929), Barth (1956), or Helm (1962), they might have appreciated the long history of ecological concepts such as "oecumene" and "ecological niche," which take long-term relationships with neighboring cultures into account. (2) During the 1960s, several scholars called for reexamination of definitions of cultural types, units, and boundaries: Fried's (1966) and Barth's (1969) contributions are well known, and Fox's (1969) is especially pertinent. Fox criticizes naïve assumptions regarding the primitivity and isolation of South Asian foragers. He maintains that they became "enclaved in and exploited by a pre-industrial" system, persisting for centuries because of "patterned interaction" as "marginal, non-tribal, occupationally specialized social adjuncts of an agrarian state" (1969:139, 140, 143, 158). His very title, "professional primitives," derives from Seligmann and Seligmann (1911:40), insight about the subject being far from new. (3) Headland and Reid note reports on San's cycling in and out of food production, but they seem unaware of a wider literature on such cycles and the factors which may regulate them (Gardner 1983, 1985; Sandbukt 1986).

These are merely selected examples of the overlooked literature. They show, nonetheless, that the chorus asking for a change in anthropological perspective is older and more diverse than Headland and Reid realize. Now that they have added their voices to it, perhaps two of the things most needed from them are additional careful case studies (new or reanalyzed) and ways of testing the *various* theories before us.

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Headland and Reid follow through on an idea that has slowly been gaining ground: that contemporary and historically attested hunter-gatherers, rather than representing societies and forms of organization that evolved during the Palaeolithic or Mesolithic and have persisted unchanged, are the products of ongoing evolutionary processes that involve interaction with other populations, including farming societies and complex sociopolitical systems. While previous observations in this regard have been made primarily with reference to specific geographical or ethnic contexts (e.g., Bahuchet and Guillaume 1982, Denbow 1984, Dunn 1975, Schrire 1980, Wilmsen 1983), the statement by Headland and Reid is sweeping, universal, and, as such, polemical. In spite of some specific quibbles, I am in full support of what they say and think that it needed to be said in such a general way.

Now that this insight has been stated both in particular and in general, it seems almost self-evident, and it becomes necessary to look beyond it to its implications. I would like to suggest just a few points:

1. It is important to recognize that the nature, context, and intensity of interaction between hunter-gatherers

and other populations have varied enormously over time and from place to place. This is obvious when one compares, for instance, northern Australia, which had low-level contacts with horticultural societies in New Guinea and later with seafaring populations from Indonesia; Tasmania, effectively isolated by the end of the Pleistocene; Southeast Asian hunter-gatherers, who likely had contact with agricultural societies for several millennia and with state societies for as much as 2,000 years; southern Africa, where contact with Iron Age herders can be documented for some 1,500 years; Amazonia, which evolved a unique and complex cultural mix of its own; the Arctic, which was colonized by hunting populations relatively late in prehistory; and so on. This diversity, combined with questions of adaptation to vastly different habitats and population movement into new territories, means that one must expect a great variety of evolutionary histories, ranging perhaps from the conversion of marginal social units in farming and complex societies into "professional primitives" (Fox 1969) to transformations between different hunting-and-gathering organizations.

2. For archaeologists, the thorny issue arises of how the ethnographic record of contemporary hunter-gatherers can be used in building models for the organization of prehistoric hunting societies. It is highly unlikely that any living hunter-gatherer group can serve as a ready-made model for a prehistoric situation. However, while the ethnographic record is in itself clearly not an archive of earlier evolutionary forms, it can be used as an arena within which to investigate organizational relationships among sets of variables relevant to the formulation of models for prehistoric situations. In this way, it is possible for us to wean ourselves from the "tyranny of the ethnographic record" (Wobst 1978) without denying its importance. Demythologized, hunter-gatherer ethnography has its own limitations. In the area of subsistence organization, for instance, the roles of scavenging, hunting vs. collecting, meat vs. plant food, etc., cannot be realistically modelled unless the political economy of contemporary hunter-gatherers is taken into account. This political economy may include outside inputs into the subsistence economy through trade, wage labor, scavenging, or "theft," social storage in outside communities, economic safety nets provided by association with agricultural communities, and so forth.

3. Classical and neo- evolutionists have attempted to construct models of social organization for the earliest stages of human evolution by boiling the ethnographic record down to a set of social common denominators, extracting from it the notion of bands and hordes. Although the ethnographic data have always resisted tidy classification, the idea of band society as the first stage of social evolution for some time commanded broad consensus. Clearly, the continuous and diverse evolutionary histories of hunter-gatherer populations account, to a large extent, for our difficulties in reducing their organizations to a single social type (see Steward 1936, 1955). Ironically, Service (1962, 1966) seemed to realize the connection between social variability and population interaction but failed to see its historical depth.

4. It is not only hunter-gatherers that must be seen within a broader and more realistic historical perspective. Fried (1975) proposed some years ago that "tribes" are no more than forms of organization that tend to spring up throughout history on the peripheries of chiefdoms and states under the influence of the centripetal social, economic, and political life of these systems. Thus, rather than being evolutionary precursors of chiefdoms and states, tribes may be consequences of the interaction of the latter with societies living within their sphere of influence (see Hutterer n.d.). What Headland and Reid say here about hunter-gatherers can also be said about many other organizational abstractions arrived at through comparative analysis of a statistically perceived ethnographic record and used to build schemes of evolutionary stages and sequences. As grand an idea as the "comparative method" was when it was first proposed (see Maine 1861, Tylor 1871), it has proven untenable in the face of more adequate ethnographic, ethnohistorical, and archaeological information.

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Headland and Reid's article is an interesting way of looking at hunter-gatherer societies. Most of it is empirical in nature, and the facts are marshaled in support of an "interdependent model" for the majority of hunter-gatherer societies. In my opinion, this part of the work is reliable. The authors are still, however, far from explaining any concrete situation, and some of their assumptions may have serious consequences.

We must bear in mind that the general characteristics of a given population result from the biological and cultural (material, energetic, and informational) requisites for its creation and reproduction and the possibilities presented by its exploitative abilities and the environmental conditions. In this context the authors are correct in pointing out that the character of a hunter-gatherer society's culture is a result of long-term adaptational strategies. It seems that several types of adaptation can be distinguished among these societies, each of which must have been constantly evolving, in some cases leading to specialization. The article wisely stresses the food supply, an aspect of the functioning of human populations that is often overlooked in examining prehistoric groups.

The thesis that contacts among the groups under examination were mutually profitable (cf. Pianko's "mutualism") is too general. To get at the core of the phenomenon, it is necessary to answer such questions as whether the interaction was constant, whether it was exclusive (there being no contacts with other groups),

whether the environment's resources were altered by other than human activities, etc. The issue here that interests me most is why some groups that formed mutually profitable contacts with agricultural societies accepted the new mode of subsistence while others did not. If the Agta knew how to cultivate the soil, why did they not change their mode of subsistence—given that the adoption of production is the most important factor of change and that presence or absence of cultivation serves as the main criterion for classification of prehistoric societies?

This article is certainly another piece of evidence in support of the need for revision of the concept of hunter-gatherer society. I agree, however, with the authors' conclusion (citing Vierich 1982) that "if the hunting and gathering way of life has survived in the Kalahari, it is not because of isolation."

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Headland and Reid have admirably described anthropology's fascination with alleged isolated hunting-and-gathering societies. I agree with them that the hunt for the "Savage Other" serves Westerners' psychological need to believe in a primitive purity and their moral need to justify wholesale appropriation of hunter-gatherers' land and other natural resources. Anthropology may have been caught in this trap of its own construction.

A Yupik living in Nunapitchuk in the lower Kuskokwim River delta sets up his fish camp at a bend in the river that his kin group and extended family have used as long as can be remembered. While patching his set nets and rebuilding his drying racks, he ponders U.S. government efforts to limit the high-seas catch of the salmon upon which he relies. A Tlingit chief from Hoonah takes his grandson to their clan territory in old-growth spruce and hemlock forest on Chichagof Island for the first deer hunt of the season. Perhaps this year the boy will learn how to call the deer. By the time this boy is a man, however, the deer may be gone. The chief believes that destruction of the forest will restrict subsistence hunting and fishing. Any day now a federal court may rule on the suit he has brought against the U.S. Forest Service to stop the clear-cutting. An Inuit whaling captain from the small community of Kivalina pores over the latest satellite photos showing the location of leads in the pack ice that will channel the bowhead whale migration. He and his crew are camped on the ice at a lead, alert. At the sight of bowhead whale they can quickly and silently slide their walrus-hide-covered skin boat into the water, paddles in hand, harpoon ready. A few days earlier the captain attended meetings of the International Whaling Commission in New Zealand, where the Eskimo representatives successfully argued for a revised quota for the coming year's hunts.

We could, of course, argue that the cultural complexity that these three vignettes illustrate is a strictly modern phenomenon and search for data from a less complex era on groups not "spoiled" by contact with the larger world society. Leaving aside the epistemological paradox presented by the idea of contacting an uncontacted group, finding cultural groups that have not experienced major interaction with colonial powers is problematic even with Arctic societies. Should we try to reconstruct Arctic hunting-and-gathering societies before the impact of the late 19th-century gold rushes in Alaska, before Russian colonization and the fur trade, before the exploration of Hudson Bay, or before the reestablishment of European colonies in Greenland around 1500? Even if we reach into this past, we find significant trade and contact within and between ethnic groups, with reindeer skins and other furs being traded across the Bering Strait, whale skin and other marine mammal products inland across ethnic boundaries, and canoes and slaves up and down the coast of southeastern Alaska. A romantic, isolated ethnographic present for hunters and gatherers, if it ever existed, is simply not accessible to us.

With Headland and Reid, I think we have been on the wrong track in trying to find or create the isolated hunting-and-gathering society and should look to extant hunting-and-gathering groups for data relevant to the general phenomena of hunting and gathering. The rich corpus of academic and grey literature concerning contemporary hunters and gatherers in the North American Arctic and Subarctic may provide better theoretical foraging than speculation less well grounded in field data. Burch's (1975, 1985) work among modern hunters in Kivalina, documenting subsistence hunting and fishing and patterns of land use, Nelson's (1969, 1973) studies of Athapaskan and Inuit subsistence strategies, Wolfe's (1981) description of the subsistence economies of lower Yukon River societies ca. 1978, and my own work documenting contemporary subsistence harvesting areas for Kotzebue Sound Inuit (Schroeder, Andersen, and Hildreth 1987) and for Hoonah Tlingit (Schroeder and Kookesh 1988) are but a few of the dozens of Arctic and Subarctic studies (see Damas 1984) that record a continuity of hunting-and-gathering cultural traditions. These studies are not about remnant populations or the continuation of Neolithic adaptations but about how hunting, fishing, and gathering take place with the technological tool kit currently available and how indigenous Eskimo and Indian peoples maintain access to the natural resources and the land-use rights that are the basis of their cultures.

We may do well to remember that cultural anthropologists have *always* studied their contemporaries and to look to Yupik, Tlingit, Inuit, and other living societies to advance our understanding of hunting and gathering. Perhaps we, as neighbors of modern hunter-gatherers, may learn not only something of the structure of the hunting past of humankind but greater responsibility as temporary caretakers of diminishing natural resources.

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Studies of contemporary hunter-gatherer societies have indeed often neglected the historical depth of attempts at agriculture. As Headland and Reid point out, even studies of symbiosis between foragers and farmers do not consider the long-term character of this contact. Most anthropologists have not denied these early interactions but more or less intentionally neglected them, not only for psychological or political reasons but also because of their irrelevance to or interference with a cultural approach to the problem.

Studies of hunter-gatherers from the historical viewpoint have been predominantly concerned with the question of the antiquity of these cultures, and they have been presented as not influenced by their peasant neighbours even when the fieldwork has been biased by the perspective of those neighbours. Structural-functional studies have tended to regard such societies as closed systems, considering their interdependence with farmers but paying no attention to the duration of the relationship. For ecological studies the historical perspective is not of interest either, and farmers have often been excluded because only foraging strategies focus upon the relevant biotope. Nor have studies of culture change recognized the long-term relationship between foragers and farmers, focusing instead on their present situation. Most anthropologists have not assumed the total isolation of hunter-gatherers, a tendency frequently apparent in the popular literature; they have simply neglected the question of the duration of contact. Indeed, most scholars dealing with the topic have not denied the possibility of early trade, some of them even believing in a form of "silent trade" that could, however, never be demonstrated.

Referring to the examples given by Headland and Reid, which are very convincing in their linguistic, ethnohistorical, and ethnoarchaeological argumentation, I would like to add some further points from my observation of the ethnic groups mentioned:

The Negritos of Zambales, where shifting cultivation can be traced back to pre-Spanish times, bear out Headland and Reid's theory: Here aspects of the pre-Spanish peasant Zambal culture lost in the process of change to the Christian Filipino culture, such as certain rice ceremonies, marriage customs, magical practices, and healing procedures, are preserved in the contemporary Negrito culture (Seitz 1984:296, 297). The same is true in the case of the Penan, and the question arises whether all non-Negrito groups in insular Southeast Asia might not originate from an agricultural base and constitute a phenomenon of devolution (Seitz 1981).

Contacts can, however, assume quite different forms. In insular Southeast Asia, long-distance trade may indeed have played an important role for the interactions between foragers as collectors of forest products and farmers as intermediaries in this trade (Hoffmann 1984, 1986). In Central Africa the interdependence between Pygmies and Bantus may date to the earliest times, when farmers invaded the forest and the Pygmies, as is suggested by some Bantu oral traditions, probably served as their guides. They still share membership of their clans. The Pygmies are also a convincing example of the integration of hunter-gatherers into state formation, namely, the divine kingship of Central Africa, in which they constitute the lowest social level. Finally, the first Europeans to encounter Pygmies noted small-scale agricultural activities among most of them, though these reports were overlooked by later scholars constrained by the hypothesis of isolation (Seitz 1977).

Reply

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Dallas, Tex./Honolulu, Hawaii, U.S.A. 28 IX 88

We thank the commentators for their constructive criticisms and suggestions. While some of them raise important questions or state reservations about points in our argument, all reject the isolate model of prehistoric Holocene hunter-gatherers and concur with our interdependent model.

Several commentators strengthen our case by referring to recent anthropological writings cast in the isolationist mold (e.g., Bishop's discussion of Diamond's 1988 isolationist view of New Guinea). Several also add support to our argument by citing cases that we did not discuss of prehistoric interaction of supposedly isolated peoples. Flanders and Schroeder show that the model applies to prehistoric Arctic Eskimo, and Bishop and Schroeder point out that Subarctic Indian groups cannot be considered "possible exceptions." Blust reviews several bodies of linguistic evidence which support our model for Africa (see our n. 8). Hutterer contributes to our thesis by pointing out some implications for hunter-gatherer model building. We must recognize, he says, that forager/farmer interaction has varied enormously over time and space, that hunter-gatherers today can hardly serve as models for a single social type in prehistory, and that "tribes" as well as "bands" may be the consequences of influence from states rather than their evolutionary precursors. Seitz makes an important contribution to our discussion of Philippine Negritos by pointing to evidence of prehistoric contact between Zambales Negritos and non-Negrito farmers.

Marciniak asks two important questions: (1) Why did some groups form a new mode of subsistence with food producers while others did not? The reasons seem to be food and ideology. Hunter-gatherers in food-scarce areas may develop contacts with food producers sooner than those living in areas where wild food is abundant. Some groups (e.g., the Tunebo of Colombia) reject contact with

outsiders for religious reasons while others (e.g., groups in highland New Guinea) actively seek it for similar reasons. (2) Why, if the Agta knew how to cultivate, did they not adopt that mode of subsistence? One of us has devoted a separate paper to this question (Headland 1988*a*). Though there are several reasons, including ideology,the major one is that the dominant non-Negrito peoples of the area have prevented them, usually by depriving them of land.

Bishop agrees with our thesis but suggests that we do not carry it far enough. He brings out an important point which is only implicit in our paper: that if our model is correct, then most Holocene hunter-gatherers may have been socially stratified rather than egalitarian. The implications of this are significant, since anthropologists have used such hunter-gatherers to model the way Pleistocene humans must have lived.

In his discussion of our linguistic argument, Blust points out that there are two possible views as to what happened in prehistory to have caused the language picture we have today: either (1) the African pygmies (and the Asian Negritos) always spoke the languages they now speak and were phenotypically identical to their present-day farming neighbors until they underwent microevolutionary genetic change after they moved into the forest to take up foraging or (2) they were always physically distinct from their neighbors and were once linguistically distinct from them but at some time in the past adopted the languages of their patrons. We of course hold to the latter view, as does Blust. Rambo (1988) is the only anthropologist we know of who has seriously proposed the former.

Blust outlines the four alternatives for the history of the Negrito languages and agrees with us that the one that best accounts for the linguistic evidence is that the various Negrito groups independently, and sometimes repeatedly, adopted the languages of their "patrons." These events have continued over thousands of years, ever since Negritos first came into contact with immigrating non-Negrito agriculturalists (in the Philippines, sometime after 3000 B.C. [Blust 1984–85:56]).

Blust may be overstating the case when he says that the linguistic evidence for our position "is abundant and has been known for decades." This may be true for the language situations of some hunter-gatherers in Africa, but it certainly is not for Philippine Negritos. Although it has long been known that they speak Austronesian languages, prior to recent work by Reid (1987) and Headland (1986:174–78) no one had addressed the question of when the switch to speaking Austronesian languages took place or what the implications of this might have been for the prehistory of these peoples. Even for Africa, too many anthropologists have written about hunter-gatherers without taking into serious account the references that Blust cites. We hope that our paper will help remind anthropologists of the value of looking more closely at the linguistic data as a tool, along with archaeology, for reconstructing the past.

Blust misinterprets us as defining "pure" hunter-gatherers as people who practice no food production. We should have made it clearer that a "pure" hunter-gatherer group is one without any dependence on produced foods. There may be hunter-gatherers today that practice no cultivation or animal husbandry of any kind, but they are not "pure" because they depend at least seasonally on produced foods that they secure through trade.

Three of the commentators raise more serious criticisms. Gardner makes the strongest negative comments. We concede that we may have overstated our findings when we implied in our conclusion that all foragers live by symbiotic commercial foraging. We should have said that "most foraging societies we know today" live that way. We appreciate Gardner's pointing out certain groups that may be exceptions, such as the Andamanese and some 19th-century Australian groups (but note Hutterer's comment on Australia and our discussion of the Australian findings of Cowlishaw [1987] and Campbell [1965]). In any case, we agree with Gardner that our model is a theory in need of testing, not a conclusion. We should not have implied otherwise.

Gardner also faults us for not covering everything in ecological anthropology, such as the concept of the "oecumene," niche theory, and the works of Fox (1969) and Barth (1956). Space precluded that, but one of us has discussed the implications of all of these elsewhere (Headland 1978, 1986, 1988*a*, *b*). He concludes by stating that one of the things now most needed from us is careful case studies. We feel we have provided such a study in our work on the Agta and in the linguistic studies on the Arta and Alta Negritos. Marciniak, too, says that we are "far from explaining any concrete situation." We suggest that the Agta is such a concrete case.

We do not think that our paper reads as a "breathless announcement" of a new model which ignores earlier anti-isolationist studies. We specifically stated that we were not the first to question the myth of the primitive isolate. Likewise, we do not, as Bicchieri suggests, "claim to be the rightful heirs to evolutionary-adaptive theory." We are thankful for Gardner's addition of references to earlier studies (especially from India) which we had missed in our search, but our explicit references to the numerous writings of other anti-isolationists should have made it obvious that we did not mean to ignore anyone or try to make it appear that we were the first to reject the view of hunter-gatherer isolation. Gardner's added references strengthen our argument, because they substantiate what we wish to point out, that scholars and lay people alike have disregarded what even anthropologists such as Kroeber (1946:18) were saying years ago. Incidentally, though Peterson (1978*a*, *b*) did well in pointing out the importance of the symbiosis of Agta/farmer interaction, her study was ahistorical. Gardner is mistaken in implying that she referred to "what may be a several-thousand-year-old 'tandem system.' " We refer Gardner to a careful critique of her study (Headland 1978) which points out both her contribution and her misinterpretations of the Agta culture.

Bicchieri claims that we should have put more emphasis on "demonstrating the presence, historically and pre-

historically, of more interdependence of food collectors and food producers than had been thought." This is in fact the emphasis throughout our paper. More detailed "demonstrations" of interdependence from the Philippines may be found elsewhere (Headland 1978, 1986, 1987; Reid 1987, 1988*a*, *b*; Headland and Reid n.d., 1989).

Flanders says that we draw no conclusions about existing interpretations of foraging societies except to imply that they must be understood through their relations with the larger world. That, indeed, was our main point, and we appreciate his helping us to make it explicit. We are not saying that the present data do not provide useful analogies with the past, but, as Hutterer brings out, the analogies are not nearly as close as we all once assumed.

Bicchieri and Flanders criticize us for putting too much of the blame for the popularization of the isolate model on anthropologists. They rightly point out that the perpetuation of that view comes more from the work of missionaries, journalists, government officials, and the media than from that of anthropologists. True enough. But we think that we have cited enough anthropological works in our paper to substantiate the role of anthropologists in keeping alive the outdated isolate view. Flanders disagrees, too, with our suggestion that anthropologists perpetuate the isolate model because of their emphasis on exoticism, saying that they "were looking not for the exotic but for the pristine." Call it what you will, the search for the pristine has led too many anthropologists to describe various small-scale societies as if they were more isolated than they really were. And other anthropologists, as Seitz points out, without assuming the total isolation of hunter-gatherers, "have simply neglected the question of the duration of contact."

In fact, some have not only neglected but denied it. According to one anthropologist, "It is the fondest dream of every ethnologist to discover and establish friendly contact with one of the few genuinely isolated and unacculturated societies that still survive in the modern world" (Lukesch 1976:9). In his ethnography of the Asurini in the Xingú Park area of Brazil he says, "We were the very first investigators to enter the pristine world of this particular population" (p. 10). The dust cover of his book states, "This book provides significant documentation of a first encounter with an aboriginal Indian tribe in the 20th century. It was in this case an anthropologist with many years of experience among Indians who . . . entered . . . as the very first the untouched world of a forest people." How isolated were the Asurini? When Lukesch visited them they already had a few iron tools, some cotton clothing, glass bottles, latex bags, mosquito nets, commercial buttons and beads, and "objects of aluminum and plastic" (pp. 113–15, 120). They also had gardens which they cleared with steel axes (p. 113). This is exactly the kind of ethnographic writing that we are reacting against.

Our argument appears to be part of an emerging movement of reaction against isolate descriptions of "pristine" hunter-gatherer societies. The increasing momentum of this reaction is reflected in a recent report in *Science* (Lewin 1988), in an unpublished paper by Howell (1988), in a forthcoming book edited by Spielmann (n.d.), and in several chapters of a two-volume work edited by Ingold, Riches, and Woodburn (1988). We were unaware of these until after our paper was in press, and none of those authors have heard of our work; yet they are making the same epistemological point as we are. Lewin's key statement is that "modern hunter-gathering is a largely post-Pleistocene phenomenon. Rather than being an adaptation ancestral to food production, it is a parallel development . . . [with] agricultural systems" (p. 1147). The three chapters that we have seen from the Ingold et al. work reflect this same revised view of hunter-gatherers. Foley (1988) suggests that *Homo sapiens sapiens* in the Upper Paleolithic were morphologically so different from today's hunter-gatherers that their subsistence and mating strategies must have been very dissimilar. The chapters by Bender and Morris (1988) and by Bird-David (1988) also reflect our thesis in their main points. Howell's 1988 paper is especially revealing, because it is a confession of an anthropologist who helped popularize the isolationist myth in the '70s (Howell 1979). She now apologetically admits that she and her Harvard colleagues badly skewed the now-popular view of the !Kung by failing to recognize their long "history of experiences with agriculture, herding, trading, warring, and banditry" (Howell 1988:18).

Whether or not we want to call this shift of theory an emerging paradigm, it is certainly a fast-developing idea moving farther and farther away from the orthodox view of Holocene hunter-gatherers as isolated, timeless, pure foragers. As Bird-David (1988:18) says, "There has been a snowballing increase in acknowledging and examining the significance of contact with other people." In spite of Gardner's criticism, we see this interdependence theory as "new." This is not because no one has stated it before (as both we and Gardner have pointed out) but because the few who did state it in the past were not heeded. Today, however, we see a theory finally coming of age.

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