



The Hunter-Gatherer 'Revisionist' Debate

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Do you think that certain problems in the Amazonian area ethnography – such as kinship or the relationship to the environment – have been solved? Or are there still burning questions which should concern the anthropologist?

Yes, there are still big, unresolved questions. One is the problem of hierarchy. Probably because of the writings of Pierre Clastres, one has tended to see the Amazonian Indians as I presented them earlier – as simple anomic societies. But some of these societies have, if not a strong hierarchical political system, at least a strong conception of hierarchy. And we know from the archaeology and ethnohistory that there were many hierarchical societies in this region of the world before they were destroyed by the Spanish and Portuguese conquests. So an important aspect of Amazonian study is the relation between apparently simple systems and the symbolically complex hierarchical organization of the world which in some cases must also be a hierarchical organization of society. We never really talk about that. It's important to understand how the apparent reversion from complexity to simplicity can occur. Most, if not all, hunter-gatherers in South America were formerly horticulturalists and this type of regression has been very little studied in anthropology in general.

Another thing which is very striking in Amazonia is the whole topic of religion and the relation to the supernatural world. There is no religion in a traditional sense in that area, i.e. no transcendence. The grammar of sensibility is important, in the sense of hearing, speaking, singing, seeing, and it is conveyed through a very complex conception of communication which has been very little studied. This takes its most conspicuous form in the widespread use of hallucinogenics. This implies more in-depth study of semantics, linguistic anthropology and native theories of knowledge.

You mentioned earlier your current writing. How do you situate your present work in anthropology?

I came to Cambridge to do two things, but unfortunately I've been able to do only one. The one I'm doing corresponds to ethnography. Many anthropologists have written personal accounts of their fieldwork. Perhaps it will be interpreted as a 'post-modern' book given the

success of French philosophical thought in the U.S., but it is not so in intent.

Who are you writing for this time? What readership?

For the general public. Mainly for non-anthropologists, for the 'public cultivé' – a fictional entity which apparently exists because it still buys books. I still, however, want it to be readable by anthropologists as a monograph. It's very difficult to write for a general public for you have to stop yourself using the usual jargon, and you must express very complex things in an understandable way. Yet I find it easier to get into nuance when writing like that than when writing anthropological jargon.

In writing this book are you saying that anthropologists have a duty to write or 'converse' beyond the discipline?

Absolutely. It is why in the first place I accepted the challenge of writing this book – I hate to talk about myself and this interview is, I fear, a bad example. But at the same time there is a huge amount of very bad travel literature especially on Amazonia, books in which you get to know much more about the writer than the people who live there. I read one by a young woman a while ago to explore this – I knew of the people amongst whom she travelled – and I tried to see what anthropological information I could gather from the book. None! Not a sentence in 250 pages! So I see it as part of our responsibility as anthropologists to give the people who pay us – the public – an idea of what these societies are about, how they work, love, hate, enjoy friendship, express sorrow and so on. Few anthropologists have dared to do this. I can do it now because I am an established anthropologist. I would never have done it as a young anthropologist before doing a Ph.D for example, because I know it would have cost me my career. I think it's still like that. But it's important to do so. I've been struck recently by the strong re-emergence in France of very racist stereotypes regarding 'primitive societies', for example the use of Amazonian Indians in advertising, which are quite obnoxious, and I feel we have to react. □

The hunter-gatherer 'revisionist' debate

DANIEL STILES

The author has conducted research with hunter-gatherers in East Africa since 1978 and is currently researching Malagasy and Asian hunter-gatherers in the context of ancient Indian Ocean trade.

Are there any untouched, pristine hunter-gatherers left in the world? Have there been any anywhere recently, i.e. over the last few centuries? As a result of recent criticisms of the Harvard Kalahari Project's approach to their twenty-plus year study of the !Kung (Wilmsen 1983; 1989; Denbow 1984; Wilmsen and Denbow 1990; and Howell 1988) and of a summing up of other hunter-gatherer studies over the past two decades (Headland and Reid 1989), popular, if not anthropological, views are changing (Lewin 1988; Bowen 1989; and Headland 1990a). What exactly is the 'revisionist' argument and what implications are there for current and

future hunter-gatherer issues and research?

To take two of the more polemical recent examples (Headland and Reid 1989; Wilmsen and Denbow 1990), it seems to be a revelation that most, if not all, existing hunter-gatherer groups are not pristine and that they have had historical relations with non-foraging groups, sometimes for centuries. The *Man the Hunter* (Lee and Devore 1968: 11-12) generalizations about hunting-gathering societies and assumptions that some foraging groups today are isolated remnants of Pleistocene groups (Murdock 1968) come in for serious 'myth-bashing'.

The Punan of Maimantan hunt with blow-guns and collect a wide variety of forest products to eat and trade. Photo by Daniel Stiles.



What many of the critics tend to forget, or at least downplay, is that in the 1960s anthropologists were specifically looking for existing isolated foraging groups '... because of the light they may shed on the behaviour of Pleistocene man' (Murdock 1968: 335). Many exciting hominid fossil finds were beginning to come out of Africa, and Pleistocene archaeology of early hominid sites was taking off. People wanted to be able to infuse some life into the dry bones and stones of the past, and it is not surprising that ethnoarchaeology as a sub-field took off in the late 1960s and 1970s as well (see Stiles 1977 for a summary). Paleoanthropologists wanted ethnographic analogies, and people like the !Kung provided them. It is no coincidence that the late Glynn Isaac's (1972; 1976) influential model of early hominid behaviour had many features in common with the 'trial formulation' of hunter-gatherers presented by Lee and Devore (1968: 11-12). Isaac's model, too, has come in for its share of criticism (e.g. Binford 1985).

None of the views expressed by the 'revisionists' are really new, however. They are just synthesizing and stressing facts that people working with foragers have noted for a long time about foraging contacts with non-foraging neighbours (e.g. Parker 1909; Kroeber 1946; Harrison 1959). These foragers were also discussed at the 'Man the Hunter' conference, referred to as 'devolved' or 'dependent' peoples, therefore unfit to be used as models of the past. In the 1970s-1980s many researchers concluded that trading between foragers and their food-producing neighbours was essential to their subsistence economy and might even explain their existence (Peterson 1978; Schrire 1980, 1984; Stiles 1981; Wilmsen 1983; Hitchcock 1982; Hoffman 1984; Headland 1987; and others). It was not a case of 'devolution' or a failure to succeed in food production, but rather people took up foraging as a rational response to ecological, economic and socio-political realities. Thus, the earlier polarity of 'pristine' versus 'devolved' hunter-gatherer has given way to a more complex appreciation of the historical, ecological and socio-economic dynamics involved in explaining and analysing their existence today.

The view that various types of exchange relationships

are crucial to the existence and persistence of hunter-gatherers is emerging as the dominant paradigm, replacing the 'isolated remnant' view of hunter-gatherers (Stiles 1981: 858-859):

The hunting peoples ... have persisted until the present because they occupied a needed place in the human ecology and economy of the coastal region. Products of the hunt have always been highly valued by the non-hunting communities ... for profit, food and crafts, and the hunting peoples traditionally fulfilled certain social and economic functions in the [neighbouring] societies ... [T]he most rational use of the forest, while preserving its ecology, is hunting-gathering.

The existence of hunter-gatherers who historically had no non-foraging neighbours, such as the Andaman islanders and Australian Aborigines, must, however, be accommodated, and Hoffman (1984) has come up with the useful categories of *primary* and *secondary* hunter-gatherers. This idea is not new, either, and Huntingford (1931) pointed out that there seemed to be 'free' hunter-gatherers, along with various types of dependent groups, in East Africa. He later (Huntingford 1963) refined this concept and defined 'Bushman' and 'Symbiotic Hunters', analogous to Hoffman's 'primary' and 'secondary' categories. The first type were independent, subsistence hunter-gatherers, and the second type were involved in exchange and social relations with non-foraging neighbours. Huntingford (1963) assumed the Symbiotic Hunters were dominated by their neighbours, a harbinger of Woodburn's (1982) 'encapsulated' forager type.

The real paradigmatic shift that is occurring in hunter-gatherer studies is *not* that there are no 'real', 'pristine' hunter-gatherers. No one, including Lee, Devore and Murdock, ever said that there were. The shift is that more anthropologists are now seeing value in studying the interdependent foragers, and new theoretical and practical issues are being raised concerning them. We are not so much interested any more in using living foragers as models for the past (though see below). Freeman (1968) strongly advised against doing so at the 'Man the Hunter' conference, but he was in a distinct minority. Foley (1988) has more recently taken up the argument that we cannot use present societies in explaining the Pleistocene by analogy or modelling.



All these medicines and cosmetics are collected and traded from the forest by the Mikea foragers in Madagascar. Photo by Daniel Stiles.

The two basic paradigms relating to hunter-gatherers, i.e. (1) that there are those who have been isolated, they subsist from their own food production, and therefore they can be used as models of the past and (2) that there are those who are interdependent with neighbours, have resulted in two quite different approaches to research objectives and methodologies. The first one, best known from the Harvard Kalahari Project, has been called the 'evolutionary ecological' approach (Lewin 1988). It views hunter-gatherers in a very similar way to how ethology looks at chimpanzees or baboons. Since modelling human behaviour in the past is one of the avowed aims, certain features of study take precedence over others. These commonly include a detailed description of ecological and natural resource variables, followed by studies called 'optimal foraging strategies', 'input-output' or 'cost-benefit' analyses, 'time allocation' studies, etc. Trade and relations with neighbours are not seriously investigated, and the hunter-gatherer subsistence system is viewed ahistorically and as a closed entity.

The second approach, which can be called 'historical particularism' (Lewin 1988), sees historical influences on present hunting-gathering groups as being much more profound than the 'ecological evolutionists' would admit. Each foraging group has a history as long as anyone else's, and they have had varying degrees of contacts and interrelationships with neighbours for centuries or millennia. They have also probably had varying experiences with food production and trading in the past, depending on the group's particular history. 'Devolution' is not a term liked by this approach, as it views foraging on an equal basis with agriculture and commerce and, usually, as being integrated with larger political systems that comprise all three.

There has been a need for specialist, 'commercial' hunter-gatherers in various parts of the world for at least 2000 years to supply the wild forest commodities that helped feed East-West trade in ancient times (Stiles

1981; 1991a; Hoffman 1984). The importance of hunter-gatherers in this trade is still undervalued. Many still see trade as being small-scale and related to subsistence (Headland and Reid 1989; Baily *et al.* 1989). Subsistence might be a viable explanation for foragers trading with food-producers in tropical rainforest ecosystems, owing to a local scarcity of starches (which implies people did not live full-time in this ecosystem prior to the Holocene), but the archaeological record shows that people lived in a wide variety of ecosystems prior to food production. Food for survival does not seem to be the principal explanation for trade in most cases.

One implication of this increasingly dichotomized view of hunter-gatherers and research paradigms relates to funding for research. A grant proposal to conduct research on history, subsistence and trade with an admittedly interdependent (though as yet unstudied) foraging group was recently turned down. The main negative reviewer comment was, 'Studies of subsistence economies ought to include basic stuff on time spent and return rates earned and amounts of important resources taken... We want to encourage quantitative data collection, i.e. the answer to questions of who does what, when, for how long and with what result comes from watching, timing, counting, weighing'. The foundation in question has made it policy to support only quantitative, subsistence and settlement-pattern oriented research with foragers.

The population number and distribution, territory and ecosystem variations are not even known for this group. Yet the particular granting body reviewing the above proposal wanted a lot of time and effort devoted to counting and weighing tubers. Why? It is because this organization is firmly on the evolutionary ecological side of hunter-gatherer studies. It is not surprising that researchers funded by this organization report that the groups they study subsist totally from inside inputs and that trade is not necessary for their survival (Brosius 1990: 7; Hill and Kaplan 1990: 1). Closed systems are studied with the hope of applying models to the past – and, needless to say, these groups were until recently 'isolated'.

On the other hand, there are funding bodies, such as the U.S. National Endowment for the Humanities, which give preference to historically oriented studies. Most funding organizations, fortunately, will support a project on its merits irrespective of the paradigm employed (such as the National Science Foundation, National Geographic Society and Wenner-Gren Foundation).

To a certain extent, I agree with Hoffman's (1984: 147) conclusions:

A serious re-examination of the ethnographic record in its historical perspective may well indicate that commercial hunting and gathering not only accounts for most hunter-gatherer groups of the ethnographic present, but also explains why the hunting and gathering adaptation has persisted to the present day. If this is proved to be the case, then the seemingly endless array of new studies on hunters and gatherers that focus on such matters as optimal foraging strategies, cost-benefit ratios, patch use models, and other aspects of subsistence really tell us very little about these people.

Headland (1990b; 1991), however, has shown how quantitative studies can provide valuable information about the current nutritional, health and demographic situation of foragers, and on the success or otherwise of the peoples' adaptation to their environment in the context of modern realities. This application of quantitative studies has relevance to important questions concerning land rights and forest conservation, in addition to

theoretical anthropological issues. This shows how the two paradigms can be combined.

Another implication of the revisionist debate can be seen in how we view controversial cases of hunter-gatherers, such as the Tasaday and Tau't Batu of the Philippines. I won't review the Tasaday case, it has received attention recently in A.T. (February, April, June, October 1990) and Berreman (1991) has recently presented a detailed history and discussion of the case. He calls the Tasaday of Mindanao a complete hoax which was engineered due to the megalomania of the head of PANAMIN. The Tau't Batu of Palawan were later (1977) also proclaimed to be a Stone Age tribe recently 'discovered' in the jungle. In both cases, President Marcos created reserves to 'protect' these people from the outside world.

There is abundant evidence that the Tasaday and Tau't Batu had been in contact with neighbours before their 'discovery' (Berreman 1991; Peralta 1979 and pers. comm.). Viewing them in the light of what we know of symbiotic/secondary/interdependent foraging peoples in South East Asia, what would we expect to find with a foraging group living in the South Cotabato area? They would be a small, mobile group, living in a primary forest area. They would have a well developed trade and social relationship with at least one agricultural community. They would probably share linguistic and certainly cultural features with this neighbouring community. They would be trading forest products in exchange for food, tobacco, metal, salt, cloth, etc., and perhaps even money could be involved. They would obviously have objects from the outside and be visiting agricultural areas from time to time on their trading forays.

It astounds me that in all of the verbiage expended on the Tasaday no one has tried to find out if they aren't just normal, twentieth century foragers. I wanted to visit the Tasaday in March of 1991, but unfortunately kidnappings and military operations in the area prevented me from doing so. I interviewed Dr. Jesus Peralta of the Philippines National Museum, someone who has visited the Tasaday, and he openly admitted that in 1971, the time of their 'discovery', they had implements, food and clothing from the outside. He maintained that they were a distinct group who depended on hunting-gathering, however, and that their isolation and simplicity had been exaggerated. I think it most likely that the Tasaday were your run-of-the-mill foraging group and that they were made to appear pristine, peaceful and in need of protection.

I think it quite possible the real reason behind the Tasaday and Tau't Batu deceptions – which was to pass off interdependent foragers as isolated subsistence ones – was to make it possible to declare reserves off-limits to everyone. These two groups happen to live in two of the last remaining primary forest areas in the Philippines, and primary forests have lots of large, valuable hardwoods. Governments can keep everyone except approved people out of forest reserves. Approved people can and do include timber tycoons in the Philippines (Stiles 1991b).

This leads me to my last implication. If anthropologists can demonstrate that present-day hunter-gatherers have been for centuries and are today integral parts of larger economic systems, then one is led to conclude that it is not necessarily inevitable or desirable that they be 'modernized' or 'developed' to join modern society (which means Western society by most governments and aid agencies). Commercial foragers are performing economically useful work and

they are respecting their environment while doing it. I was interested to observe recently that the *caboclos* (mixed-race forest dwellers) of Amazonia are basically interdependent foragers, collecting a wide variety of forest products for sale and exchange in the towns. They would be an example of new commercial foragers, within the last 200 years at most, demonstrating that there is still a need for this economic function. This understanding of foragers can be used to argue for action that will guarantee these people rights to their traditional territories and their way of life. Even influential news magazines are beginning to see value in these people (Linden 1991). My experience leads me to believe that most of them would like to return to or continue with foraging, but with a few of the modcons thrown in.

The popular view that foragers are remnant isolates from the past, still current outside of informed anthropological circles, that their way of life is anachronistic, and that they must 'progress' like the rest of us, is condemning them to cultural extinction. John Yellen (Lewin 1988: 1148), formerly of the Harvard Kalahari Project, said that 'The confrontation between the "historical" and the "traditional" or "evolutionary" approaches ... dissolves, for the most part, into the unanswerable question of which alternate ... goal is most worthy of anthropological pursuit'. I believe the question is answerable. □

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obituary

ANTHONY FORGE

Anthony Forge, foundation Professor of Anthropology at the Australian National University (Faculties), died on 7 October last year. His death is a grievous loss to the anthropological community, not only in Australia but also in Britain, the United States, Indonesia, Papua New Guinea and elsewhere. He had an exceptionally large circle of friends, colleagues and ex-pupils who will remember him with undiminished fondness, as the months and years go by, as a thoroughly admirable man and a fascinating, individual, personality.

Anthony Forge was born in 1929 and educated at Cambridge and the LSE, where he taught from 1961 until 1974, when he was called to his chair in Canberra. His contributions to anthropology were in two main fields: the study of New Guinea societies (particularly the Abelam, where he conducted prolonged fieldwork) and the study of indigenous art, his special interest. His influence on the development of New Guinea studies was exercised through certain important, if infrequent, publications, notably his essay on 'The Golden Fleece' which set the stage for the astonishing efflorescence in studies of Melanesian exchange systems which occurred during the

ensuing decade. But equally important was his role as a teacher of graduate students at the LSE and subsequently at the ANU, not to mention his participation at ANU seminars where a stream of passing Melanesianists were able to submit themselves to his always genial and helpful commentary. He had the art of being critical without being offensive, patronizing or unctuous, and he was an enormous help to many young and nervous scholars in this way.

But his most distinctive contribution was to the study of indigenous art in New Guinea, Indonesia (Bali) and among the Australian Aborigines. He possessed a marvellous collector's eye, and the ability to communicate his deep aesthetic sensibility in the most direct and unpretentious way. It was a revelation to allow him to untangle the web of formal elements and symbolic meanings investing an Abelam facade or a Balinese painting. His originality in developing an anthropological approach to art ought not to be underestimated; it has to be remembered that when he began his work (encouraged by Raymond Firth), studies of art had virtually no place at all in the British anthropological curriculum. That they do

now is to a considerable extent his doing. His papers on art were ahead of their time, and it is pleasant to report that they are shortly to be republished.

But, when all is said and done, Anthony the man of action was of greater significance to the world than Anthony the writer of academic papers. By the end of his career he had a long string of practical achievements to his credit, notably the establishment of a very thriving teaching department of anthropology at the ANU, which will continue to play, as it did under his guidance, a central role in the training of Australian anthropologists. He also made a most significant contribution to a number of museums: Basel, which has a collection he made in the Sepik, the Australian museum, which obtained his collection of Balinese paintings (for which he wrote an excellent catalogue) and the National Museum in Canberra. He did much to advance the position of Australian Aboriginal artists. He was also involved in development and health care projects in the Pacific.

He was able to be such an effective professor and public figure because of certain outstanding personal gifts. Strikingly tall, with the manner of an unusually