

CONTEMPORARY HUNTER-GATHERERS: Current Theoretical Issues in Ecology and Social Organization

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The purpose of this review is to highlight those issues in recent studies which I believe are potentially the most significant for the advancement of our understanding of foraging societies generally. Like Orlove's review of ecological anthropology (91, p. 235), it is "critical" rather than "encyclopedic." To narrow the topic further, my primary concern is theoretical rather than ethnographic. Yet to some extent this is a false dichotomy, since theoretical issues inevitably rest on an ethnographic foundation and ethnographic findings almost inevitably stem from theoretical interest.

A further complication arises in deciding what issues in ecology and anthropology are especially relevant to the study of contemporary hunter-gatherers, as opposed to the study of any other contemporary peoples. In his summation of the proceedings of the Second International Conference on Hunting and Gathering Societies, Bernard Arcand stated boldly that "hunting and gathering societies" did not constitute a meaningful category. If I were totally in agreement, there would be little point in my going further with this review. Yet Arcand's deliberately provocative statement represents a view with which I have some sympathy. I will save discussion of this interesting problem until the end.

Other issues currently under debate include aspects of settlement patterns, subsistence ecology and "affluence," evolution and typology (which has seen some resurgence in the past 5 years), the application of sophisticated mathematical and ecological models to ethnographic data, problems in social organiza-

tion and cognition, and Marxist approaches. Several of these issues are of direct relevance to social anthropology generally, to archaeology and ethology, and to Marxist theory.

Readers interested particularly in the application of evolutionary ecological theory to hunter-gatherer populations should consult Winterhalder & Smith's recent book (140), which contains a number of theoretical essays of interest to both archaeologists and ethnologists, as well as useful case studies. My own views are perhaps more eclectic and more skeptical of ecological models than theirs, and therefore my review touches on different problems. Nevertheless, I recommend their short essay on perspectives in hunter-gatherer socioecology (114), and a similar recent paper by Hayden (47), as complements to this review.

The Conference Milieu and the Sociology of Hunter-Gatherer Studies

It would be misleading to consider new developments in any field as isolated intellectual achievements. The exchange of ideas in hunter-gatherer studies has taken place almost as much in conference meetings as in published work. Three major conferences have been held to date: "Man the Hunter" (Chicago, 1966), the "International Conference on Hunting and Gathering Societies" (Paris, 1978), and the "Second International Conference on Hunting and Gathering Societies" (Quebec, 1980). A third "International" is planned for Munich in 1983 and a fourth for an Australian venue in 1988.

In their preface to the *Man the Hunter* volume, Lee and DeVore speculated on the reasons why the participants at that meeting had taken up the study of hunter-gatherers: "We cannot avoid the suspicion that many of us were led to live and work among hunters because of a feeling that the human condition was likely to be more clearly drawn here than among other kinds of societies" (68, p. ix). This Rousseauian notion of natural purity and cultural pollution permeates the field of hunter-gatherer studies. Foragers are perceived as more "natural" and therefore, almost contradictorily, more "human" than other branches of humanity.

The Paris "International" reawakened the "Man the Hunter" spirit, although the extravagant plans to publish the complete proceedings of that conference were never followed through. Two of the three planned volumes were abandoned, but the third (58) has now been published. It deals with *politics*, in a suitably broad sense, and contains a useful introduction (59) and some 20 papers on decision making, exchange, relations between bands, relations between the sexes, ethnicity, applied anthropology and other issues. The Quebec symposium was both more intimate and more fragmented. Participants were divided into six workshops, ranging from "Social and Symbolic Structures" to "Survival and Transformation of Hunting and Gathering Societies in

Developed Nation States " A bound collection of the papers (30) was later distributed to participants but has not been made available commercially

In addition to these large and well-known conferences, two smaller ones on related themes were convened in Ottawa in 1965 (26) and 1966 (27). The latter was not concerned exclusively with hunting and gathering societies, but several important papers on hunter-gatherers were included (see also 19, 132). Bicchieri's volume (17), although not based on conference proceedings, also provided extremely useful ethnographic summaries which were to give a stronger basis for comparative research. Finally, in 1978 a session of the American Anthropological Association meetings in Los Angeles dealt with hunter-gatherer socioecology. In general, the papers at this symposium were more theoretical and more geared to the interests of archaeologists than were the papers of the other major conferences. Winterhalder & Smith's book (140) is based on this symposium.

Each conference has consisted, to a very great extent, of younger scholars of each successive generation. The grand old women and men of preceding generations were present too, but younger scholars made major contributions both to discussion and to the growing literature of conference papers and publications. This has given hunter-gatherer studies a vitality lacking in many other subdisciplines of anthropology. Also contributing to the development of new ideas has been the deliberate international character of the events. The Paris and Quebec conferences brought Anglophone social, cultural, and ecological anthropologists face to face with some of the intricacies of Francophone structuralist and Marxist thought.

The Demise of the Steward-Service Typology

The Steward-Service typology of band societies has been remarkably resilient. It was first formulated by Steward in a 1936 article (115), which in considerably revised form is reprinted in *Theory of Culture Change* (117, pp. 122-50). It was later modified by Service (107), but its fatal flaw, the supposed widespread existence of the "patrilineal" or "patrilocal" band, still remained.

Briefly, Steward's original formulation (115) recognizes three types: patrilineal band societies, composite band societies, and matrilineal band societies. Steward tries to show that the patrilineal type is the most common. Patrilineal bands are defined as "politically autonomous, communally land owning, exogamous, patrilocal, patrilineal in land inheritance, and consisting theoretically or actually of a single lineage, which, however, comprises several households or elemental bilateral families" (115, p. 331). His examples include "N.W. Bushmen," Pygmy groups, Australian Aborigines, the Ona, and various California Indian groups.

Steward defines composite bands as "nonexogamous, bilateral in descent, lacking in rule of residence and consisting of several independent families"

(115, p. 331). In general, they are larger in population than patrilineal bands and occupy larger territories. His examples include "Southern Bushmen" generally, the Andaman Islanders, and a number of Canadian subarctic societies. Matrilineal band societies are not discussed and no examples are given, although uxori-local residence is shown to affect group composition in societies otherwise classified as patrilineal or composite. Steward's theoretical interest in this paper is in trying to show the conditions under which each pattern can be expected to develop.

The most significant innovation in Steward's later version of this typology is his discussion of the uniqueness of Shoshonean band structure (117, pp. 101-21) in terms of his concept of "sociocultural integration" (116, 117, pp. 43-63). In hindsight, it was prophetic that Steward's own ethnographic region should yield an aberrant case. For in the 1960s and 1970s it would be shown by later fieldworkers that his original noncomposite, "patrilineal" type contains as many aberrant examples as typical ones.

In the meantime, Service (107, pp. 59-109) refined the model. His "patrilocal" band societies include essentially the same ones as Steward's "patrilineal." The new label is in some cases more accurate, but still misleading, and the weakness in the overall classification is demonstrated by Service's qualifications. The Philippine Negritos and the Semang are classified as "patrilocal," even though "the evidence is not very clear" (107, p. 62). For African Pygmies, "It could be argued that the patrilocal band organization was borrowed from the Negroes, or imposed by them" (pp. 62-63). Likewise, the "patrilocal" Bushmen "probably are not good examples of either patrilocal or composite bands" (p. 63). Service's later popular book on hunting peoples perpetuates the myth of the patrilocal band, even in its revised edition (108).

Service's "composite" type, although theoretically identical with Steward's, is shown to include peoples whom Service believes were originally patrilocal (or at least noncomposite): the Algonkians, Athapaskans, Yaghan, and Andaman Islanders (107, pp. 84-94). The Eskimo, together with the Shoshone, are classified as "anomalous" (107, pp. 94-107). In short, Steward and Service sowed the seeds for the destruction of their own model, but at the same time remained committed to its empirically groundless assumptions.

Yet in spite of this, their efforts were of great benefit. Their ideas helped to generate an unprecedented enthusiasm for ecological anthropology in general and hunter-gatherer studies in particular among young anthropologists of the 1960s and 1970s (28, p. 4). During this period, new generations of scholars gave the *coup de grace* to the patrilocal model. All over the world, societies of small community size were shown to be neither essentially virilocal nor patrilineal in any sense. "Flux," "flexibility," and "fluidity" became the new buzz words to describe their social organization (see e.g. 69, pp. 7-12).

Even so, this view is not shared by all scholars. On the basis of statistical

cross-cultural comparisons. Ember (36, 37, cf Martin & Stewart 78) has tried to resurrect the patrilocal model. Specifically, the data suggest that, contrary to current opinion, recent hunter-gatherers are typically patrilocal, typically have men contributing relatively more to subsistence than women, and typically have had fairly frequent warfare" (37, p. 447). What she fails to take into account is the inaccuracy of the early ethnographies on which her *Ethnographic Atlas* sample is based. The Nharo (Naron) and Hadza (Hatsa), for example, are cited in the *Atlas* as 'virilocal' (88, p. 62), whereas recent ethnography (14, 141) suggests otherwise. Probably many more similar examples exist.

The Original Affluent Society

The ethnographic studies of the early 1960s led not only to the destruction of the old model (that of Steward and Service), but also to the generation of new ones. One such model emphasized the economic and social *advantages* of hunting and gathering and completely reversed the exaggerated assumption that foragers were perpetually on the verge of starvation, had little leisure time, and therefore failed to develop the forms of social organization associated with supposedly more advantageous means of production. On the contrary, foragers were more affluent than the armchair speculators had realized.

The data came from many societies, but probably none had more impact than the work of Marshall and Lee. Marshall's famous 'Sharing, Talking, and Giving' paper (76) emphasized the exchange relations which redistribute wealth among the 'Kung'. Lee (60-64) in turn supplied the concrete evidence for 'Kung' leisure: the fact that each adult 'Kung' spends only two or three hours per day in activities directly related to subsistence. Initially this finding was derived from only a very limited period of detailed observation and based on a rather narrowly defined notion of 'subsistence activity'. But it was significant nevertheless, given the expectations of those who had assumed (without any evidence at all) that hunting and gathering were labor-intensive activities.

Although Lee, Marshall, and others provided the data, the most articulate formulation of the theory of hunter-gatherer affluence was that of Sahlins (101, 102, 103, pp. 1-39). He distinguishes two kinds of affluence: 'the Galbraithian way' and 'the Zen road to affluence'. The former is the conventional conception which assumes that man's needs are great but his means limited. In this sense affluence is measured only in terms of goods produced or procured. Such a concept is applicable to the way in which people in market economies think, but not to most hunter-gatherer world views. Instead, foragers are prime exponents of 'the Zen road to affluence'. They do not value the accumulation of material goods. They are affluent because their needs are few and are easily satisfied by a relatively meager amount of labor time.

Sahlins attacks the ethnocentrism of earlier writers. *Bourgeois ethnocentrism*, he says, has led scholars to overemphasize material wealth in their

formalist definitions of affluence. Likewise, *neolithic ethnocentrism* has given us a misleading picture of the development of agriculture. Far from reducing the amount of labor, the Neolithic Revolution demanded *more* labor than had previous, foraging lifestyles.

Settlement Patterns and Levels of Socioterritorial Organization

Ever since Mauss's pioneering essay on the Central Eskimo (80, 81), settlement patterns and socioterritorial organization have been dominant interests in the study of North American hunter-gatherers. Mauss attributed the causes of seasonal dispersal and aggregation to environmental factors and saw the effects in social differences, e.g. the simple, private religious observances of Eskimo summer family groups versus the ceremonial occasions of the winter band aggregations. Later writers on the Eskimo have stressed regional differences (e.g. 9, 25, 29).

In the 1960s and 1970s similar approaches were applied to subarctic ethnography (e.g. 49, 50, 57, 82, 98, 99, 112). Increasing emphasis was placed on task groups and on the definition of levels of organization in both kinship composition and territorial exploitation. The ethnography of other regions lagged far behind. Writing in 1963, Meggitt laments: "The blankest pages in our ledger are those dealing with [Australian] Aboriginal local organization" (83, p. 214). Although the problem was clearly evident in the 1960s, it is only relatively recently that Australianists have managed to redress this balance and concentrate more on empirical over ideological aspects of territorial organization (e.g. 92, 93, 148) or on the interrelations *between* these aspects of territorial organization (e.g. 121, 129).

Paralleling such ethnographic studies there is today a greater interest in models drawn not only from the study of human populations, but from work in animal ecology as well. Wilmsen (139), for example, utilizes Horn's (53) territorial model derived from the study of blackbirds. Wilmsen argues that stable food resources (e.g. evenly distributed vegetable foods) are best procured from dispersed locations and that unpredictably located food resources (e.g. mobile herds of game) are best procured from centralized locations.

Similarly, Smith (113), drawing on a review of literature on animal feeding strategies (105), suggests a number of factors relating to group formation. He mentions three broad areas which entail these relations. In the first instance, aggregation may be of no benefit or even disadvantageous for foraging efficiency, while having compensatory benefits for the avoidance of predators, the defense of resources (cf 32) or reproductive strategies (cf 137, 138). In the second instance, groups may aggregate not because aggregation is beneficial in itself, but because a concentration of resources brings individuals or small groups together where such resources are located (cf 42, 147). This model, which is not unlike central place theory (23) in geography, would apply for

example to Shoshone pinion harvests and fish runs or to 'Kung aggregations at permanent waterholes in the dry season. The third and most complicated possibility is that aggregation in itself is directly advantageous for foraging activities. Smith cites as possible reasons increased encounter rate, increased capture rate, increased prey size taken, reduction in foraging area overlap, passive information sharing, active information sharing, and risk aversion.

Smith further discusses these models in terms of their application to ethnographic cases, with groupings or aggregations of varying sizes. He distinguishes four levels which he says should occur in all hunter-gatherer populations: "foraging groups" (task groups), "resource-sharing groups" (camps or local bands without territorial identity), "information-sharing groups" (not really groups at all, but networks), and "coresident groups" (any level of group which resides at a particular place at a particular time). Ironically, this applicative aspect of Smith's work tells us essentially that ethnographers have been on the right track all along in their empirical characterizations of group structure.

The next logical development in models of local organization would be the exploration of still more deductive procedures, based on the notion of logical possibilities. This may be of significant theoretical interest, especially to archaeologists who presently hover between trendy ethnographic analogies and antiquated "if I were a horse" methods of deduction. To take an example, with one relevant environmental factor (e.g. the seasonal distinction summer/winter) and one variable of social organization (aggregation/dispersal), four patterns of seasonal settlement could be generated (including permanent aggregation and permanent dispersal). All four can be found among Kalahari Bushman groups (12). If we add a second variable (e.g. upland/coastal lowland), 16 patterns are generated. While some of these may be unlikely, the probability is that more than just one, say, the summer-upland-dispersal/winter-coastal-aggregation model derived from Clark's (24) interpretation of Star Carr, will be viable.

Comparative Studies

THREE STYLES OF COMPARISON It is impossible to establish ecological correlations without cross-cultural comparison, although some ethnographers have tried. Those who have grounded their theories about the relationship between environment and social organization in a comparative framework have employed three logically distinct methods: illustrative comparison, regional comparison, and global-sample comparison (104, pp. 53-74).

In ecological anthropology, illustrative comparison is a frequently used but dangerous technique. It is characterized by the use of casual or unsystematically chosen examples to illustrate, rather than test, hypotheses. The danger is that authors frequently assume that they *are* testing their hypotheses or finding correlations where none exist. Bicchieri (16) for example, compares three

African foraging societies: the net-hunting Mbuti, the Hadza, and some undifferentiated southern Bushman groups. His chosen variable is permissiveness of environment (that of the Mbuti is most permissive and that of the Bushmen is least permissive). Many of his specific comparisons do yield interesting ideas for future research. Nevertheless, these three cases constitute neither an ethnographic region nor a global sample. A regional-comparative study of Bushman societies would yield examples which are not consistent with his generalizations on ritual and belief, for example. Such a study of Bushman groups might effectively limit the number of ecological and cultural variables where Bicchieri's study does not, only then could a full explanation of causative factors begin. Alternatively, a global sample of a statistically significant number of cases might yield a similar basis for correlative explanation.

In an earlier paper (12, pp. 131-32), I criticized Yellen (146, p. 48) on similar grounds. He suggests that a careful comparison of his own very thorough data on the 'Kung with those of Gould (41) on Western Desert Aborigines would give correlations between the distribution of resources and settlement patterns. I argue that this is not the case. The 'Kung, as Yellen notes, aggregate in the dry season and disperse in the wet season, but so do some Western Desert Aboriginal groups. Gould's Aborigines aggregate in the wet season and disperse in the dry season, but so do various Bushman groups not mentioned by Yellen (see e.g. 110, 119). Evidence for microenvironmental causes for radically different settlement patterns is strong for the Bushman, but the illustrative method will not provide it.

Even so, careful illustrative comparison with more limited objectives can raise interesting issues. Peterson's (94) discussion of territorial adaptations among the 'Kung and Australian Aborigines is a case in point. He argues convincingly that there is greater similarity between the 'Kung and desert Aborigines (Pintupi, Walpiri, and Aranda) than has often been supposed. Specifically, similarities in territorial organization have been obscured "because the behaviour of the one people ['Kung] has been compared with the ideology of the other" (94, p. 124). While recognizing the obvious differences, Peterson suggests that there are marked similarities, both in rights in residence and resource allocation, and in the ways in which the two societies cope with demographic fluctuation. In a similar way, Turner's (127, 128, 130, 131) ingenious comparisons of Cree and Australian Aborigines have generated a new paradigm for the study of hunter-gatherer (and non-hunter-gatherer) social formations. This paradigm is based on the opposition between "kinship-confederational" (Australian) and "locality-incorporative" (Cree) principles. Although its use in limited comparative studies does not lead directly to any correlations, it has suggested new ways of approaching kinship, settlement patterns, and mythology.

Both global-sample comparison and regional comparison are superior to illustrative comparison for finding correlations. However, the global-sample method also has its drawbacks. Choosing a sample is the main problem. It must be suitably large, suitably global, and suitably diverse to offset the coincidence of common origin or diffusion between contiguous societies. Murdock, the founder of the method, has been well aware of this problem (88, pp. 1-6, 89).

REGIONAL COMPARATIVE STUDIES The method of regional comparison implies careful control. It is conceivable that controlled comparison would not have to be regionally based, but certainly it is safer if it is. As I have argued above, two demographically similar hunter-gatherer populations do not constitute a controlled sample any more than do three disparate African foraging populations.

In the 1972 issue of the *Annual Review of Anthropology*, Heider (48, pp. 213-17) commends four attempts to make environment the independent variable. "Three of these deal with hunter-gatherers (29, 100, 126). Surprisingly, given the amount of data now available, few have followed this course. However, two attempts have been made with Kalahari Bushman data (12, 21), one with northern North American data (97) and one with Fuegian data (118), each of which has yielded interesting results and new theoretical insights. Stuart's paper (118) is particularly significant. Comparing the Ona and the Yahgan, he demonstrates the incompleteness of the patrilineal/composite distinction and substitutes a notion of flux between two ideal types: homeostatic and disequilibrium. The "patrilineal" bands of the Ona are seen as forming part of a relatively homeostatic system in which a small, transhumant human population hunted small herds of game. In contrast, the apparently "composite" features of Yahgan social organization, i.e. bilaterality, neolocality, a lack of local exogamy, and high population aggregates in outlying areas, imply a state of social disequilibrium possibly resulting from changes in environmental circumstances. Insights such as this are at least as likely to emerge from regional comparison as from more comprehensive but less detailed surveys of the entire world.

Marxist Approaches

The last two decades have seen a growing interest in Marxist theory in anthropology, and with it a growing confusion about its applicability to the study of hunter-gatherers. Marxist anthropology is often quite far removed from Marx's own interests; it has come to mean the science of applying rather nebulous underlying principles of dialectical materialism to ethnographic reporting and interpretation. This is all the more problematic when dealing with classless societies: either they are vestiges of a "primitive communist" mode of

production (51, 56, 65, 66), or class-like structures based on age or sex must be found to complete the dialectic. Fortunately, Marxist hunter-gatherer specialists, unlike some of their West Africanist colleagues, have steered clear of the latter approach.

Instead, two general schools of thought have emerged, each of which claims to follow the spirit rather than the letter of Marx's *Capital*. I shall refer to these schools as "land and labor" and "structural and ecological" Marxism respectively.

LAND AND LABOR MARXISM Meillassoux's (85-87) analysis of foraging society rests on Marx's (79, vol. I, pp. 174-80) distinction between land as an instrument of labor (in agricultural societies) and land as a subject of labor (in foraging societies). On the latter Meillassoux comments:

It is sufficient for present purposes to say that the use of land as a *subject of labour* fosters a type of "instantaneous" production whose output is immediately available, allowing a process of *sharing* which takes place at the end of each enterprise. The hunters, once they share the common product, are free from any further reciprocal obligations or allegiance. The process gives no ground for the emergence of a social hierarchy or of a centralised power, or even the extended family organisation. The basic social unit is an equalitarian but unstable band with little concern for biological or social reproduction (86, p. 99, italics in original).

In contrast, the use of land as an instrument of labor "introduces a radical change into the entire social, political and ideological structures" (86, p. 99).

The distinction is important for Meillassoux as part of his argument that the development of agriculture implies control over people (the means of production) rather than simply control over land (the means of material reproduction). "Kinship," in the sense of family and lineage as productive units, emerges only with the development of agriculture and its consequent accumulation of surplus [for an alternative but complementary view, see (13, 15)]. Yet Meillassoux's paper is important for *hunter-gatherer studies* for a different reason, specifically for drawing attention to the fact that foraging economies are based on rights of access to land, rights to shared resources through various forms of exchange, and the relatively egalitarian forms of social structure (relations of production) which permit such economic activities. Meillassoux's assessment of the forager/nonforager or forager/postforager dichotomy foreshadows both Woodburn's typology of band societies (142, 144, 145) and recent ethnographic and theoretical discussions of forager, and particularly Bushman, reciprocity [(20, 31, 135, 136), for an overview of sharing customs in foraging societies, see (95)].

STRUCTURAL AND ECOLOGICAL MARXISM Like Meillassoux, Godelier is an ethnographer of cultivators and a theorist on foragers. Both Meillassoux (85, 86) and Godelier (38, pp. 66-82, 39, 40, pp. 51-62) offer reanalyses of

Turnbull's (125) sketchy but nicely written Mbuti material and argue a case for the relative kinshiplessness of hunter-gatherers. Yet the theoretical work of Godelier and his followers is on the whole different from Meillassoux's, for it entails a more sophisticated understanding of ecological effects on social organization and an obvious debt to the best of both British functionalism and French structuralism. These debts and the apparent similarity between Godelier's approach and that of the substantivists have left Godelier seemingly vulnerable to Marvin Harris's recent attack (46, pp. 216-57). Godelier's structural Marxist approach is very different from Harris's infrastructural reductionism (see 90), and from Sahlins's substantivism as well (see 73), although there are peculiar similarities between Marxist and Stewardian theories of society.

Steward's "levels of sociocultural integration" (116-117, pp. 43-63) resemble the Marxist notion (or notions) of "modes of production." Steward's "cultural core" (117 *passim*) and the implicit residual category of "cultural periphery" represent respectively notions not unlike "infrastructure" and "superstructure." Testart (120, 122, 123), a member of Godelier's *équipe*, has been particularly concerned with theoretical problems closely related to those in Steward's work, while other French writers, e.g. Bahuchet (5-8) in his descriptions of the Aka Pygmies, are pursuing practical research interests in the mainstream of cultural ecology.

The differences between Stewardian and Marxist views emerge when we look at concepts of evolution. Steward's "multilinear" evolutionism denies the utility of any concept of stages of evolution, which is so crucial to the Marxist interpretation of history (if not to Marxist interpretations of particular social formations). In Steward's view, the broad "universal" stages postulated by Leslie White and V. Gordon Childe are so general that they are neither very arguable nor very useful" (117 p. 16), while nineteenth century "unilinear" evolutionism can be readily dismissed on empirical grounds. Modern Marxism wavers somewhere between universal and unilinear evolutionism, or even denies any commitment to determinist concepts of evolution (4, 71).

In my view, these different yet convergent interests of cultural ecology and ecological Marxism highlight the incompleteness of both perspectives as presently formulated. There are two areas in which a more radical move away from incipient environmental and technological determinism and evolutionism might be of benefit to Marxist analysis. The first is in the area of social transformation, where Marxism needs to be liberated from such ethnocentric notions as "precapitalist society." This notion is literally meaningless except in terms of an assumed norm of capitalism, and its use implies a unilinear or universal evolutionary dichotomy (precapitalist/capitalist) which is of very little significance for the study of the range of variation in human society.

The second area in which I would argue for a bolder approach is in the

assessment of the relative importance of superstructure and infrastructure. Although cultural ecologists and Marxists agree on the primacy of infrastructure, the "cultural core," Marxists today are showing considerably greater interest in superstructure than cultural ecologists ever have. The problem is that there is still a tendency to regard these elements of culture as "peripheral", this is a strange thing given that Marxism purports to be a theory of history. The fact is that Steward's phraseology is very misleading. It could be argued that the really central elements of a culture are those which are *not* affected by changes in environmental circumstances or technology, rather than those which are so affected. Social organization (perceived by Marxists as part superstructure and part infrastructure) is determined both by environmental constraints and by relatively fixed ideological premises, which will be found cross-culturally within a culture area and through time within a particular culture. This point comes out most clearly in Australian Aboriginal ethnography (74, 84).

The whole area of ecological and Marxist interpretations of social organization is presently one of major theoretical debate. Abruzzi (1, 2) propounds population pressure determinism and attacks Turnbull (126) for failing to provide evidence that the environment is as permissive as he (Turnbull) maintains, or that flux is a cultural rather than an ecological phenomenon. Godelier (39) finds the contradictions not in Turnbull's writings but in Mbuti society. He attacks cultural ecology for failing to see that classless foraging societies are sometimes plagued by contradictions between production and ideology. Legros (72) assesses precisely similar contradictions in Nunamiut society and argues that flexible and cognatic group structure hinders the cooperation required for collective hunting. Balıkcı (11) defends the traditional ecological perspective and charges Legros and Godelier with putting lofty theories ahead of the facts. Using a more empirical approach, he then comes to a conclusion not unlike that of Godelier and Legros. For Balıkcı the contradiction in foraging society, or at least in his Netsilik data, is between the rigid egalitarianism of the ideology and the "pecking order" hierarchy which can be observed in individual behavior (cf. 10, pp. 173-93). The situation may be much more complicated in other foraging societies, however, as Biesele (18) and Silberbauer (111) show in their studies of Bushman political pressures (cf. 64, 70, 77).

New Typologies

With the Steward-Service typology in tatters, a number of new typologies have appeared. Woodburn's (142, 144, 145), Testart's (123) and two by Watanabe (133, 134). Their significance lies not so much in classification in its own right, but rather in the issues which stem from the typology-making process.

WOODBURN'S TYPOLOGY Woodburn has drafted a "preliminary formulation" (144, p. 115) of a bipolar typology: immediate return and delayed return.

His tentativeness refers not so much to the general distinction between these two concepts, but to the means of assessing the various ethnographies according to the distinction he draws

Immediate-return economic systems are characterized by a behavior and attitude which rejects the notion of surplus. As Woodburn says in reference to the Hadza, "Encumbrances are unacceptable and people do not take on even short-term commitments which might provide a few additional days of desirable food" (144, p. 100)

Delayed-return systems, in contrast, allow for planning ahead. In this category are included part-time hunters, sedentary hunter-gatherers, fishermen who invest in boats or large-scale fish traps, trappers who invest in labor-intensive hunting traps, beekeepers, mounted hunters, and, perhaps anomalously (from my point of view), all Australian Aborigines. Part-time hunters are delayed return because the amounts of time and energy they put into horticulture or stock-keeping, even where these are relatively insignificant for subsistence purposes, necessitate a delayed-return social organization. Such a form of social organization, to turn the argument full circle, is one which encompasses task specialization sufficient to allow a surplus so that those engaged in long-term tasks are supported by those actively engaged in immediate pursuits. The same principle applies to the fishermen, trappers, beekeepers who use man-made hives, and hunters (such as the Plains Indians) who invest their time in keeping horses. Sedentary and semisedentary hunter-gatherers are delayed return because of the accumulation of property and of surplus which ensues from this way of life.

So why the Australian Aborigines? Woodburn's argument is that the Aboriginal men "maintain and transmit long-term rights over their female kin" and that Aboriginal social organization is "centrally and essentially connected with the maintenance, manipulation and transmission of these long-term rights" (144, p. 109). On the whole, food production in Australia resembles that of immediate-return producers in other parts of the world, but in light of Woodburn's wider view of production (which includes reproduction), Australian systems come to resemble instead those of delayed-return cultivators. In short, Australians "farm out" their women (144, pp. 108-9, cf. pp. 107-11).

Significantly, Woodburn's model specifically rejects technology as a major factor and downplays the role of the environment. Societies with immediate-return systems have both the technology and the environments to engage in delayed-return activities, and delayed-return societies could similarly adapt to perform immediate-return activities. Ideology is the causative principle, although Woodburn himself emphasizes effects rather than causes. His primary interest is in the resultant forms of social organization. One of the major characteristics of immediate-return systems is their egalitarianism, which is lost in delayed-return systems (145, cf. 143).

It is well worth pointing out that the immediate/delayed dichotomy is applicable not only to hunter-gatherers but to all societies. Some hunter-gatherers have immediate-return systems and others have delayed-return systems, but non-hunter-gatherers invariably and of necessity have systems of the latter type. Thus societies such as the Nuer, the Trobrianders, modern Japan, feudal Europe, subarctic Indians, and neolithic farmers are included in one type (delayed), and the 'Kung, the Hadza, the Batek, the Malapantaram, and the Mbuti are included in the other (immediate).

In spite of Woodburn's tentative exposition, his theory is as bold and radical as any comparative economics has yet produced. Instead of a capitalist/precapitalist, civilized/savage, or producers/foragers dichotomy, we now have one which pushes the boundary between ourselves and our primitive contemporaries much more toward their end of the scale. This, above all else, is what makes Woodburn's typology so interesting.

TESTART'S TYPOLOGY Testart (123), in a paper originally delivered at the Quebec conference, suggests a typology similar but with more distinctions than Woodburn's, and more clearly substantive (as opposed to formal). Like Woodburn, he emphasizes the differences between hunting-and-gathering economies in terms of their relative reliance on non-hunting-and-gathering modes of subsistence, but he also takes particular account of the degree of contact hunter-gatherers have with herders and cultivators. Testart's paper gives a detailed account of the social organization of four types, the last including three subtypes.

Mounted hunter-gatherers of central Asia, Argentina, and the Great Plains of North America are characterized as extremely nomadic and relatively inequalitarian.

Storing hunter-gatherers include Northwest Coast Indians, western Siberians, and southeastern Siberians. These societies are characterized as sedentary, dense in population, inequalitarian, and developed in terms of exchange relations, warfare, and division of labor between the sexes.

Nonstoring, sedentary hunter-gatherers are few in number. The only definitive example cited is the Asmat of West Irian, but the semisedentary 'Kung and various semiaccumulating societies are mentioned as having a tendency toward this form of socioeconomic organization. The importance of the type lies in its postulated occurrence in areas of relative abundance of resources, and therefore in its potential for the transformation to agriculture.

Nomadic hunter-gatherers are widespread but normally either "enclosed" (*enclavés*) or "circumscribed" (*circonscrius*). Enclosed nomads are egalitarian, but subordinate to the surrounding agricultural peoples on whom they depend for technology and trade. Typically, they use the language of and share other aspects of culture with their dominant neighbors. Examples include most Asian

hunter-gatherers, African Pygmies, and the Bushmen of Angola. In contrast, circumscribed nomads are not economically or culturally dependent upon their neighbors. They occupy the marginal, less-fertile territories not utilized by cultivators or pastoralists. Examples include North and South American hunter-gatherers and the Bushmen of southern Africa. Testart's residual subcategory of "others" belongs to the Australian Aborigines, characterized by cultural isolation and homogeneity.

Interestingly, both Woodburn's and Testart's typologies take the accumulation of surplus as the single most important typological distinction. Yet accumulation is seen as important not in its own right, but either as a result of a changing ethos which marks the difference between immediate-return systems and all others, or as a prerequisite for the transformations leading to social hierarchy. Though in my view less interesting for anthropology generally, Testart's classification is more directly relevant to the understanding of contemporary hunter-gatherer social organization. Ironically though, Testart himself sees the typology as a step toward refuting the existence of a "Neolithic Revolution" (123, pp. 213-15, cf. 122, 124). He holds that surplus in foraging societies creates social hierarchy [for a contrasting view, see (65, 66)].

WATANABE'S TYPOLOGIES Typologies which recognize a category of hunting and gathering societies either as a type itself or as a universe within which types are delimited are usually formulated either on the basis of mode of production (e.g. Woodburn's, Testart's) or on the basis of settlement pattern or social organization generally (e.g. Steward's, Service's, and the implicit typologies of those writers who speak of "band societies"). Watanabe's first typology (133, p. 70), proposed at the "Man the Hunter" conference, is of the latter kind. In this typology he recognizes six main types or a total of eight subtypes of hunter-gatherer social structure. Their defining characteristics stem from the relation between the seasonal cycle and what he terms the "residential shift patterns." The six or eight types are ordered on a scale of increasing residential stability from fully nomadic to permanently settled. Since that typology was in Watanabe's view tentative and has now been superseded, and since it is in my view of limited value, I will not discuss it further here.

In contrast, Watanabe's 1978 (134) typology is based on a new principle, not mode of production or settlement pattern, but "food habit." This typology identifies three major types of food habit on a spectrum from largely vegetarian to largely carnivorous.

His Type I is defined as 'largely vegetarian.' His examples are the Semang of mainland Malaysia and the Paliyans of south India. Both populations subsist primarily on root crops. Planned hunting is only a part-time activity.

Type II, "mixed diet" peoples, includes several subtypes, but their defining feature is that planned hunting is a "full-time" activity for men. Type II-A

peoples have plant foods as staples. These include Type II-A-1 peoples, those for whom small game is more important than large game (he cites the Yiwara of Australia and the Shoshone, both with seed crops as staples), and Type II-A-2 peoples, for whom large game is more important (the nut-eating Bushmen, i.e. 'Kung, and the root-eating Hadza). Type II-B peoples have animal foods as staples. This type again is subdivided. Type II-B-1 includes those groups with mammals as staples (e.g. the Kutchin and the Blackfoot, both land-mammal hunters). Type II-B-2 are those with fish as staples (e.g. the Koryak of Siberia and the Sanpoil of the Northwest Coast, both salmon-eaters).

Type III is defined as "largely carnivorous." For these peoples, plant-food collecting is a negligible activity. Watanabe's examples are the Nunamiut, who subsist on land mammals, and the Tareumiut, who subsist on sea mammals.

Watanabe explicitly relates his classification of hunting-and-gathering peoples to that of animals as herbivores, omnivores, and carnivores, although the subdivisions within his Type II indicate that the problem is more complicated than this. He concludes by noting that the spectrum of variation in hunter-gatherer food habits seems to be roughly correlated with the distribution of the world's climatic zones. Relatively carnivorous peoples tend to be found in the northern hemisphere, in cool climates, whereas relatively herbivorous peoples tend to be found in the southern hemisphere, in warm climates. This could have implications for the study of human prehistory.

It is interesting that Watanabe's typology is not formulated primarily on the basis of the kinds of plants gathered or animals hunted, but rather on the relative importance of gathering and hunting activities. This suggests that social organizational features are implicit in the typology, perhaps as a result of food-procuring activities. There is no space here to consider this problem, but future comparative research might do well to take the principles of Watanabe's classification into account.

Hunter-Gatherers: A Meaningful Category?

The existence of "hunter-gatherers" as a meaningful category for research entails two general fields of inquiry: first, the extent to which hunter-gatherers can be distinguished from non-hunter-gatherers, and secondly, the extent to which this distinction in subsistence pursuits is meaningful for the comparative study of other aspects of culture. For the purpose of this review, I shall confine my comments to the first field of inquiry [cf. Arcand (3), who deals with both].

Ellen (35, pp. 170-76) has recently drawn attention to the difficulty in describing subsistence pursuits.

Much confusion has been caused in the ethnographic and comparative literature by assuming ostensibly predominant subsistence techniques represent total subsistence strategies, and by a general use of excessively simple criteria for the description of life-support techniques. For example, we happily describe Eskimo, Hadza and Tasaday as "hunter-gatherers", the

Nuulu, Gadio Enga, Iban and Hanunoo as 'swidden cultivators'. when the differences between them *in purely subsistence terms* are of as much social and ecological significance as those between populations to which we attach different labels (35, p. 170, *italics in original*)

As the subtitle of his book suggests, Ellen is interested in "the ecology of small-scale social formations," not in a narrowly defined concept of subsistence. He argues further (35, p. 174) that such activities as collecting firewood or appropriating resources for exchange also involve social and ecological consequences. Similar points are made by David Harris (43–45) in his descriptions of subsistence and trade in the Torres Strait Islands and Cape York Peninsula. He argues that the boundary between foraging and cultivation is not easily definable, and that trade networks further blur the distinction between social types.

Ingold (54, 55) considers an analogous case for a blurred distinction between hunting and herding. In his earlier paper (54) he outlines four forms of Lappish reindeer exploitation and the transformations between them. These include the exploitation of 'domesticated' deer in a hunting economy, symbiotic pastoralism (individual ownership of deer and a 'social contract' between these deer and their owners), predatory pastoralism (pursuer and pursued), and true stock-rearing. In his recent book, Ingold (55) gives a more theoretical model of the relationships between land, animals, economy, and ideology. In this, he defines three oppositions: the 'hunting—pastoralism—ranching triangle.' His point though is less concerned with fuzzy boundaries and more with explaining the logical distinctions between ideal types, distinctions through which we can explore the complex ecological relations between men and animals.

Finally, here it is important to consider the difference between viable foraging communities and dependent fringe-camp dwellers (former hunter-gatherers who attach themselves to a wider non-hunter-gatherer society). As Sahlins (103, pp. 8–9) points out, writers have sometimes been thinking of the latter when supposedly describing the former. A danger nowadays, however, is the tendency to *overemphasize* some of the differences between 'pure' and part-time foragers. In the 1960s it was taken for granted that there were three levels of acculturation among Bushmen: for example 'pure' hunter-gatherers, clients of black pastoralists, and clients of white ranchers (60, pp. 11–37, 109). Bushmen associated with whites were erroneously assumed to be more acculturated than Bushmen associated with blacks, and the transition from foraging to stock-keeping was erroneously assumed to be irreversible. Recent ethnography among widely separated, 'acculturated' Bushman groups has shown much greater dependence on hunting and gathering than had been supposed (22, 52), just as recent archival and archaeological work suggests a long history of herders becoming foragers and foragers becoming herders (33, 75, 106). Social change and acculturation are very complex problems, as hunter-gatherer specialists are only beginning to realize.

Conclusions

As I have implied throughout, the current status of hunter-gatherer studies is in disequilibrium. The interests of specialists are divergent, models and counter-models abound, and even the existence of hunter-gatherer studies as a definable subdiscipline is being questioned. My own views on the last issue aside, this is a beneficial state of affairs for progressive work, both specialized and integrative. *Man the Hunter* (67) was hailed by some reviewers (e.g. 34, 96) as if it were almost the final word on the subject, however, the last 15 years, and especially the last 5 years, have shown that this was not the case. New paradigms and exciting new debates are still arising, and there is every sign that this will continue for a long time to come.

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