TOWARDS A GENERAL MODEL OF THE EVOLUTION OF RANKING AMONG FORAGERS

Kenneth M. Ames

INTRODUCTION

Several recent papers and presentations (Ames 1981; Burley 1979b, 1980b; Langdon 1976; Matson this volume; Schalk 1977) have dealt directly or indirectly with the development of rank societies on the Northwest Coast. Burley and Langdon attempt to explain the development of particular archaeological or historic cultures, Marpole and Nootka respectively, while I attempt, through a general, processual model, to link the development of ranking on the coast to the development of ranked and stratified societies anywhere. Schalk's discussion of the issue is somewhat tangential to his model of salmon as a resource on the coast. Matson sees ranking evolving in conjunction with sedentism and subsistence intensification. Other studies by Fladmark (1975) and Borden (1975) treat what these authors see as necessary preconditions for these developments, but not with their actual course.

In my paper (Ames 1981), I proposed that ranking evolves because, under the appropriate conditions, it provides improved monitoring of the environment and improved responses to environmental shifts through hierarchical information flow. Flexible adaptations were described in terms of systemic resilience (*sensu* Hollings 1973:17) and rank societies as stable (*sensu, op. cit.*) systems, with less systemic flexibility in the face of environmental shifts. In a resilient system, subsystems or variables

---

Kenneth M. Ames, Department of Sociology, Anthropology and Criminal Justice Administration, Boise State University, Boise, Idaho, 83725.

may assume a wide range of values and the system persists, while in a stable system survival depends upon the ability of systemic variables to maintain a limited set of values despite environmental shifts. Thus a stable system is one which maintains its internal coherence and form and is relatively inflexible.

I argued that limitations were placed upon early, resilient adaptations on the coast by the regional specialization in salmon fishing and by circumscription. Food storage was seen as a critical element in the salmon specialization. Circumscription was used as a rubric essentially for locational constraints imposed by the distribution of resources, but which caused competition between human groups, leading to warfare. The procurement and storage of salmon was seen as imposing a need for social organizational complexity and leadership. The processes bringing ranking into being were population growth, sedentism and promotion.

My intention here is to link that model to more general models and theories of the development of hierarchial societies and of cultural evolution. Implicit in the orginal model is the idea that the processes involved are universal processes which may vary in their intensity, scale and duration in particular historical contexts, but that hierarchical societies on the coast are explainable by the same body of theory employed to explain the rise of the classic Maya, states on the Peruvian coast or Bronze Age chiefdoms in southern England. This conviction also seems to underlay at least the work of Schalk and Matson. My discussion will emphasize the ideas of resilience and stability, how they provide a framework for discussing change and a basis for linking that framework to more general theory.

In this discussion I will draw upon cultural evolutionary theory and historical materialism. By cultural evolution I mean the kind of theory being outlined by, among others, Durham (1976) and Dunnell (1980). This theoretical approach is the more congenial, at least to Americanists, while archaeological applications of historical materialism seem primarily limited to Europe and Britain (e.g., Friedmann and Rowlands 1977; Tilley 1981). A detailed review of both these classes of theory is far beyond the scope of this paper, and, with regard to historical materialism, the competence of this writer. However, there are certain basic tenets of historical materialism which seem critical to any theory of cultural evolution. Among these are what Godelier (cited in Friedman 1974:417-449) terms inter-systematic and intra-systematic contradictions. However, before pursuing this point, several other issues must be discussed.

The linkage between cultural evolution on the one hand and resilience and stability on the other will be made by use of the concept of adaptive strategy as it has recently been employed by Cordell and Plog (1979), Bettinger (1978) and Jochim (1981). The major emphasis in the paper will be on making that connection. When that has been done, the discussion will turn first to Godelier's contradictions and then, in conclusion, to what all this may tell us about the development of hierarchies among foragers in general and on North America's western and eastern coasts specifically. It is first necessary to outline what I feel to be archaeology's role in building theories of cultural evolution.

HISTORY, HETEREOGENEITY AND SCALE

Much of the recent archaeological work on foragers (e.g., Binford 1978, 1980; R. Gould 1980a) rests upon the assumption that there are general, or global processes at work shaping forager adaptations and that these apply to any group. It is sometimes further assumed that these processes are not only universal but ahistorical as well, i.e., they not only do not show local and regional variation in tempo and rate, but do not show temporal variation either. (I am not implying that either Binford or Gould are ahistorical; they are not.) Trigger (1978a) attributes this assumption to archaeologists viewing archaeology as a generalizing or nomothetic social science and misunderstanding the role of historical explanation in archaeology. He sees archaeological explanation as having four goals: 1) generalize about the nature of culture and human behavior; 2) to test existing theories; 3) to explore interconnections between existing bodies of theory; and, 4) to explain the past (Trigger 1978a:51). He sees number four as the major goal.

Goals number two and three involve applying social (and natural) science theory to explain historical events. He sees archaeology then as a particularizing, or idiographic, science, not generating theory. Social science theory, in his view, can only be generated studying living peoples. He uses paleontology as his role model for archaeology, arguing that paleontology cannot generate evolutionary theory, but does test it. However, there have been serious efforts to change paleontology into a nomothetic science (S.J. Gould 1980; Ayala and Dobzhansky 1974), with the result of forcing a serious reconsideration of evolutionary theory (Eldridge and Gould 1972; Gould and Eldridge 1977). Evolutionary theory based upon shortrun observations of modern experimental data was not accounting for patterns observed in the fossil record, according to some paleontologists. They have therefore formulated a theory based upon the fossil evidence to be tested both on experimental and paleontological data, where the issue currently rests.

Here then is the role of archaeological explanation, to formulate and test general theories of cultural evolution against the historical data that are the archaeologists' particular province. The goals are twofold and equal in importance: to generate theory and to explain the past. None of this is new. What is important here, however, is the emphasis upon explaining the past; explaining particular historical sequences or events is an important an archaeological goal as constructing nomothetic theories. Further, the nomothetic theories themselves must take history into account.

Much of current explanation in archaeology relies on the concept of adaptation (Dunnell 1980). Winterhalter's (1980) criticisms of environmental analysis are applicable to common anthropological usage of adaptation:

1. there seems to be little realization that adaptations reflect the action of past environments. Thus present, or ethnographic present, behaviors are only partially the product of present circumstances. In other words, adaptations display time lag.

2. Ethnographic descriptions are usually based upon short term observations and thus the adaptations described are treated as though they are static, lacking any dynamism. (Archaeological interpretations based upon such descriptions reconstruct a static past.)

3. Environmentally caused cultural changes can get overlooked. (This is less a problem for archaeology.)

4. Typological environmental classifications and descriptions mask critical environmental variation.

5. Much of cultural ecological theory is ahistorical, as a result, in part, of points 1-3.

6. The idea of adaptations is misunderstood by anthropologists in any case.

According to Winterhalter, the emphasis should be upon environmental analyses which stress the dynamic nature of environments, i.e., with temporal and spatial variability. In his view, and that taken here, "Ecological adaptations result from historical processes in natural ecosystems which have as their most important characteristics temporal variance and spatial heterogeneity" (Winterhalter 1980:136). Our understanding of these processes must be based upon the physical limitations of events, of rates, magnitudes and distances of real environments. What is required then is detailed and careful application of general theory to particular historical sequences. There does have to be, on the other hand, a general theory to apply, and archaeology is as capable of generating it as is paleontology.

ADAPTATION AND STRATEGIES

I have suggested elsewhere (Ames 1979, 1981) that cultural adaptations on the northwest coast, or anywhere for that matter, can be analyzed using two polar types defined by Holling (1973): resilient and stable systems. These are heuristic devices, and do not represent any real system, but systems may be described in these terms. As heuristic devices, these types offer certain advantages (Ames 1979:223, 1981:792), one of which is that adaptive success is measured on some basis other than survival or population growth. Rather, adaptive success is measured using the persistence of relationships among systemic variables, including population size. Thus change, evolutionary change, occurs when relationships among variables change, break down, restructure, etc.

There are two questions to be answered here; first, what are the systemic variables being discussed, and second, what are the ramifications of the definition of evolution implicit in the statement that evolutionary change occurs when relationships among variables change?

The first question may seem to be the old question of what cultural elements are important for understanding cultural evolution, the answers to which usually approximate Steward's "culture core" (Steward 1955) or White's layer cake model of culture (White 1959), that is, technological, subsistence and economic variables are the most important. This view is not adequate. From an evolutionary standpoint it is now evident that many traits are adaptively neutral, possibly even certain subsistence activities, though that is less likely than that traits associated with ritual may be neutral. However, traits associated with ritual can be adaptive; these ritual traits may be part of what historical materialists call the relations of production, that is, "those social relationships which dominate (i.e., determine the economic rationality of) the material process of production in given technological conditions at a given stage of development of the forces production" (Friedman 1974:946). That is to say, that social and ideological variables are as important in determining the form of a particular adaptation as are subsistence variables. While certainly not a new insight, this important point frequently gets forgotten. In any case, variables which are adaptively neutral or adaptively positive will have to be discovered with each instance, though theory can predict what general classes of variables to examine. I am going to discuss these variables in terms of strategies or solutions to adaptive problems.

> It is self evident that any living species, at least any species not about to become extinct, has a valid set of solutions for certain basic ecological, or biological problems. It must secure food to replenish its energy store, must have a place to live, and a method to reproduce and thus perpetuate its genes in succeeding generations.
>
> (Dobzhansky 1974:323, cited in Jochim 1981:14).

Particular solutions are strategies in the terminology used here. As Jochim (1981) notes, this definition with its insistence upon genetic perpetuation raises problems for the analysis of cultural evolution. These problems will be discussed below. The basis or level of these solutions is not specified, thus it may be individuals, populations, societies, etc. (c.f. McCay 1981:357). It follows from this, as has already been mentioned, that there may be cultural elements which are not solutions to environmental problems, thus they are not strategies. They may, however, limit the choice of available strategies (e.g., McGovern 1980). At this point it is necessary to turn to the second question, the implications of the statement that evolution is changes in the relationships pertaining between variables.

This view of cultural evolution is consonant with what Dunnell (1980) has termed the transformational view of cultural evolution, i.e., that systems are transformed and become increasingly complex. They are not replaced. This view is perhaps most clearly expressed by Buckley's concept of morphogenesis (Buckley 1967). Dunnell criticizes this view in his excellent paper on cultural evolution. A statement of his criticisms is essential. In Dunnell's view:

> Evolution is a particular framework for explaining change as differential persistence of variability ... evolution views change as a selective and not as a transformational process. Variability is ... discrete. Change is accomplished by alteration in the frequency of variants rather than alterations in the form of a ... variant.
>
> (Dunnell 1980:38)

From an anthropological viewpoint, this is a radical view of change, which stems from Dunnell's close adherence to the idea, dominant in biology, that natural selection acts upon individuals (phenotypes) and that evolution can be withnessed in changes in gene frequencies across generations. Dunnell is not alone among anthropologists in this approach (e.g., Durham 1976; Jochim 1981). Dunnell does not, however, offer a suggestion as to what the discrete unit of cultural variability is, nor how it arises. This is not a criticism since that problem is a extraordinarily complex one.

It is now clear, for instance, that genes themselves are not the unitary, discrete units of inheritance envisioned by Mendel. Genes may be pleiotropic, affecting many traits, or polytropic, many genes producing a "single" phenotypic trait such as skin color. Thus selection acts not only upon genes, but on the relationships pertaining between genes. As genes alter in frequency, their relationships change as does the resultant organism, sometimes in unforeseen ways. Thus it would be simplistic in this case to argue, for example, that cultural evolution proceeds as the adaptive strategies employed by a cultural tradition change frequencies, some becoming dominant, others rare or disappearing. If selection acts upon human behavior in the manner envisioned by Dunnell, it acts upon sets of "solutions to adaptive problems," upon constellations of strategies.

What I am going to suggest is that cultural evolution proceeds in a manner reminiscent of species selection (Stanley 1979). Before introducing this concept, the following *caveats* are in order. First, this is a controversial theory in biology, and is propably not held by the majority of biologists. Secondly, there arises the inevitable problems when theories are transferred from one discipline to another; that is, they do not always translate well, and thirdly, I am not sure the species concept is in any way applicable to con-specific human groups. I am not advocating species selection, but believe it can provide useful insights into cultural change. In Stanley's view, (1979:191) species selection is natural selection among reproductive isolates, species, and therefore among different adaptations. It is this aspect that I want to emphasize. The source of ultimate variation remains the individual, but the level of major evolutionary change is the species. (Stanley carefully distinguishes species selection from Wynne-Edwards' (1962) now moribund concept of group selection.) Species selection is selection among sets of adaptive strategies. The concept also recognizes that many traits may be neutral, and these become irrelevant to natural selection, and that many of the processes of speciation are random and unpredictable. I am suggesting here that variations, new strategies, arise from individual human actors, but ther success depends upon natural selection among constellations of strategies. Species evolution proceeds by the processes of speciation and extinction. We may say that cultural evolution proceeds by innovation and failure.

I cannot pursue the issue here but the general notion of species selection may obviate the apparent problem of individual selection versus group selection ("the altruism problem") (c.f. Durham 1976) with regards to human groups. Dunnell (1980:66) offers a similar suggestion, though he restricts it to complex societies. He suggests selection shifts from individuals to groups as human groups become large. I suggest it always works on groups (though not exclusively).

To summarize at this point, I have argued (1) that the internal analytical variables are adaptive strategies, (2) that these adaptive strageties are not limited to subsistence or economics, but include the relationships of production and (3) that change is the result of natural selection among discrete constellations of these solutions. To say that change is "the result of natural selection" does not explain anything. The next section discusses that issue.

DYNAMICS OF CHANGE

According to Alland, there are three levels of selection:

> The first selection will occur by virtue of the system itself. Emitted traits are limited by the characteristics of the system, by its genetic and cultural elements. The second selection will occur as emitted traits are accepted or rejected on the basis of systemic properties and will depend on how well a trait fits a particular structure. The third selection will occur as a result of interaction between the system and the environment.
>
> (Alland 1975:69)

Variations do not arise from whole cloth, they arise from the particular potential of the system. Oysters do not suddenly begin to talk. Secondly, variations must fit what is already there, the variables and relationships already existing, and then thirdly, natural selection as it is usually understood operates.

However, it is implicit in this view that adaptive systems are well integrated wholes. They are not: they are compromises, and a trait will be positively selected if its positive benefits even marginally outweigh its negative effects. Different strategies may accomplish their ends, but have undesirable or conflicting side effects.

Alland cites Morris Goldman's notion that organic systems are both internally and externally adapted. "Internal adaptation represents the coherence ... of the system ... External adaptation

... the goodness of fit between the system and its environment" (1975:69).

Thus adaptation involves accommodation to intra-systemic and inter-system constraints. It is useful here to introduce Godelier's concepts of inter-systemic and intra-systemic contradictions, as discussed by Friedman.

> ... (intra-systemic) contraditions are within a structure ... between systematically self contradictory aspects of a social relation. Inter-systemic contradictions are those that exist *between* structures (emphasis Friedman's) ... This relationship is one of mutual constraint ... it is analogous to mutually limiting functions in systems or equations ... The *functions are autonomous, but the range of values which they can take is limited by the other functions*. (emphasis mine)
>
> (Friedman 1974:447).

In the terminology used here, intra-systemic contradictions are those within an adaptive strategy, or closely tied set of strategies. Thus the accumulation of radioactive waste is an internal contradiction within the strategy of atomic energy. Inter-systemic contradictions are between strategies which are functionally independent, but which constrain each other. These are both internal contradictions, within a single adaptive system. They might be better termed intra-subsystemic and inter-subsystemic, but those terms are overly cumbersome. I do not wish to introduce or belabor jargon, but to emphasize the importance of Goldman's internal adaptations, and that the internal order of a system is a function both of internal order and the external order. That is to say, the internal order depends upon the internal coherence of variables, the relationships among variables and the selective pressures on variables. Changes proceed, in my view, from either internal inconsistencies, or changes in external selective pressure. As Winterhalter (1980) points out, adaptations display time lag. Different strategies within a single system will, since they are each solutions to different environmental problems, display differing degrees of lag. Thus inter-systemic contradictions are inevitable, and in extreme cases these internal contradictions require their own solutions. The relationship between the system and its environment, and the system's internal relationships are seen here as semi-autonomous sources of variation and change. Cultural selection (the cultural equivalent of species selection) should operate then on both internal and external adaptation. At this point, the discussion will return to a consideration of stable and resilient systems and then conclude with a consideration of social hierarchies among foragers.

STABLE AND RESILIENT SYSTEMS

In resilient systems, a variety of strategies may be employed to meet external adaptive problems without generating major internal contradictions. The strategies themselves may contain contradictions (intra-systemic contradictions), but there should be few contradictions between strategies (inter-systemic contradiction). When problems arise, solutions should be simple and straightforward. Internal disputes may be resolved by someone moving to another group, for example. Because of the lack of internal contradictions, these systems are capable of persisting for long periods. However, they are not in internal equilibria. Resilient systems persist through their internal fluidity and their capacity to assimilate environmental extremes. Thus these systems accommodate both external and internal hetereogeneity.

Stable systems cannot accommodate external hetereogeneity. They are complex systems which adjust to external variation by maintaining an internal equilibrium. If equilibrium is not maintained, then contradictions inherent in the system will be exacerbated, and this should result in the development of greater internal complexity to solve those problems. These systems, unless the environment is stable, may not persist for long periods because of their difficulty in maintaining internal coherence.

It follows from this that selection in unstable conditions should favor increasing resilience at the cost of less internal complexity, or increasing stability (with greater internal complexity), at the cost of persistence. In the earlier paper I specified five kinds of change (Ames 1981): 1) a resilient system adapting; 2) a stable system adapting; 3) a resilient system becoming, or being replaced by, a stable system; 4) *vice versa*; and, 5) systemic extinction. The key question becomes, under what conditions will selection favor increasing simplicity and under what conditions increasing complexity. According to Stanley's model of species selection, new variants (species) will arise more or less at random, and unpredictably, i.e., one cannot know where or when speciation will occur. Speciation events are, in his view, experiments, some of which fail, some which succeed. These experiments must be within permissible bounds (Alland's first level of selection). Because of this conservatism, and long term selective pressure (directional selection, [Grant 1963:237]) trends may result, but these are not inherent, or progressive trends. In the same way, we may expect cultural experiments with either increasing complexity or simplicity, and trends resulting from cultural conservatism and directional selection. Whether these trends are transformational or replacive is not critical to my argument.

If selection favors either complexity or simplicity, can we specify events in which both have happened? Can we predict under what conditions complexity will be favored and when simplicity will be favored? The answer to both questions is yes. The prehistory of portions of central California is an example of a historical sequence where shifts in conditions have favored first complexity, then simplicity and then again complexity (Moratto, King and Woolfender 1978). The changes of house patterns reported by Fitzhugh (1980) may reflect alterating selection for internal simplicity and complexity. The second question is discussed in the next section.

SOCIAL HIERARCHIES AMONG FORAGERS

I would argue that social hierarchies will arise wherever and whenever internal or external circumstances increase internal contradictions or internal flexibility is not possible, i.e., whenever circumstances constrain the capacity of the system to adapt internally. At this juncture a system can no longer adjust to external conditions by altering internal values and therefore strategies which maintain internal values in the face of external changes will be positively selected. Ranking is such a strategy.

Demographic change, whether growth or changes in dispersion, new technology, sedentism where it has not previously been practical, subsistence intensification or specialization are all factors which can produce contradictions. Increasing conflict, either internal (e.g., Tilly 1981) or external (e.g., Carniero 1981) are symptomatic of the contradictions and exacerbate them. None of these are new suggestions. However, their effects and the contradictions they can cause may not be generally anticipated. McGovern's (1980) fascinating study of Greenland Norse is an excellent example of inter-systemic contradictions limiting a society's capacity to respond, and ultimately causing the society's physical extinction. Jochim's (1979b) simulation of mesolithic German subsistence patterns shows that technical innovations can require additional strategies to deal with problems caused by the initial innovation. We, of course, are surrounded by examples of this in our daily lives; the automobile is one excellent case. Conversely some of these may not always be sufficient to cause change. Thus sedentism may produce no significant social changes (e.g., Fagan 1978). Matson (this volume) may be correct in arguing that a combination of sedentism and resource intensification will produce complex societies. However, on the Columbia Plateau, there appears to be both partial sedentism and resource intensification, with, at present, no important evidence of social complexity similar to that of Northwest Coast societies. There are probably critical thresholds below which contradictions are not serious, above which change occurs.

I do not think that the west coast or east coast (Miller this volume) or coastal societies in general are special cases requiring special theories (e.g., Yesner 1980a). Rather, I think that Kehoe (1981) will be borne out and we will discover that aboriginal social organization was everywhere more complex than we now realize. Ranked societies were unstable and areas will have been like the Sacramento Valley, with temporal shifts between ranking and egalitarian systems.