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CONTENTS

- DOUGLAS R. WHITE / Process, Statistics and Anthropological Theory: An Appreciation of Harold E. Driver 295
Jorgensen, ed., *Comparative Studies by Harold E. Driver and Essays in his Honor*
- BIOBEHAVIORAL PERSPECTIVES: A SYMPOSIUM 314
- RALPH L. HOLLOWAY / On the Brain and Human Status 314
Count, *Being and Becoming Human: Essays on the Biogram*
Jerison, *Evolution of the Brain and Intelligence*
- GEZA TELEKI / Primate Aggression, Territoriality, and Xenophobia: Current Research Trends 322
Holloway, ed., *Primate Aggression, Territoriality, and Xenophobia: A Comparative Perspective*
- CLAIRE MONOD CASSIDY / Anthropocentrism and Androcentrism in Biology 329
Hinde, *Biological Bases of Human Social Behavior*
- PATTY JO WATSON / Archaeology as Diachronic Anthropology 337
Plog, *The Study of Prehistoric Culture Change*
- EVERETT M. ROGERS / The Anthropology of Modernization and the Modernization of Anthropology 345
Poggie and Lynch, eds., *Rethinking Modernization*
- PETER M. GARDNER / Ecological Idealism and the Peasant Village 358
Beals, *Village Life in South India: Cultural Design and Environmental Variation*

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PROCESS, STATISTICS AND
ANTHROPOLOGICAL THEORY:
AN APPRECIATION OF HAROLD E. DRIVER

Douglas R. White

Joseph G. Jorgensen, ed. *Comparative Studies by Harold E. Driver and Essays in his Honor*. New Haven: HRAF Press, 1974. 248 pp. Diagrams, tables, figures, maps, appendix, and bibliography. \$20.00.

This collection of new and reprinted articles edited by Joseph Jorgensen emphasizes the contributions of Harold E. Driver to the integration of culture theory in American anthropology. Harold Driver shares with other leading figures of postwar American anthropology, a concern for socio-cultural processes, cross-cultural regularities, and methods for testing the validity of anthropological theories. Driver has been a major innovator in the development of statistical models and quantitative procedures for testing theories. His control over statistical theory discouraged its missapplication in various aspects of comparative research. This volume brings together many of his papers on processual theories in cultural anthropology and contains many excellent examples of his careful and creative use of statistics.

Driver's contributions to North American ethnography and ethnology began with his studies of California Indians and culminated in the monumental *Comparative Studies of North American Indians* (Driver and Massey 1957) and *Indians of North America* (1969). While it is primarily his comparative work that is presented in this volume, one is struck by the depth and range of his contributions to anthropological research. His intensive immersion in a major ethnographic area forms part of a complex research strategy, utilizing formal comparative research to test, supplement, and enrich more traditional approaches to regional data. Within

this research model, cross-cultural studies with worldwide samples constitute one of several levels for the analysis of sociocultural process.

Driver has used a broad range of approaches to data in order to compensate for the limitations of any single approach. He has shown a variety of ways to use statistics as an objective means of measuring relationships and difficulties in the interpretation of statistical results. His essential concerns have been to rethink formal comparative research as well as to solve specific substantive problems. He has contributed simultaneously to theory and method by synthesizing an integrative theoretical framework of processual analysis, which includes functional adjustment, evolutionary change, and regional interaction. In showing how statistical analysis can contribute to the unraveling and identification of these processes, Driver has made a number of major contributions to comparative methods. In his writings, however, he has consistently recognized that methods do not stand alone, but are useful only insofar as they contribute to theory and explanation, and that no method in and of itself assures valid results. Thus, for example, since the appearance at the earliest comparative work of Murdock (1937), Driver has been both a critic of and contributor to cross-cultural methodology.

In assessing Driver's contributions to this volume and to comparative research, I will consider the substantive problems he has investigated, then turn to methodological problems, and conclude with general perspectives deriving from Driver's work.

Because of the scope of Driver's writings, this essay bears on issues that are central to anthropological theory and method. Much of the lack of integration in contemporary anthropology arises from divisions of opinion over the rather premature assumptions in the early development of comparative method. Driver is one of the few anthropologists of his generation who has kept to the fundamentals of comparison, treatment of evidence, and validity of inference which stem from issues raised by Tylor, Boas, Kroeber, Murdock and others. He has done so by accumulating an extensive body of data which is basically regional in scope (i.e., North America) but which serves as an empirical proving ground for solving larger issues. These issues will be discussed here in terms of six major methodological problems which are relevant to sociocultural anthropology. Driver attempted solutions within a holistic framework which led him to a reunification of historical, evolutionary, functional, and structure-functional perspectives.

Problems of Substantive Interpretation

The most recent of Driver's substantive or theoretical articles concerns kinship behavior. "Geographical-Historical *versus* Psycho-Functional Explanations of Kin Avoidances" (1966; in part two of this volume) is the

best example of the strengths of Driver's methodology. Driver examines three theories of avoidance:

1. Tylor's proximity theory, e.g., that matrilineal residence puts mother-in-law and son-in-law in stressful proximity, and thus helps to create the conditions of mother-in-law avoidance.
2. Frazer and Freud's theory of avoidance as an outgrowth of incest taboos or the extension of rules of exogamy and sexual prohibitions, with same-sex avoidances arising by extension from cross-sex avoidance (also, Murdock 1949; Stephans and D'Andrade 1962; but see Sweetser 1966 for a different interpretation).
3. Lowie's theory that while avoidance and joking are more frequent between relatives of opposite sex, there may be several different casual sequences leading to avoidance or joking—sometimes cross-sex prohibitions, sometimes proximity, and sometimes convergent development from diffusion.

Driver's results, in the study of avoidances, are strikingly Boasian in the sense of finding multiple causes for similar cultural patterns. Unlike Boas, however, Driver does not regard multiple causality as making comparison invalid. To the contrary, Driver shows that by analysis of data within a regional context it is possible to discover potentially different origins of the same institution.

For North America, Driver finds that there are four widely separated language groups (Penutians, Siouans, Muskogean, and Haida-Tlingit) where avoidances are predominant. Taking shared traits within language boundaries as indicators of ancestral proto-cultures, Driver creates an argument that these are the likely ancestral "donor" cultures (i.e., cases of independent invention) for avoidance patterns in North America, and that many other cases of avoidances may have diffused from these centers. This inference is supported by distributional data. These areas are the only ones in North America in which Crow-Omaha systems predominate, Driver's mode of inference points to an ancestral connection between avoidances and Crow-Omaha systems, which involve the extension of marriage prohibitions. Such a connection would support theory [2], but the total body of data cannot be explained in any such simple fashion.

After removing all of the probable "donor" groups from his sample, Driver finds that the strongest positive correlations with avoidance for his sample of "recipient" societies (i.e., where avoidances were presumably acquired by diffusion) are not with Crow-Omaha terms, but with matrilineal residence. This would seem to support theory [1], and by implication, Lowie's theory [3] of multiple causality, were it not for the anomalous correlation, in "recipient" societies, of a positive correlation between mother/daughter-in-law avoidance and matrilineal residence (contra the proximity hypothesis).

Driver's mapping and distributional analysis of the data is very useful to other researchers who wish to offer alternative explanations. Most of the Plains and southwestern Apache region, for example, is classified by Driver as a "recipient" region where avoidances have spread by intermarriage. The fact that avoidances are nearly universal in this area (except for the Comanche and Pawnee) might suggest an ecological hypothesis.

The high incidence of avoidances in the Plains-southwest Apache area may reflect a very high importance attached to the labor value of wives in an environment where fuller exploitation of large game was facilitated through introduction of the horse. With the presence of sororate and wife inheritance claims on affines as well as extensive polygyny, interdependence and stress between affines is potentially high. Bilateral exogamy is extended, for most of the societies in this area, to second cousins or beyond, further augmenting the potential for stress between affines. Affinal avoidances might be viewed as a consequence of conditions which generate both interdependence and stress between affines, as suggested by Eggan (1937). Eggan noted that avoidances were one of the common cultural responses in the new ecology of horse nomadism in the Plains area.

Given the potentialities of Driver's continuous area comparative methodology, it is unfortunate that he does not consider the theories of kinship behavior of the social anthropologists (e.g., Radcliffe-Brown or Eggan). Apparently this is because Driver (1966: 157) feels that stress or tension between relatives could not be measured. Like many American ethnologists (but see Sweetser 1966), he is unwilling to deal with social structure as an interlocking network of relationships.

The rift between the American and British points of view on social structure is apparent in Driver's avoidance paper and the responses to it in *Current Anthropology*. The cleavage is between a stochastic functional model of culture lag vs. a structure-functional model of social relationships. Driver tends to see kinship avoidances as best explained functionally within a general theory of cultural lag in a chain of cause-and-effect between division of labor (or other aspects of economy), residence, descent and kin terminology. In Driver's view, kinship behavior comes last, or next to last in this chain before changes in terminology. In his classic (1956) article on "An Integration of Functional, Evolutionary, and Historical Theory by Means of Correlations" (not reprinted in this volume) Driver assessed Murdock's culture lag theory of sequences of development of forms of social organization, again using continuous area data from North America. The stochastic process model of culture lag was tested by means of correlations, showing that the highest correlations between items in a sequence such as

(patricentered) division of labor → residence → land tenure →
descent → kin terms

were between adjacent variables, with the strength of correlations diminishing the further apart the variables in the sequence. A stochastic functional model of this sort fits easily with a diffusion model in that deviations from the "main sequence" of development can be explained by reference to adoption "out of sequence" from neighboring societies. In his recent (1966) article, Driver shows that mother/son-in-law avoidances fit the matricentered pattern of correlations as the last item in sequence, and that father/daughter-in-law avoidances fit the patricentered pattern of correlations next to last in sequence, with exceptional cases explained by diffusion. This culture lag theory thus assumes two different developmental processes (internal or primary development vs. diffusion or secondary development) from synchronic correlations and distributional evidence. This theory tends to support the view that kinship behavior, at least for avoidances, shows the greatest culture lag, or is the slowest to change of any of the main social organization variables.

The British structure-functional model of kinship behavior, on the other hand, tends to dismiss culture lag, and emphasize the immediate structural interlock between social roles. Change in one role behavior is expected to affect other role behaviors almost immediately, since the process of role interlock is behaviorally interactive.

There is no reason why Driver's methodology could not be applied to stochastic vs. structural functional models of social relationships, as well as functional vs. diffusion hypotheses: what is really at issue is the question of how long it takes for one factor to affect another in an adaptive process, and it is surely of major importance to determine in what respects behavioral variables show very short-term internal adjustments, or long-term adjustments to external variables. The argument between the British and Americans over comparison of a few social structural models with many variables but only a few cases *versus* the correlational analysis of many cases focusing on only a few variables (see Leach 1951) is ripe for synthesis. If it is the weakness of the British that they shy away from correlations and large comparative samples (perhaps because structural comparisons seem to involve too many variables to handle on a large scale and statistical correlations seem to carry the baggage of culture-lag theory), it is also a weakness of Driver's and the cross-culturalists' approaches that they fail to contextualize their comparisons of a few variables by coding many closely related variables and analyzing them, e.g., for structural relations.

Lowie's plea for "careful study of all avoidances and joking relationships between all relatives in a single society or continuous area in order to grasp the entire context within which each functions" is as relevant today as in 1920, and represents a possible bridge between the American and British comparative traditions. Driver, while he has greatly expanded comparative methods in the direction of examining regional context, has



not gone far enough in examining the social context in terms of networks of relationships.

A fuller treatment of Driver's theoretical interests and career development is given in this book by editor Joseph Jorgensen (part one), including a complete bibliography of Harold E. Driver. Driver's reviews of his own work may be found in several of his recent publications (Driver 1965, 1970, 1973a, 1973b). It is to the strengths of his continuous area approach that we turn, in assessing his contributions to comparative research.

Strengths of the Continuous Area Approach

There are six basic methodological problems where Driver makes major contributions by use of the continuous area approach:

1. Restoring ethnographic context to comparison
2. Treatment of Rival Hypotheses
3. Validity of Interpretations from Statistical Correlations
4. Causal and Processual Inferences
5. Replication of Findings
6. Discreteness of Ethnic Units

Each of these are of major importance to comparative method, whatever the theoretical goals or approach. I will not attempt here to compare the continuous area vs. controlled comparison vs. cross-cultural sampling approaches, but the continuous area approach has many strengths in terms of research design (see Campbell and Stanley 1963) that other comparative methods lack. Of Driver's numerous contributions to comparative methodology, some of which have been generalized and incorporated into cross-cultural sampling methods (see Naroll et al. in this volume). Here I will merely indicate some of the strengths of the continuous area approach.

1. One strength of the continuous area approach is that it can be used as a means of restoring the ethnographic context to the variables extracted for analysis. This context includes the kinds of variables considered, the historical context, the interactional context of relationships between societies, and the analytical context of characteristics of an entire region which constitute a single system.

Kinds of variables. In a continuous area comparative study, the researcher is able to utilize regional ethnographic knowledge to a much greater extent than is possible in a world scale comparison. Thus, Driver, in becoming familiar with virtually all of the ethnography on traditional North American Indian societies, is able to bring to bear a more holistic understanding of patterns and varia-

tions within this region in considering a particular theoretical problem, and thus identify what may be crucial ethnographic variables.

Historical context. Driver has shown how processual regularities can be inferred from the evidence of synchronic distributions, where historical time-depth is lacking. Cases of independent invention vs. diffusion can thus be isolated, and hypotheses can be tested about parallel vs. multiple causes for each type of process. Driver's goal in the treatment of inferences about social or historical process has not been to reconstruct history *per se*, but rather to avoid the errors of inference that result if one interprets cross-cultural correlations as evidence of direct functional associations. This will be explored below under "Galton's Problem."

Interactional context between societies. Diffusion, trade, warfare and migration are examples of some of the interactional processes at the intersocietal level that can best be studied by continuous area comparison. In making causal inferences from synchronic data, it is obviously essential to be able to identify the effects of diffusion in order not to mistake the overlapping diffusion of two or more traits as evidence of functional association. Again, this is an aspect of "Galton's Problem" described below.

Analytical context of regional systems. In many cases it is easier to find functional or evolutionary regularities within major regions that can be defined in systemic terms, e.g., the old world cattle-complex area, North American tribal area, etc. It is obviously useful to be able to specify the common characteristics of a macro-region in terms of systemic variables (e.g., population density, limits on available technology or food production, etc.) and study the relationship of other variables within these constraints.

2. Another strength of the continuous area approach is that it provides an explicit means of testing rival hypotheses when it comes to making causal or processual inferences from synchronic data. There are four general aspects of this problem: the validity of making functional inferences from synchronic data; that of making historical inferences from synchronic data; that of making causal inferences from historical reconstructions; and that of distinguishing different kinds of processes from synchronic data.

Galton's Problem: *Functional inferences from synchronic data.* Sir Francis Galton first raised the question of the validity of causal-functional or evolutionary inferences from correlational data when Tylor (1889) read his paper on the development of institutions, and put forward his hypotheses, among others, about the relationship between matrilocality and mother-in-law avoidance. As evidence of a functional-evolutionary relationship, one would ideally want to identify as many independent cases of the

development of matrilocality as possible, and then to test to see in how many cases mother-in-law avoidance developed subsequently, as opposed to cases of avoidance occurring without matrilocality. Where one or both traits spread by diffusion, however, it becomes problematical to tell whether a correlation between the variables resulted from functional adaptation (parallel evolution), co-diffusion of functionally linked variables, co-diffusion due to functional linkage with still other factors, or accidental overlap in the areas of diffusion of the two traits.

Historical inferences from synchronic data. Boas referred to the difficulties of functional inference from synchronic data in 1896 in discussing the limitations of the comparative method, and again in 1927:

The fundamental difficulty of this method is our lack of knowledge of historical connection. In order to make a statistical method a success it is essential that the phenomena counted be independent of one another. If a number of them go back to the same historical sources they cannot be counted as separate units (120).

Boas, however, erred in the opposite direction to Tylor in inferring that trait similarities within a limited culture area, such as the northwest coast (Boas 1894, 1916) could be taken unequivocally as evidence of common origin or diffusion of these traits. Kluckhohn (1939: 36) was first to note that while Boas "impaled the attempt to verify sociological generalizations on this dilemma," Galton's objection applies equally forcefully to the use of comparison, whether statistical or not to reconstruct historical relationships. While Boas and Kroeber treated all regional similarities as due to historical factors, Steward showed that ecological and sociological factors could not validly be dismissed (1937: 102). Driver (1939) similarly criticizes an earlier article (Driver and Kroeber 1932; in this volume) in which Kroeber maintained that history could be reconstructed from statistical analysis of regional similarities.

Causal inferences from historical reconstructions. One of the fallacies of Boas' scientific program was the assumption that since historical reconstructions could be made validly from synchronic evidence (shown to be false by Driver, Kluckhohn and Steward, above), historical reconstructions could thus be used as evidence to develop or test causal, functional, or evolutionary theories. A similar view is evident in Aberle's paper in this volume entitled "Historical Reconstruction and Its Explanatory Role in Comparative Ethnology." The view taken by Aberle is antithetical to Driver's approach, as Driver has several times disclaimed that specific historical reconstructions can be made from distributional

data (see p. 105). Aberle comes to the unwarranted conclusion that matrilineality was the likely descent rule for the protocultures of eight of the nine major American Indian language families. The evidence is simply that matrilineal descent occurs on the fringes of continuous regions of bilateral or patrilineal descent for these eight families. Inferring time depth from an age-area model of the invention and diffusion of descent rules, matrilineality would be the oldest descent rule. This conclusion lacks validity in that it is not shown how rival historical reconstructions can be eliminated, or why one should accept the age-area hypothesis.

Distinguishing different kinds of processes from synchronic data. Driver, recognizing the problems inherent in the approaches of the early cross-culturalists and the historicists, makes a major contribution to the solution of Galton's problem in using the evidence of continuous area comparisons (where historical data is lacking) to construct two complementary kinds of models. One model is of primary development, that is, of independent invention or causal-functional sequences assuming no change in the external environment or outside acculturative pressures; the other model is of secondary development, that is, of diffusion, migration and acculturative pressures by which one society is acted upon by others in its regional environment.

The evolutionists have largely accepted the distinction between primary and secondary development, although Driver addresses a new article on "Diffusion and Evolution" (in part two) to a few recalcitrants who claim that diffusion is "capricious" while evolutionary development follows lawlike principles. In showing that there are regularities in diffusion (secondary development) as well as in independent invention (primary development), Driver's strategy of joining together models of complementary development processes is obviously a more encompassing approach to the study of evolution.

Cross-culturalists, however, have generally not accepted the importance of the distinction between primary and secondary development, and still appear to be content with correlations as evidence of uniform causality, in spite of numerous objections about the validity of causal inferences from correlational data (see Campbell and Stanley 1963).

A third strength of the continuous area approach is that it allows use of statistical correlations in an optimal way for identifying different kinds of patterns (e.g., culture area vs. culture type) and processes (e.g., regional vs. functional) with a minimum of *a priori* assumptions. There are several aspects to the use of statistics here: the assumption of independence of cases; the double-clustering

technique; extension of statistical controls to cross-cultural samples; and the use of statistical classification.

Independence of cases. Driver correctly perceives that Galton's objection is not against the use of statistics for comparative research, but directed toward the kind of inference made from correlations.

It is commonly believed that statistical inference is invalid unless the historical independence of the ethnic units can be established. This is nonsense. The independence of cases in statistics refers only to the drawing of the sample. . . . Historical independence is necessary only if the researcher is trying to establish a causal-evolutionary sequence. This is exactly what Tylor was trying to do and is the main goal of cross-cultural research today, but it is not the only one (Driver 1973a: 338).

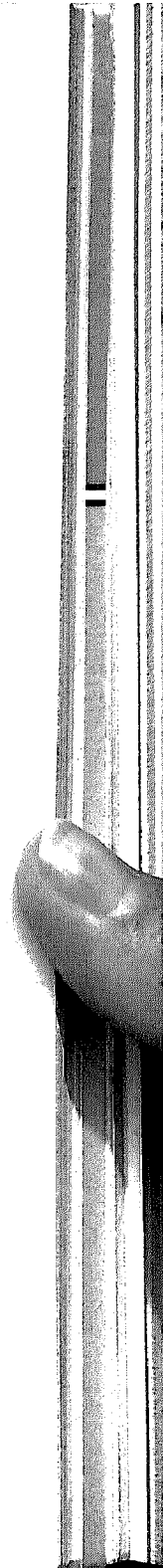
After reading the first cross-cultural studies out of Yale (Murdock 1937; Simmons 1937), Driver realized that their approach was causal-evolutionary and depended on establishing independence of cases, but that their method of taking sampling units that are not closely related historically was inadequate. (see Jorgensen p. 196).

Double-clustering method. The method which Driver pioneered (Driver and Kroeber 1932, in part two of this volume; Driver 1939 and 1941 in parts three and four) to steer a middle course between the extreme evolutionary functionalism of the Yale School and the extreme historicism of Boas and Kroeber, was the use of coefficients to measure both the correlations between variables across societies (R-mode analysis) and the similarities in cultural inventories between societies (Q-mode analysis) within a continuous area. The method was first used by European diffusionists (Klimek, Milke, Fürer-Haimendorf) in response to Driver's use of Q-mode analysis (Driver and Kroeber 1932), but Driver was the first to use this method to evaluate functional vs. historical hypotheses simultaneously. In 1939 Driver (see part three of this volume) renounced Kroeber's view that history could be reconstructed from statistical correlations, and analyzed how various types of coefficients reflected different types of trait distributions. His classic 1941 study (partly reprinted in part four) on girls' puberty rites in western North America used the double clustering method in an attempt to determine whether similarities resulted from universals, cultural heritages spread by migration, relayed diffusions, or functional-evolutionary convergences. Driver showed how these processes could be distinguished by careful inference from synchronic or distributional data. These studies laid the foundations for the later theoretical works (Driver 1956, 1966) discussed above, and also gave a foundation for other kinds of solutions to Galton's problem in comparative research.

Extension of Galton's problem solutions to cross-cultural samples. Driver's use of the double-clustering method for continuous area samples was the first objective solution to Galton's problem. It remained only to generalize the double-clustering approach to non-continuous area samples. Stimulated by Driver's work (see pp. 141-42 this volume), Naroll (1961, 1964; Naroll and D'Andrade 1963) successfully generalized solutions to Galton's problem for use with cross-cultural samples. All of the methods for solving Galton's problem depend on either arranging the societies in a cross-cultural sample into a quasi-continuous order (diffusion arcs, etc.) or into groupings based on linguistic similarity or other measures of genetic heritage. There are various ways of measuring regional or genetic clustering and screening out the effects of such clustering on inter-trait correlations. These so-called solutions to Galton's problem (including Murdock and White 1969), however, are still only partial solutions in that they do not guarantee that valid functional inferences can be made from cross-cultural correlations where the effects of geographical or linguistically-related similarities have been removed. In some respects, continuous area studies still provide a better control over Galton's problem in that consideration of variability within a continuous region can lead to a more valid interpretation of process from synchronic data.

Driver and Chaney (1970, in part three of this book) show that D'Andrade's (Naroll and D'Andrade 1963) solution to Galton's problem does not necessarily measure "diffusion" vs. "functional" association, but, more properly, continuity of geographical distribution vs. non-contiguous association. Driver shows that his continuous area method (as in Driver 1966) is needed to establish diffusion as a factor, and notes that it is important to know whether a cultural behavior has very few or very many independent origins, as well as the relative time-scale of divergences between societal units which are ancestrally related. These kinds of inferences, as well as inferences based on continuity of distribution, can be most reliably done from a continuous area approach.

Use of statistical classification. Driver's work with continuous area studies shows that even such mundane problems as the classification of cultures on the basis of similarities requires processual considerations about the nature of cultural change. In a recent article reprinted in this book (pp. 205-25), Driver and associates produce a classification of cultural similarities in North America from Murdock's social organization data, in an effort to match, by an objective method, the more traditional culture-area classifications. The effort proved to be fruitless in that the clusters did not yield clear-cut regional or culture-area groupings. The authors concluded that the ethnographic atlas (Murdock 1967) data, for this



purpose, is "deficient in the quantity of material culture and technology which reflect geographical and historical relationships more strongly" than social organization. In a new article in this book, Driver and Coffin (pp. 225-28) take the Driver-Massey (1957) data which is about three-fourths material culture and technology and use Q-mode analysis to generate a statistical classification of North American Indian ethnic units. The results are extremely close in their match to the intuitively developed culture-area classifications of Driver (1969), Murdock (1966), Spencer and Jennings (1965), Kroeber (1939), and the North American Handbook. Thus, they conclude that "a sample heavily loaded with material culture and technology produces a more conventional scheme of culture areas than one loaded heavily with non-material culture." Early attempts at culture area classifications thus reflect a museum-based emphasis on material culture as well as the general principle that technological items tend to diffuse more continuously than social organization traits.

4. A fourth major strength of continuous area studies is that causal and processual inferences can be made from the evidence of continuous regional distributions combined with trait correlations. There are three levels at which specification of process is involved in making causal inferences from correlational data. (a) What is the process which is generating the variability in each of the two variables considered in a correlation? For culture traits, these generating processes may include independent invention (parallel evolution), ancestral retention and spread by migration (functional maintenance), or spread by diffusion (borrowing). (b) What is the process which is mediating in the relationship between the two variables (X and Y)? These may include direct causality (e.g., $X \rightarrow Y$), contingent causality (e.g., if A : $X \rightarrow Y$), indirect mediated causality (e.g., $X \rightarrow A \rightarrow Y$), third factor causality (e.g., $A \rightarrow X$ and Y), accidental correlation arising through a random process, or accidental correlation arising through sampling error. (c) What is the temporal direction and time lag in causality? This may range from relatively instantaneous processes such as social interaction, to inter-generational processes (Murdock 1948; Driver 1956; Barth 1965), to long-term trends, cycles, or sequences. A fourth aspect of causal inference from synchronic data is essentially structural: (d) What is the nature of the relationship between two variables at one point in time? This may range from a necessary or sufficient relationship (asymmetrical dependency) to a biconditional relationship (symmetrical dependency), and approximations thereof, or to linear, curvilinear, etc., types of relationships. Driver has been concerned with each of these four problems of causal inference from

synchronic data. The advantages of continuous area studies at each level are discussed below.

Inferences about mediating processes. Driver has been very much concerned with specific causal relationships between variables (see especially Driver 1956), without assuming that correlation always equals functional association. Examination of a trait inventory similarities other than those of the primary variables can suggest whether other explanatory (third) factors, mediating factors, contingency factors, or direct causality is involved in the relationship between the primary variables; distributional data are of use in estimation of the likelihood that a correlation is due to historical accident (e.g., overlapping areas of diffusion) or to sampling error.

Inferences about direction of causality and time-lag. Driver's work on diffusion shows clearly his preference for direct temporal-sequence evidence from examination of changes in individual cases or of distributions over time (e.g., historical, ethnohistorical, acculturational, longitudinal, or social change studies). Where direct evidence is not available about the sequence of change, continuous area studies can yield valuable clues, particularly when attention is given to the specific form of a relationship between two variables (see section following). Driver's stochastic model of "main sequence" changes in kinship organization is a good example of his concern with the direction of causality. In a single sequence model, an item with a narrower regional distribution (e.g., patrilineal descent) can hardly be the cause of an item with a broader regional distribution (e.g., patrilocal residence), thus the sequence patrilocal residence → patrilineal descent → Omaha cousin terms is also an ordering from the most frequent to the least frequent of the traits considered. Where the possible models of causality between a set of variables can be sufficiently limited logically, Driver (following Blalock, 1960) has shown path analysis or partial correlations can be used to choose between the remaining models.

Inferences about the nature of relationships at one point in time. Driver's approach to mapping continuous trait distribution allows for visual inspection of distributional covariation in addition to correlational and clustering analysis. If the distribution of one trait is a subset of another, this can be discovered through inspection of distributional maps, or through the use of correlations (such as Q) that are sensitive to zero cells. Driver has paid a good deal of attention to these kinds of discrete-trait relationships (but relatively little to continuous trait relationships), to set/subset and Guttman scale structures in cultural data. But he also warns (see also Graves et al. 1969) that processual inferences about sequences of development from Guttman scales or set/subset relationships are not neces-

sarily valid—items which develop later in time may come to have broader distributions than older traits because a newer trait may diffuse more widely. Here, continuous area studies of diffusion are again an important adjunct to methods which simply examine the relationship between traits at one point in time.

5. A fifth strength of continuous area comparisons stems from the need to replicate the results of any cross-cultural survey by means of extensive studies within limited regions or contexts.

Continent-wide comparisons. Continent-wide comparisons usually contain sufficient heterogeneity for testing cross-cultural hypotheses. Driver (1973a: 353) and Murdock (1940) are agreed any valid cross-cultural hypothesis should hold true for any area of the world in which there is sufficient variability to put the hypothesis to a test. It has been shown, for example (Anonymous 1965: 393-94) that of the correlations in Murdock's *Social Structure* (1949), none holds across all of the major continental areas even where there is variability within each area. Romney (op. cit.) concluded that "Murdock's theoretical results as to functional relations can now be shown to be partly an artifact of distribution over the face of the earth" (p. 393). The results of many cross-cultural studies have been seriously challenged by researchers inspired by Driver who have sought to test regional replication on the same methods of controlling for Galton's problem.

One of Driver's studies in this volume (in part four) correlated and factor-analyzed thirty variables from Murdock's (1957) "World Ethnographic Sample" and replicated this analysis within each major continental area. Only one correlation replicated on all six regions: that between compact villages or towns and the dominance of agriculture. Several correlations (e.g., patrilineal descent and patrilineal group exogamy) were replicated in all the areas where variability was present within the region. The problem with this study, however, is that by selecting a very heterogeneous group of traits, there were very few significant correlations to begin with. (Only 8 percent of the relationships were significant at the 5 percent level, which is not much more than would be expected by chance). The study does, however, show the potential of continent-wide replications for cross-cultural correlations.

Where the entire continent extends from the arctic to the equator and contains sharp differences in geography . . . the total range of cultural variation may approach that of the entire non-literate world. Under such conditions, cross-cultural regularities may appear in a number of clusters separated from each other. . . . Continuous distributions of continent-wide scope can distinguish culture heritages within language families from d

fusions across language family fences . . . and can employ partial correlations, multiple correlations, factor analyses, and other multivariate techniques that require sizable samples (Driver 1970: 636).

Continent-wide comparisons are thus critical to the replication of cross-cultural results, and may be even more useful than cross-cultural samples in identifying what processes generate trait association.

Systems-level boundaries for replication, or substitution of variables for proper nouns referring to regions. Driver (1973a: 354), following Przeworski and Teune (1970), recognizes that one of the goals of the comparative method is not to ignore regional differences or dismiss correlations that do not replicate in continent-wide comparisons, but to recognize the existence of regional variables which can be substituted for proper nouns. Rather than saying "this correlation holds for Africa and parts of the Old World" the aim is to find what systemic characteristics apply to the region (e.g., domestication of large animals) in which the correlations hold. Driver's proposal for seeking more valid correlations from comparative studies is consistent with this general strategy of specifying variables and systems:

When a more empirically determined set of world culture areas, ranging from area of continental size down to the smallest clusters, has been achieved, all interculture-trait correlations could be tested for areal differences and, when possible, new variables could be substituted for the proper names of the areas (1973a: 354).

6. To conclude this list of strengths of the continuous area approach with the units of comparison, the continuous area approach does not assume discrete cultural unit boundaries, nor does it regard the comparison of societies as analogous to the comparison of individuals, species, or organisms. The continuous area approach avoids falling into the trap of treating comparative studies like the butterfly collecting aptly described by Leach (1961), that of reifying ethnic units, or assuming that "cultures" are homogeneous rather than internally variable.

The ethnic units problem. A defect of the early cross-cultural method of computing correlations between variables as a basis for functional inferences is that heterogenous ethnic units are treated as if they were somehow "natural" units like individuals or species:

The classic objection to this method is that one is counting a mixed assemblage of cherries, olives, apples, oranges, grapefruits, watermelons, papayas, and pumpkins. A common answer to this criticism is that all the things counted are plant foods and comparable enough on this general

level. This cavalier attitude towards ethnic units does not satisfy the social scientist whose position is somewhere between the poles of extreme historical particularism and extreme scientific abstraction (Driver 1973a: 330-31).

British social anthropologists are particularly sensitive to this statistical razing-over of what may be critical differences in the nature of the ethnic units compared (in terms of size, scale, structure, etc. [cf. Leach 1961]). Naroll's (1964) attempt to define objective criteria for ethnic unit boundaries can hardly be regarded as a success, and Driver has pointed out that boundaries in terms of cultural inventories are generally not coterminous with boundary criteria for membership in social or political groups (see Driver 1973a). Driver is one of the few comparativists to pay explicit attention to this problem and pose some practical suggestions for solutions which are comparable with the more critical point of view.

A practical solution to the ethnic unit problem . . . is to accept the wide range of variation' that everyone knows exists in ethnic units and add variables describing the most obvious kinds of variation to every cross-cultural study. . . . [Through] factor analysis . . . every new variable would thereby be placed in a meaningful ethnic unit context. This would rule out bivariate comparisons in favor of multivariate analysis and raise standards of cross-cultural research enormously (1973a: 332).

This suggestion would move cross-cultural methodology closer to the kind of structural and context-sensitive comparison envisioned by the British, and closer to treating societal comparison as a comparison of social or cultural systems rather than on comparison of "natural" units. The suggestion is consistent with Driver's general research strategy of exploring to the fullest possible extent the range and contexts of variability with which one is dealing, and establish contextual controls for comparison. It is also an extension of Driver's earlier use of the double-cluster method for simultaneously examining similarities and differences between ethnic units as well as clusters of covariation between variables.

The continuous view of cultural variation. Another problem that Driver's methodology solves for us stems from the earlier ethnographic myth that each society is internally homogeneous in terms of shared cultural content. A wide range of recent studies of intracultural variability has shown that these simplistic assumptions about cultural sharing are not valid. Rather than viewing each "culture" as homogeneous and discrete, Driver views ethnographic data as observational "sampling points" showing both local and intergroup variability.

General Perspectives in Driver's Work

Solutions to the six major methodological problems we have discussed in Driver's work have only begun to have a major impact on the field. Cross-cultural researchers—especially Naroll and students of Naroll and Driver, but also Whiting, Murdock, and researchers in the Yale School tradition—have been very strongly influenced by Driver's contributions, particularly in terms of solutions to Galton's problem, testing of rival hypotheses, and replicative studies. Still, a number of the important innovations in Driver's work have not been assimilated: partial solutions to Galton's problem have been relied upon too heavily, to the exclusion of considerations of the kinds of processes and the ethnographic contexts or systemic relationships in which sociocultural phenomena are embedded. No one has yet utilized Driver's suggestions for the ethnic unit problem. Very few major areal differences have been related to systemic or ecological differences between major continental or subcontinental areas. Very few cross-cultural studies have dealt with the problems of time-scale in which adaptive processes take place. No one has attempted to synthesize Driver's and others' work on diffusion processes, integrating these with the theory of culture change and acculturation. Few cross-cultural researchers have been concerned with the evidence for independent invention as an aspect of culture change. Fewer yet are concerned with the validity of causal inferences as Driver has been.

Driver's general perspectives on the problems of explanation of social and cultural phenomena tend to be buried within his work, but might be very useful for future generations of cross-cultural researchers to adopt. A major characteristic of his work is that he does not take for granted the simplifying assumptions about culture and society that have long been popular in anthropology. He does not make the assumption that "a culture" comes neatly packaged with a natural skin or discrete boundary. He does not assume the existence of "a social structure" for each society which is unitary, self-maintaining, integrated, or teleologically-directed apart from concrete individuals and decision-making groups. He does not subscribe to a view of culture as "superorganic." He does not reify cultural elements as natural "atoms" of culture or rely on the assumption that the cultural trait or custom has any special ontological reality. Even Opler's (1959) attempt to distinguish subject units varying in scale from elements, components, assemblages, patterns, or themes, in Driver's view, is "insufficient to account for the indefinitely numerous hierarchies of classes within classes of cultural phenomena (Driver 1965: 329). In this regard, Driver has noted, even contemporary ethnoscience has failed to identify the "natural units" for comparison of subject matter. Driver is concerned, quite simply, with the measurement and explanation of variability in human society, and the problem is partly in establishing what kinds of

variabilities are significant in terms of patterned covariation, and then in explaining such variability in terms of human adaptation.

Driver's perspective on the interpretations of human variability suggest that American functionalism is in a rather sorry state because of the lack of a comparative perspective. In his puberty rites article he noted his fundamental critique of the kinds of interpretations that are often done from the perspective of a single culture:

From the viewpoint of a single tribe at a single point in its history, no doubt every element of a [ceremonial] rite is believed or felt to be an integral part of a unified whole. Any good informant, if pressed hard, would probably construct enough rationalizations to make any ceremony appear to have functional unity (164).

From a comparative continuous area (and time) perspective, however, "the elements which constitute a small spatial and temporal unit of culture are almost invariably found to have wider distributions and different associations in a broader spatial and temporal frame." Thus:

The broader the comparative universe in either space or time, the looser the integration of . . . parts of a single culture at a single point of history appear to be. . . . Natives have less comparative knowledge than ethnologists and consequently they would be expected to believe or feel the presence of a relatively small number of integrating impulses in their cultures which to them produce or maintain the harmonious whole" (154).

Only if the ethnologist "develops a state of mind in which he eliminates his comparative knowledge" (as when an ethnographer ignores the surrounding culture area or region of his field study) can he or she regard a folk logico-functional model, or interpretive model (albeit "thick description") to stand as a "complete description," or even less an explanation of the phenomena observed or recorded.

Driver is both immersed in the materials of ethnography and standing back from any particular ethnography to look at cultural variation in terms of culture area (regional, continental), and finally, a worldwide framework. At each level he could ask what kinds of processes would account for such variability and how could one validate explanations by testing rival hypotheses, combining those that are supported by the evidence into a general theoretical model that integrates historical and diffusion processes with functional and evolutionary ones. For his diligence in pursuing an old set of problems in anthropology, and his inventiveness in offering a much broader and more flexible set of research tools for comparative studies, we are indebted to him and to the editor of this book for making Driver's work more widely available.

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BIOBEHAVIORAL PERSPECTIVES: A SYMPOSIUM

ON THE BRAIN AND HUMAN STATUS

Ralph L. Holloway

Earl W. Count. *Being and Becoming Human: Essays on the Biogram*. New York: Van Nostrand Reinhold Co. Behavioral Science Series, 1973. xv + 349 pp. Tables, bibliography, and index. \$14.95 (cloth; \$6.95 (paper).

Harry J. Jerison. *Evolution of the Brain and Intelligence*. New York: Academic Press, 1973. xiv + 482 pp. Illustrations, figures, tables, index, and bibliography. \$25.00.

This review covers two recently issued books which should be read by anyone seriously interested in human evolution. Since I have reviewed Jerison's book in detail elsewhere (*Science*, December 6, 1974), this review will be devoted mainly to Count's "Being and Becoming Human." Both authors are, in a very major way, concerned with the evolution of the