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Authors

White, Douglas R
Brudner-White, Lilyan A

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THE MURDOCK LEGACY: THE ETHNOGRAPHIC ATLAS AND THE SEARCH FOR A METHOD

Douglas R. White and Lilyan A. Brudner-White*

There is no doubt that George Peter Murdock contributed to anthropology something novel, vital, and authentic. To explore the Murdock legacy is a valuable experience for anyone who is concerned with the history of the discipline, its core paradigm shifts, and its basic assumptions about the nature of fact. Among other things, Murdock challenged traditional culture theory as it had been handed down by the evolutionists and later sharply revised by Boas and Kroeber, attempting to free it further from its procrustean bed of nineteenth-century evolutionary determinism and accompanying mystifications. His contribution was to be a pioneer in forging some of the new intellectual linkages by which anthropology began to move toward becoming a more self-reflexive discipline, capable of questioning its own theory. Accordingly, the present paper examines several parts of that legacy.

Within anthropology, Murdock was for decades the preeminent spokesman of the empirical tradition of direct comparison of societies. His monumental task, along with others, was to create a complex scientific apparatus by which anthropology could eventually become both a comparative and a formal science, capable of testing and falsifying theory against a worldwide data base. He built more directly on the Cross-Cultural Survey that he and others initiated in 1937 as part of an integrated program of research by the Institute of Human Relations at Yale. That research unit compiled data on 150 societies. Murdock's most immediate antecedents in this context were Sumner and Keller (1927). Like Morgan, Tylor, and Spencer, who went before

**Douglas R. White is Professor of Comparative Culture, School of Social Sciences, University of California, Irvine. Lilyan A. Brudner-White is a former Research Associate, Department of Anthropology, University of California, Irvine.*

him, Murdock put his faith in the comparative method. But in reaction to these predecessors, he eschewed cultural evolutionary theory, and he consciously attempted to liberate the comparative method from any single grand theoretical scheme, so that the research system could test any potential range of theories.

While disagreeing with Boas, Kroeber, and others on many points, Murdock did take a position that generally viewed cultural differences as related to environment, location, and adaptive processes. In that sense, all were heirs to Darwin, but Murdock was highly skeptical of direct historical reconstruction in the absence of a body of outside data and materials. His most general theoretical tendency was closer to that of Bronislaw Malinowski: namely, a tendency to look directly into the adaptive process in its societal setting. In consequence, perhaps even out of his ambivalence toward history and historical inference, the cross-cultural samples were pinpointed in time and space and allowed for comparisons in a broad spectrum for both synchronic and diachronic analysis, depending on the specific theoretical framework applied. Ultimately, then, the evolving comparative system supported many lines of inquiry.

There are four parts to the intricate Murdock legacy, which includes dialogues with his students and his critics. First, there were his books, such as *Social Structure* (1949), *Our Primitive Contemporaries* (1934), *Africa* (1959), and his recent *Theories of Illness* (1980). Of these, *Social Structure* was clearly the most crucial for the growth of the discipline. While some of his formulations were primitive, his grasp of critical scientific and substantive issues was uncanny and remains so. This volume, considered by many to be among the most significant to the field, raised basic questions of how a scientific anthropology might be possible, including issues of axiomatic theory, of sampling, and of statistical tests, and cautions as to separating cross-cultural similarities due to diffusion from those due to independent invention and functional association (Galton's problem¹). His synthesis of middle-range theories and his explanations of social structure in his expository text—of kin terms, sexuality, polygyny and other forms of marriage, residence, descent, the role of warfare, and the division of labor—were unsurpassed at that time. Much of his book has stood the test of time, in that a surprising number of his specific propositions have been supported in subsequent research. But also, for reasons that go to the core of the discipline, many colleagues inevitably would not agree with his specific agendas, his methodology, or, consequently, with his conclusions.

At the turn of the century, as Harris (1968: 1) notes: "there arose in England, France, Germany and the United States schools of anthropology that rejected the scientific mandate." Resistance to Murdock's approach was inevitable. The reasons for this lay in the history of the discipline itself, the details of which obviously cannot be recapitulated

in a paper of this scope (see Harris 1968 for a further discussion). A main point, however, is that for decades anthropologists had been split and divided over the very idea and problematic of a method and framework for the comparison of societies. Some remained evolutionist, but most adopted antievolutionism and were avidly opposed to wide-ranging comparisons. For them, the mandate was to study societies as wholes and on their own terms. Under Boas, the dominant paradigm was historical particularism. Consequently, the ground for direct statistical comparison was only partially prepared.

Murdock joined with other illustrious members of his cohort, such as Eggan, Spier, and Lowie just before him, to create a forum for consideration of anthropology's comparative concerns. The broader context, however, was far more polarized. As Harris (1968: 2) noted: "In the United States the dominant school flatly asserted that there were no historical laws and that there could not be a science of history." Murdock's comparative system was radical for its time, in that it provided a way to test some of the assumptions of such positions.

Murdock's focus closely followed after Morgan. As Sahlins later observed (1976: 92), Murdock essentially operated in the "Morganian understanding of the relationship between practical circumstance, utilitarian action, and cultural order." And Lévi-Strauss (1963: 300) notes: "Morgan's genius at one and the same time founded social anthropology and kinship studies and brought to the fore the basic reason for attaching such importance to the latter: permanency, systematic character, continuity of changes."

Murdock's article on statistical comparisons of kinship terms (1947) focused on theories advanced by Rivers, Kroeber, Lowie, Sapir, and Radcliffe-Brown. He was concerned with attempting to make definitive decisions between alternative scientific theories in anthropology. For some, Murdock took the question of comparison several steps further than the field was ready to go at that time. For others, Murdock did not go far enough. Harris (1968: 627–627), for example, accepted Murdock's specific 1947 findings but drew radically different conclusions from his discovery of a finite number of major kinship systems. Criticizing Murdock's retrenchment from causal theory, he suggested that Murdock's findings "indicate the existence of extraordinarily powerful, selective, deterministic evolutionary forces."

Although a variety of different intellectual frameworks struggled and continue to struggle for hegemony, Murdock's efforts to codify data from ethnographic research in a worldwide context had an especially stimulating effect on the rise of middle-range theory. In many respects, *Social Structure* disrupted the idiographic interlude of anthropology, where, as Harris (1968: 6) states: "an image of anthropology as a new discipline contributed greatly to the élan of the pioneering field workers. The achievements of [ethnography] were stressed almost to the complete neglect of the historical conditions out of which the

discipline had arisen." In consequence, "a kind of pragmatism has resulted, in which it is held that a bit of research is to be judged only on its own merits—that is, for what it has accomplished" (Harris 1968: 6). Murdock in part returned the field to its earlier mandate of societal comparison. *Social Structure*, for example, is dedicated to a range of pioneering figures, both in anthropology and in the social sciences more generally: Kroeber, Linton, Lowie, Morgan, Radcliffe-Brown, Rivers, Dollard, Freud, Hull, and Keller. Murdock laid the foundations for testing cross-culturally the ideas of these and other major theorists, and he was more open than many others to appreciating and incorporating major figures like Freud in his thinking.

Most importantly, Murdock's volume served as a whetstone to sharpen controversy around the nature of the scientific mandate of anthropology. It served to organize a debate around topics most central to the field and in that manner to raise a variety of fundamental epistemological questions and issues. *Social Structure*, in this respect, had for decades a profoundly stimulating effect on further refinement and growth of the discipline.

Social Structure was to focus anthropological attention far more than the historical particularist school had done on the maintenance systems of societies; namely, on the family, the social organization and functionalist integration of communities, the division of labor, and the regulation of sex and reproduction. Although the book contained an appendix on historical reconstruction, Murdock's theories gave less primacy to the economic, technological, and historical factors than did other perspectives in anthropology. At the same time as his theories incorporated some of the concerns of earlier pioneers in the field, he added new dimensions of focus.

By the 1950s, major theorists were debating the problem of worldwide versus limited comparison (Radcliffe-Brown 1950; Evans-Pritchard 1951, 1963; Eggan 1954). In the extreme, those like Evans-Pritchard (1963) were doubtful that any kind of comparison could produce useful universal generalizations, while other critics questioned the representativeness of his data or disavowed typologizing as trivial (Leach 1950). But Murdock (1949: 272–283) was well aware, for example, that patterns of kinship behavior form a continuum (following Eggan 1937), even while he tried to break that continuum into pragmatically useful segments (Driver 1974: 29). Driver, Aberle, and others joined Murdock in exploring various aspects of synchronic inquiry, especially relations of borrowing, inheritance, and innovation in culture and processes of diffusion. Many anthropologists doing cross-cultural research confined themselves to the culture area concept that emphasized history and geographical contiguity. There was thus a constant interplay among studies of a variety of types that shed light on such relationships as those between linguistic kinship

categories, social structure, behavior, and processes of change (Driver 1974: 37). Questions of the usefulness of particular approaches were continuously subjected to empirical investigation.

As Goodenough notes (in this volume), Murdock was in the vanguard of those who laid the foundations for much of what followed in the comparative study of social structure. *Social Structure* and his 1960 article stimulated much research in nonunilineal or cognatic kinship. This research made more explicit the fundamental distinctions between mechanical and statistical models that had been raised by Murdock's approach, a distinction further clarified by Lévi-Strauss (1963) and in the work of other major theorists. Murdock's work also provided an explicit contrast for Lévi-Strauss's (1963) discussion of ethnographic, historical, and structural models. In consequence of such debates, anthropology became richer in its epistemological understanding and clarification of what is involved in different aspects of the research process.

In general, there is no doubt that Murdock moved the social sciences many steps forward. Harris, for example, notes some of the criticism of Murdock's approach, but also states (1968: 612) that "we must credit Murdock with his historic contribution, the triple linking up of modern ethnography, modern statistics and the statistical comparative cross-cultural survey method."

Murdock was disappointed at the blank reception of his more formal contributions to a logico-deductive or axiomatic theory of kinship terminology, excruciatingly tested through all of its various consequences. Yet *Social Structure* did stimulate a rethinking that was conducive to modern mathematical approaches in anthropology. Early papers on mathematical anthropology (e.g., White 1974), exploring the areas of social anthropology where theory had been axiomatized, built on insights by Murdock and his controversies with, as well as the contributions of, major French and British social anthropologists. Anthropologists now engaged in cross-cultural research use a variety of new theories and methods, but continue and deepen Murdock's concerns with quantitative and comparative work.

During the 1960s and thereafter, it became especially clear that no single approach could in itself capture the complexity of the multiple tasks of anthropological research. Each approach had its limitations, as well as its advances. But if there were problems with Murdock and the comparativists' too abstracted, birdseye view, there were also problems with respect to the views that were too close to the ground. Indeed, one of the problems of a too limited descriptive and ethnographic or cultural particularist approach often was that it tended to distort the object of inquiry by emphasizing apparent exotic differences or by looking for explanations within the logic of a culture, while ignoring the more powerful effects of world historical process on

local conditions. Slowly, but increasingly, anthropologists have been using comparative research to study the effects of world systems on markets and labor, intensity of production, warfare and conflict, modes of production, division of labor, and a range of new problems (Burton and White 1987). Many anthropologists increasingly see the need for studying the impact of capitalist systems of production on local societies and cultures. The comparative data base developed by Murdock was subsequently revised to provide the means for such comparisons.

One advantage of a statistical method, in our concern for a scientific and historical valid perspective in anthropology, as Harris (1968: 614) notes, is that: "Once the practice of stating causal relations in terms of probabilities becomes firmly established, many problems which have plagued generations of determinists and antideterminists will dissolve themselves into a more profitable level of discourse." Harris (1968: 618) also states that: "Statistical cross-cultural surveys can, indeed must, be used to supplement other modes of generating and testing hypotheses, but they cannot be used alone or even as the primary sources of nomothetic statements." He notes that some of the problems lie in the samples that were at the time all synchronic. With the absence of time-structured data, "Statistics cannot validate functional hypotheses or hypotheses about origins when the data are synchronic" (Jorgensen 1966: 162, cited in Harris 1968: 618). Presently, following decades of debate, there is a growing acknowledgment of the need for statistical and broader comparative approaches and simultaneously for considerable refinement in mathematical methods, statistical techniques, sampling, and consequently more adequate theory testing and more robust emergent theory.

Second in Murdock's legacy were his students, notable in quantity and quality, both at Yale and in his first decade at Pittsburgh. He passed on to his students, and they to their students, an unrelenting concern for the quality of ethnographic description and for attention to the more fundamental questions of the nature of comparative categories, of rules and processes to be pursued in descriptive and comparative work, and of adequate models of representation. As Lévi-Strauss (1963: 315) put the problem: "it is at the very moment when anthropology finds itself closer than ever to the long-awaited goal of becoming a true science that the ground seems to fail where it was expected to be the firmest: The facts themselves are lacking, either not numerous enough or not collected under conditions insuring their comparability." Murdock's students, as Goodenough and others have detailed, carried some of the core problems and controversies about the very possibilities of a comparative science to the center of the field. In many instances, there was a return to a more thorough study of individual cases.

By the late 1960s, some of anthropology's concern with categories and comparability had been securely carried forward, and the foundations had been laid for other work in the 1970s and 1980s that went back to rethink Murdock's concerns with axiomatization of theory. But now the focus had shifted to the nature of the fundamental constraints and processes in social structure and to the development of appropriate scientific methods and models for the study of social structure and process. It may be less generally recognized that Murdock, too, over his lifetime, did much rethinking of his own positions on these issues, so that it is necessary to divide his work into periods. As Sahlins (1976) notes, Murdock (1972) in his Huxley Memorial Lecture, explicates his theory most clearly. He had moved well beyond the discussion of typologies to emphasizing his main theme, that of seeing cultural forms increasingly as epiphenomena of social practice.

Third in the Murdock legacy were his efforts to found the Cross-Cultural Survey and its successor, the Human Relations Area Files. Here, we include his monumental efforts in cataloguing world ethnographic materials: the *Outline of Cultural Materials* (1938, 1982) and *Outline of World Cultures* (1954, 1983), which are the basis of HRAF's cataloguing and indexing systems, and the *Ethnographic Bibliography of North America* (Murdock 1941; Murdock and O'Leary 1975).

Fourth is the legacy of Murdock's cross-cultural data bases, which we subsume under the heading of the Ethnographic Atlas. This includes the 250-society data base in *Social Structure* (1949), the 565-society data base of the "World Ethnographic Sample" (1957), the 1,267-society *Ethnographic Atlas* (expanded from its 1967 *Summary*), the 186-society Standard Cross-Cultural Sample (Murdock and White 1969), and the 563-society *Atlas of World Cultures* (1981) that he worked on in his last years.

The fact that each of these various data bases served one of two very different functions goes back to Murdock's central concerns in *Social Structure*. Two of these data bases were intended to be representative samples for use in cross-cultural tests of hypotheses: the data base in *Social Structure* and that in the Standard Cross-Cultural Sample. Each of the other three data bases, with from 560 to 1,267 societies, was intended to be a sampling frame, that is, a universe of cases from which to draw a sample. In Murdock's mind—and this goes back to Galton's objections to early cross-cultural studies by Tylor—there was no benefit to indiscriminantly large samples. Large samples only compounded Galton's problem, as when Tylor counted up nearly 200 nonindependent cases of Australian Aborigines in a single cell of his contingency table, as evidence for one of his hypotheses. Galton's problem (see note 1) could not be addressed directly without alternate statistical methods that were not to become available until several decades later.

It was to overcome the limitations of his samples that Murdock labored at constructing large sampling frames, from which the world's cultures could be classified into types. The object of such a stratified frame was to pick independent cases from each type and put functional hypotheses to test under the assumption of independence of cases and, wherever possible, cases of independent similarity, presumably a result of independent invention. At meetings of the Society for Cross-Cultural Research, he often talked about testing his hypotheses in *Social Structure* once more with a new, scientifically rigorous sample. In another paper in this volume (Whiting, Burton, Romney, Moore, and White), we can see part of his dream realized, as some of the theories in *Social Structure* are examined with evidence from new samples.

When White joined the Pittsburgh faculty in 1967, Murdock had just completed his summary volume for 863 of the 1,170 societies in the *Ethnographic Atlas*. He had also completed his classification of world cultures into 412 cultural clusters, and he was about to publish his classification of the clusters into 200 major cultural provinces. These provinces displayed sufficient cultural distinctiveness to be regarded by him as independent cases for sampling purposes.

It would have been ideal, in 1968, to put the punched card deck for the *Ethnographic Atlas* through a computer program to sort the 1,170 societies into clusters on the basis of similarity. Unfortunately, at that time we did not have a program capable of clustering 1,170 societies at once, as we do today—and again we refer to the paper written by Whiting et al. (this volume).

Murdock's judgment about cultural classifications was sound. This has been ascertained in various studies by Harold Driver (Driver et al. 1972, Driver and Coffin 1974) on North American Indian societies, where Driver found high convergence between computer-generated similarity clusters and Murdock's classifications. While many others have made classifications of cultural types, Murdock was better at it than most. He had a prodigious memory for ethnographic detail, and he had read and digested well over 10,000 ethnographic books and articles. For Murdock, the culture area classifications were not an end in themselves, but a means to sampling design. Murdock's cultural provinces were a central part of the Murdock-White (1969) design of the Standard Cross-Cultural Sample (SCCS).

Murdock and White began a joint project of building a representative sample. Murdock had accepted and included in their 1969 publication a rather radical change in the foundation of cross-cultural methodology. The argument can be summarized in two injunctions: Do not try to eliminate nonindependence of cases, as it is in principle impossible in cross-cultural samples to eliminate the effects of diffusion, common history, common origin, common empire, or shared membership in a

common regional ecology. Instead, measure nonindependence of cases, and take these measurements into account in testing hypotheses. This is what Harold Driver had been advocating all along in his search for an integrated approach to historical, functional, and evolutionary processes in the testing of hypotheses.

In short, the effort to construct the standard sample (SCCS) achieved a double objective. First, it heightened the statistical efficiency of the sample by stratified sampling. That is, it avoided the essentially duplicate cases that can consume coding efforts without increasing the effective sample size. Second, it included a design for measuring nonindependence of cases, and it pointed in the direction of the solution to Galton's problem that has been recently attained with the development of cultural autocorrelation methods (Dow, Burton, and White 1982; Dow, White, and Burton 1982; Dow, Burton, Reitz, and White 1984; White, Burton, and Dow 1981).

Once involved in the effort of coding an entire range of variables for the new sample—far beyond the range of the *Atlas*—Murdock stopped expanding the *Ethnographic Atlas* through installments in the journal *Ethnology*. His goals of building a sampling frame and then an improved cross-cultural sample for testing hypotheses had been accomplished.

An unexpected bonus for Murdock's general scientific system resulted from his colleagues' successes in reconceptualizing and solving the problem of simultaneously testing different types of theories so as to include historical as well as functional and evolutionary processes. The early work on this problem was carried out by Driver (1956), Naroll (1965), and others. The solution—taking into account the effects of history in destabilizing the cell values of contingency tables (Galton's problem: see note 1) by means of autocorrectional methods—was carried out by White, Burton, and Dow (1981; see also other references, above). The bonus of the work on autocorrelation is that data sets such as the massive (1,267 societies) *Ethnographic Atlas*, intended by Murdock only as a sampling frame, but (because of Galton's problem) not a valid sample, is now usable, in its totality, for testing hypotheses, in spite of Galton's problem.

Murdock's coding efforts, then, have left us with an incredibly rich legacy for the testing of hypotheses through cross-cultural research. How was Murdock as a coder? What is the reliability of the codes he left for cumulative research? What is their utility and what is the degree of their validity? As for reliability, a number of studies (e.g., White 1988) show that Murdock's codes have amazingly high reliability. He did all of his coding on the *Ethnographic Atlas* himself, and he employed no assistants. Although many of his students worked on coding projects, he relied on their codes over his own only when they had done the primary ethnography, and, even then, he was meticulous about catching coding errors.

The question of the validity of his codes, however, relates to questions of theory, and here we must now backtrack to the central enigma: the extent to which Murdock's codes, the reactions of his students to his work, and his reactions to his students' work, can be understood in terms of the central line of theory that he developed in *Social Structure*.

Social Structure Revisited

Murdock attempted to follow a systematic and formal approach to the study of social organization and structure, following in some respects the earlier and less successful mandates of Tylor in his use of statistical methods. As Lévi-Strauss (1963: 307) notes, he had convincingly demonstrated that "cultural forms in the field of social organization reveal a degree of regularity and of conformity to scientific laws not significantly inferior to that found in the so-called natural sciences" (Murdock 1949: 259). If his approach was still imperfect, it was considerably more painstaking than any previous approach of this type, and a great deal of effort had already been expended to carve out a viable data base, even in the Yale Cross-Cultural Survey. The task itself was so ambitious that it naturally drew fire from many camps within anthropology, but it represented Murdock's impressive assault on what he would view as mere verbalizations or theories lacking adequate basis for falsification or acceptance. Many anthropologists, if they did not agree with the exact methods and formal procedures of Murdock, also saw the need for more systematic studies. Radcliffe-Brown and others of his generation saw a need to arrive at valid generalizations about the nature of human societies and particular systems, and there was a particular appeal in exploring the concept of structure, since it appeared to be "a means of linking social anthropology to the biological sciences" (Lévi-Strauss 1963: 301).

White, among many others, was attracted to working with Murdock both by *Social Structure* and by the advantages for anthropology of a cumulative data base. The two issues are closely intertwined, as it could be argued in one sense that a data base is no better than the theoretical categories that generated it. Neither Murdock nor White, for different reasons, was a subscriber to the latter view, and Murdock was appalled by it. In his view, the greatness of anthropology lay in its legacy of a worldwide, descriptive, ethnographic data base. His codes tried to do justice to the descriptive content of ethnographies, within the limitations of a comparative framework, and to identify these limitations rather than to ignore them, analyzing where we were as a discipline in our data collection.

Even at the level of the development of his social-practice theories, Murdock remained dissatisfied with his codes. The debate over residence categories (Goodenough 1956) is a case in point. Murdock's classifications of types of residence types grew ever more complex as the debate widened. In *Social Structure* (1949: 225), he offers a simple six-part code: patrilocal, matriloc, avunculocal, matri-patrilocal, bilocal, and neolocal, with alternative usages in small letters. By 1961, with the *Ethnographic Atlas*, we have a proliferation of distinctions, adding life-cycle variability (a preposited first-year code), and five additional primary types: nonestablishment of a common household by the couple; virilocal; uxoriocal; optionally viri- or avunculocal; and optionally uxori- or avunculocal. The "options" reflect an attempt to deal with the countervailing view that these residence patterns are not "norms" or "rules" of the society, but choices, under constraint, of the individual.

The problem of a societally versus an individually constructed social reality smacks of a dualism that Murdock tried to eschew, but of which he has nonetheless been accused (e.g., Leaf 1979). Like Kroeber recanting his superorganic theory in his later years, Murdock (1972) thoroughly denounced his own slips into societalism in his 1971 Huxley Memorial Lecture. It may be true, in Murray Leaf's terms, that Murdock was a dualist, if by this Leaf meant that one chooses to give causal or ontological priority to one of the two terms of oppositions, such as society versus the individual. If, in the process of comparison, Murdock veered too much on the side of individuals operating under constraint, he could be faulted for not seeing units as coordinated wholes. If he veered too much on the side of structure, systems, and the like, he could be faulted for reification and making abstractions that were several times removed from the data.

Murdock's ontology in his early work was heavily on the side of a social reality derived from individual actions and their cultural constructions. This is the central theoretical point of his book, *Social Structure*. It is the theory behind what has been termed the "main sequence kinship theory," wherein the following mechanisms are held to be operative: cooperative activities generated in the division of labor help to congeal residential groupings (the weakest link in the theory), which in turn precipitate the formation of kinship and descent groups, which then develop consistent lines of inheritance and succession (and perhaps also nurturance and authority). The proximities and interactions among kinsmen that are generated in this sequence of functional adaptation are the "Determinants of Kinship Terminology" of the famous Chapter Seven of his book.

If Murdock could have followed his students' lead in the debate over "rules" of residence, he would have done so. But how could one develop cross-cultural codes to reflect the nature of individual choices

made under constraint? Even if in theory it might be possible to do so, the level of ethnographic detail that would be needed was generally lacking. Ethnographies did not inevitably provide it. More to the point, as Goodenough (1970) has noted, a different strategy is needed in the comparison of cases than that needed for the description of cases. In his Huxley Memorial Lecture, Murdock increasingly began to consider the impact of constraints and to weaken his concerns with culture and social forms. Increasingly, over the course of his lifetime, he took more seriously the idea of needing to test not single hypotheses but a series of interlinked propositions, a change that leads directly to the current work that compares not single traits, but systems as wholes, and that searches for replication.

The search for a comparative framework in coding, however, does not tell the whole story as to how Murdock's codes are constructed from the point of view of theory. He was adamantly antisociological in the sense of not readily accepting Durkheimian "social facts" as privileged realities. In this sense, there was no purely sociological structure to be found in *Social Structure*. The central part of the book argued a social science on first principles derived from Hullian psychology, the behaviorism of his day, and that of the famed interdisciplinary group at the Institute of Human Relations at Yale, made up of Hull, Dollard, Sears, Murdock, Wissler, Malinowski, Whiting, and others.

One of the main critiques of Murdock (one that we also debated with him) was that his version of social structure was relatively weak in the crucial area of social stratification; nor did it have a place for social roles, either in the modern sense or as in Radcliffe-Brown's or Nadel's (1957) version of social structure. Roles and strata for him were the epiphenomenal outcomes of more basic processes that embodied a "tendency toward consistency," that is, toward functional integration or reciprocal adjustment, requiring time, and thus subject to "cultural lag." For the most part, he tended to substitute ethnological categories for a fully evolved theory of stratification and role. This is not a small shortcoming in the context of understanding the modern world politically, economically, or historically.

But even in the more intractable issues concerning the foundations and character of social structure or the relationship of structure and cognition, Murdock was aware of the limitations of his prior perspectives, as always rethinking the fundamentals in some area. His 1980 book, *Theories of Illness*, shows the progression of his sociological imagination, with an almost Durkheimian consideration of the effect of social organization on systems of belief. When contrasted with *Social Structure*, it is a remarkable book, in showing his continued openness in the exploration of new themes and relationships in systems. Murdock never took the further step of attempting to reconcile and elevate deeper grand theoretical perspectives as, for example, Sahlins

(1976) or Harris (1968) tried to do. Perhaps his resistance lay in his lifelong distrust of grand theory, or perhaps it was simply the task of a later generation to reconcile alternative perspectives as they evolved from some of the debates in which Murdock and other figures of his time participated.

Murdock's massive failure was the inability to come to terms with worldwide historical process, and it remained until the end of his life the greatest weakness of his comparative method. Because of this lacuna, he opened the door, despite his massive contributions, to being accused of the kind of Weberian solipsistic reduction (Sahlins 1976: 95) that omitted collective processes in favor of individual intentions. But in this view, Murdock, via what Sahlins calls his "praxis theory," also tended to liberate anthropological thinking from strictly culturological explanation. In building a system that compared populations on fundamental life conditions, he paved the way for different comparative treatments of history. If, as noted by Sahlins, he tended to dismember larger cultural systems by treating individual cases as autonomous, then the comparative method also calls for theories and variables that link the local level to larger regional and world systems.

The Legacy of the Atlas and the Standard Cross-Cultural Sample

Regardless of whether or not we choose to agree with Murdock's particular theories, some of which were excellent, others more problematic and controversial, the legacy of the *Atlas* should be judged on its own terms. Murdock's codes do not possess validity only within his particular theoretical approach. Their validity depends, to be sure, on the type and the level of the theory one is testing, but it is precisely in this sense that they *do* have a general validity. We can find, for example, massive evidence from Murdock's own codes of kinship behaviors—in particular kin dyads, contra the early Murdock—to support Nadel's (1957) views of sociological regularities in the ways that social structures are built up out of constituent social roles. Murdock's codes on the division of labor, for another example, have proven enormously useful in understanding the sociological organization of gender roles under constraint. Murdock, with a brilliant sense of problem, identifies the significance of Durkheim's earlier concerns with the division of labor, and develops his own comparative theory (Murdock and Provost 1973). Burton, Brudner, and White (1977), taking exception to his formulation, initiated a lively and interesting debate. The division of labor was subsequently examined from a range of different theoretical frameworks. Other examples abound of the utility of Murdock's codes for testing theory, and the number of such

tests, from books and articles, is now in the thousands. Useful summaries of theory, central to anthropology, and tested through cross-cultural research, are found in Levinson and Malone (1980) and Burton and White (1987).

In other cases, however, one must either use Murdock's codes cautiously or go back to the original ethnographic sources with a different conceptualization and coding of the problem. Goody (1973), for example, noted the occurrence of indirect dowry (bridewealth transmitted to the bride) in marital transactions, and Schlegel and Eloul (1987, 1988) modified Murdock's codes to produce significant theoretical results. Some of Murdock's codes obscure regional variations that he thought were inimical to finding universal regularities. White (1988) found that Murdock's tripartite classification of polygyny (absent/limited/general; the latter utilizing a cutoff of 15 percent for married men) obscures the major regional differences in marriage forms that emerge when the grounds for limited and general polygyny are identified. His five-part code (absent/exceptional/limited to a few leading individuals/limited to a higher class of men/generally aspired to by all men and usually achieved by the older and wealthier) helps to identify major differences between African, New World, and other regional patterns in polygyny. The differences are easier to explain by an integration of functional, historical, and evolutionary theory (White and Burton 1988).

Murdock's codes, and the coding directed by him as part of the Standard Cross-Cultural Sample (SCCS) coding projects, have the advantage of giving very specific information about particular societies, within a framework of similarity and contrast across the full range of human cultures. The following are useful observations about the generality of the codes and samples. (1) One is not required to establish that the cases coded in a Murdock sample "represent" any larger social unit, in the sense of focal group characteristics standing in for the attributes of a larger society or ethnic unit. All of the Murdock codes, from the *Atlas* forward, are pinpointed to particular local groups at a particular time. There is no intention of making, say, a particular Egyptian village study "representative" of all Egypt. Furthermore, in case there is any residual confusion on this point: (2) The "representative sampling" of the Standard Cross-Cultural Sample is simply a means of improving sampling efficiency through stratified sampling. As pointed out above, stratified sampling avoids the essentially duplicate cases that can consume coding efforts without increasing the effective sample size. (3) Apart from the difference in sample size, the HRAF Probability Sample (Naroll 1967) and the SCCS are equivalent in terms of statistical efficiency and design. In sample designs where available information varies markedly from one unit to the next and where stratified sampling is limited to a single well-described society in each stratum, there is no statistical advantage to random selection of

candidates over picking the best described. (4) Whatever the sample, there is no need, nor is it advisable, to assume that cases are independent. It is always advisable, and more so for larger samples (even with an N of 30), to test and control for regional or historical effects (diffusion, common linguistic origin, common membership in empires, common impact of colonial regimes, position in regional or world systems, etc.). (5) One is not limited, in cross-cultural studies, to analysis of phenomena that are widely independently invented or that show similar distributions across different major continents, as Murdock was so often wont to demonstrate as part of his argument for the validity of his findings in *Social Structure*.

Murdock came to recognize, in his later years, the validity of Driver's position, that an integration of functional, historical, and evolutionary theory was possible in cross-cultural research. This is amply demonstrated in his last major substantive study, *Theories of Illness*, which he published at the age of eighty-six. Here, he integrated a regional analysis of continentwide patterns of similarity in beliefs about causation with functional and evolutionary explanations. Many may remain dissatisfied with his formulation of this or other problems. But the field of culture and social organization, as Lowie (1920: 88–90) stated: "is not a region of complete lawlessness, and continues to invite further investigation in a comparative framework."

In many respects, the history of Murdock's work explicated the complex theoretical as well as methodological task of the comparativist. One seductively simple solution is to ignore comparison entirely. That orientation virtually ignores the cumulative efforts and findings of anthropology. Murdock's program in practice suffered the embarrassment of a vice that was at the same time its chief virtue. Especially in its early stages, it was inductive to the point of naiveté. But at the same time, his more general proposal was to substitute concrete evidence for speculative deduction. Murdock's efforts brought to our attention the fragmentary and incomplete nature of the ethnographic record, which, if anything, became in itself a subject of attention. His concerns and the work of his students, as much as that of Boas before him, strengthened awareness of the importance of fieldwork and of the observational process. Murdock's concerns tended to focus anthropological efforts less on the exhaustive collection of data and more on specific problems of general relevance to comparative research. Murdock's main concern was for the issues and laws of development of social structure and social arrangements as they emerged out of process. Ironically, however, his initial concerns did not lead him to study the world as an interconnected system in a state of flux. The codes began as an attempt to capture static forms. We might agree especially with Harris (1968: 611) that "Murdock's approach to evolutionary causality is heavily compromised by his adoption of a short time-scale and by a failure to combine his comparative approach

with the nomothetically relevant data of prehistory or, indeed, with any macro-temporal view of the human condition." It might be noted in passing, however, that Murdock's system—by its design features and also in light of new theory, technology, and method—now allows that more dynamic perspective.

The Revised Atlas

Murdock himself worked in his last years on a revision of his coding frame and his classification of world cultures. Once he had completed the work on the Standard Sample and understood the nature of the solution to Galton's problem, he also understood that his previous methods of sampling and sample-frame construction were too restrictive. Murdock and White, even in the Standard Sample, had underrepresented the European societies, because of Murdock's desire to reduce the effect of diffusion, common empire, or civilization, or that of replicating similar cases. This caution was not necessary, as such effects could be handled in other ways. In his *Atlas of World Cultures* (1981), he developed a more uniform basis for classifying relatively more homogeneous cultural strata. He shifted markedly toward linguistic grouping (shared historical origin) as the underlying principle of such homogeneity, and he provided fuller coverage of the range of European societies.

Passing Down the Legacy of the Atlas and Other Cross-Cultural Samples

Replicability, cumulativity, and multiple methods are hallmarks of science that Murdock attempted to promulgate. He founded and edited the journal *Ethnology* to provide an outlet for ethnographic and cross-cultural data. Another journal now provides the voluminous cross-cultural codes, codebooks, bibliographies, and articles in a form that can be interrelated and analyzed electronically. *World Cultures* began publication in 1985 as a quarterly journal, published on diskettes, and devoted to research in the Murdock tradition. Its issues include the *Ethnographic Atlas* data and codebook on 1,267 societies and most of the codes and data that are available, by forty different authors, on the Standard Cross-Cultural Sample. This format for diffusing the Murdock legacy to research scholars, departments, libraries, and classrooms allows the Murdock data bases, and others like them, to be used interactively, with new streams of data constantly feeding into the system. The journal includes a summary selection or cross-section of the most useful and reliable codes out of 1,200-odd published variables.

The strength of the cross-cultural data base is that it allows for continual improvement in the development, formulation, and testing of theories. As Harris (1968: 610) states: "one part of Murdock's historic role appears to center on his transmission of Sumner and Keller's interest in a 'science of society,' especially in terms of the emphasis that cultural evolution is 'orderly, adaptive change' (Murdock 1949: xii)." Ironically, however, Murdock's perspective requires greater historical contextualization, especially if we are to take techno-economic causation seriously. Murdock was, if anything, too quick to slough off other major perspectives. We need a series of different levels and types of explanations, frameworks, and causal paradigms. Yet at the same time, we need to continue the best of the Murdock legacy, including the design of careful sampling frames that contain most or all of the potential cases relevant to particular types of problems. More regional analysis is also needed. The Village Studies Program at the Institute for Development Studies, University of Sussex, for example, has constructed a sampling frame of 3,500 contemporary village studies. Given such frames and theoretical problems to be addressed and tested, we need to develop coded samples that are statistically efficient. We need many different types and varieties of new sample designs, depending on the type of problem being studied. These must be followed by codebook development, coding, and the testing of theories and hypotheses, using some of the sophisticated methods available today. The development of new samples and codes to address a wider variety of theoretical issues was, needless to say, one of Murdock's frequently mentioned goals. Ultimately, in working with others, he made a monumental contribution to the healthy expansion of the comparative and scientific aspects of anthropology.

The possibilities of many different types of analysis are inherent in cross-cultural research, so that many alternative types of cultural theory may be explored and tested in the future. Research to this end is compellingly significant, in that most anthropologists agree that causation beyond the local community or societal level is at work in the world system. We are now at the point where many different perspectives can and should be explored further in a global context. They include problems of historical materialism, problems of utility and the cultural order, problems of practice and the symbolic order, and the like. For those with orientations not adequately represented in comparative studies to date, the system invites wider participation. At this point it is not simply more codification that we need, although we need that also, but more highly evolved theoretical frameworks for further transformation of existing cross-cultural studies. This goal involves reforming the elements of different theoretical approaches and testing them against each other, pressing toward sharper problem formulations.

There are different starting points for developing comparative systems. Murdock began by defining cultural studies in a moderately traditional but solid way for comparative research. Many powerful traditions operate in anthropology within different notions of what culture, social structure, and organization entail. Murdock's system may represent for many only a half-way house. Subsequent starting points can build on the existing system. All of this speaks to the future and to further studies that differ somewhat from those already undertaken. New lines of study for comparison, from a variety of intellectual traditions, are needed to make comparative studies more representative of the wide range of concerns of the field.

Summary

Anthropologists, like other social scientists, historians, and natural scientists, are constantly reworking theory and interpretation. One need not agree with Murdock over specific theories to find much of his legacy—including codes, samples, and sampling frames—enormously valuable for the wide variety of different theoretical problems amenable to comparative treatment. Initially, the problem with cross-cultural study was that it followed in practice a too monolithic philosophy of comparison. Those following Murdock would be less likely to argue for a single comparative method. It is in the nature of the scientific enterprise that our theories improve as we learn in the process. We keep what is useful and move forward. One should not underestimate Murdock's enormous foundational contributions to the field. He was truly a remarkable man, whose vision was ahead of his time. It would be impossible to catalogue the wealth of the legacy he passed down without going through his specific contributions and those of the many people who have built on his concerns. The scope of his project facilitated a dialogue across generations. On the one hand, he passed down much of what was most valuable from past generations, including generations of research, and at the same time he facilitated and welcomed dialogues with students, colleagues, and critics of radically different persuasions. Examples are numerous. Some of White's work in mathematical modeling and the development and use of new methods of comparison, although different in fundamental respects—and separated in age by a fifty-year gap—owe a great deal to Murdock and to heated debates with him. So does the work of his very prominent students and their students, whose research has gone off in many new directions, attempting to look at different aspects and problems that they once debated with Murdock, directly or indirectly, as illustrated by a reprinted collection of the work by Melvin and Carol Ember (1983).

The notion of making anthropology a comparative and a systematic discipline poses problems that the field today has not yet entirely surmounted. But we are significantly further along than many other sister disciplines in this critical area, given the major fundamental questions that Murdock, among others, raised openly, loudly, and clearly for debate from the 1950s forward and that have since been extensively explored, both theoretically and in field research. His prodigious efforts helped massively to improve and expand the cross-cultural data base and samples and to prepare the ground for the sophisticated statistical techniques and newer methodologies that eventually followed.

In consequence of the efforts of Murdock and others in his generation, and of their students, anthropology as a discipline has moved forward in many areas simultaneously. Increasingly today we can deal with societies as interdependent, interpenetrating systems, although hardly equal in their effects on one another. Much of the legacy of the past is constantly being transformed: new bodies of data are added; new codes, new levels and dimensions for inquiry are developed. Most recently, new and potentially more powerful and subtle methods based on mathematical advances in other areas have been added that give us more power and flexibility in our treatment of intricate problems. With these capabilities and others that will be added in the future, we are evolving as a field with a cumulative, complex, and open scientific feedback system.

Note

¹Sir Francis Galton in 1889, commenting on one of the first cross-cultural papers by E. B. Tylor, noted that in statistical correlation, tabulating related societies (i.e., societies having the same traits by diffusion or historical relatedness) as separate cases increases the variances of correlations and spuriously inflates tests of significance. Murdock (1949: 115), however, goes so far as to argue that comparative or sociological analysis (functional as opposed to historical) is only appropriate where the possibilities for different cultural elaborations are so limited that independent invention is the only likely explanation of cross-cultural similarities. This caveat would severely limit comparative research, particularly if we accepted Murdock's conception of historical explanation as purely ideographic or descriptive. Contemporary research practices regard Galton's problem as a methodological problem with a relatively simple solution of controls for propinquity, diffusion, or historical effects. Moreover, we now regard historical generalizations about diffusion, acculturation, world system effects, etc., when phrased in terms of event classes rather than simply particular cases, as nomothetic explanations on a par with other types of generalizations.

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