

**T O T E M S A N D
T E A C H E R S**

**Key Figures in the
History of Anthropology**

Second Edition

**Edited by
SYDEL SILVERMAN**



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Julian Steward during fieldwork among the Carrier Indians, 1940. Photo from the University of Illinois Archives (15/2121-19), with permission of Gary Steward

JULIAN H. STEWARD

Robert F. Murphy



In 1921 the department that Alfred Kroeber had founded at Berkeley added to its faculty Robert Lowie, one of Franz Boas's first students, who had spent thirteen years with the American Museum of Natural History doing ethnological research on North American Indians. That same year, Berkeley began systematic graduate training in anthropology. Among the students in the department during the 1920s was Julian Steward.

Steward was born in 1902 in Washington, D.C., where his father was an attorney in the U.S. Patent Office. The family were Christian Scientists. When he was sixteen, Julian was sent to the Deep Springs Preparatory School, on the Nevada-California border, near the Owens Paiute reservation. It was there that he first formed his attachment to American Indians and that he developed an interest in both archaeology and ethnology.

Steward began his undergraduate studies at Berkeley, where he took the introductory course in anthropology that was given jointly by Kroeber, Lowie, and Edward Gifford. After a year at Berkeley he transferred to Cornell, for financial reasons. There was no anthropology at the time at Cornell, although its president, Livingston Farrand, was an anthropologist trained by Boas. After graduating, Steward returned to Berkeley in 1925 to study anthropology. He received his Ph.D. in 1931 with a dissertation entitled "The Ceremonial Buffoon of the American Indian." The dissertation (which Steward would later claim, incorrectly, to be the first Ph.D. thesis in personality-and-culture) combined distributional study, encouraged by Kroeber, with behaviorist psychological interests stimulated by Lowie.

Steward began teaching in 1928 at the University of Michigan, where he initiated instruction in anthropology. In 1930 he was replaced by Leslie White, and Steward moved to the University of Utah, where he instituted programs in both ethnology and archaeology. He returned to Berkeley in 1933, teaching there for a year.

Steward started his research career in archaeology, working during the late 1920s in the Columbia River Valley. During the Utah years he became the first state archaeologist of Utah; he worked on early Shoshoni and desert remains, then shifted his attention to the Pueblo expansion into southern Utah. He carried out his important ethnological research among the Shoshoni from 1933 to 1935. Thereafter, apart from one summer (1940) among the Carrier Indians in British Columbia, he did no more field research but became a research director and promoter. In 1935 he joined the Bureau of American Ethnology of the Smithsonian Institution, where he stayed until 1946. During this period he organized the work on the seven-volume Handbook of South American Indians (1946-1959). He also became director of the Smithsonian's Institute of Social Anthropology; in that role he inspired and funded research on Middle American and South American peasantry.

In 1946 Steward went to Columbia University. Along with his teaching and supervision of a large number of doctoral dissertations, he undertook a comprehensive study of the island of Puerto Rico. Steward stayed at Columbia until 1952, when he moved to the University of Illinois. He remained there until his retirement, initiating during these years an ambitious collaborative research on the modernization of traditional societies. Julian Steward died in 1972.

Coming into American anthropology at a time when it was dominated by historical particularism, cultural relativism, and functionalism, Steward played a leading role in the revival of interests in generalization and in the development of modern materialist approaches. Regarded as a scientist by some because of his attempts to formulate cultural laws and as a historian by others because of his limiting comparison to selected cases, Steward himself defined his endeavor as a lifelong search for causality. He brought ecological interests into modern anthropology through his own studies of Indians in which he sought causal processes in the interactions between culture and environment, his according priority among the elements of culture to "core" features over "secondary" ones, and his explicit concept of cultural ecology. His efforts to define culture types, his concept of levels of socio-cultural integration, and his outline of cross-cultural regularities in the development of civilizations became part of the new evolutionism. Finally, his programs for research in complex societies marked a break from folk-urban polarities, from global depictions of national character, and from the study of communities as microcosms, focusing instead on the relationship of "sub-cultures" to regional and national contexts.

It is a Durkheimian, and Freudian, maxim that the ancestors do live among us; they are imbedded in our thoughts, they motivate us, they limit us, they restrain us, they shape our view of reality, they endow us with the language in which we speak of the world and ourselves. This has long been intuited among many peoples through the institutions of ancestor worship, which speak of the organic and continuing social and psychological links between the generations, links that symbolize the corporate nature of society. The ties between our anthropological teachers and founders and ourselves may not be as primary as those of kinship, but they are commonly modeled on these attachments and share some of their qualities. They are multistranded, or functionally diffuse, they are hierarchical and entail varying degrees of authority, they are incorporative, and they are ambivalent. We may not follow the paths set by the ancestors, but our very deviations have been conditioned by them.⁷

The reality of the ancestral spirits is manifest in Columbia University's Department of Anthropology, for the department has spent this entire century centered in the fourth floor of Schermerhorn Hall. Each office is known for its past occupants. Alfred Kroeber's office is across the hall from mine, and the couch on which he took his mandatory afternoon naps is still exactly where it used to be.⁸ At the end of the hall, Franz Boas's office is currently occupied by Marvin Harris. Harris, it should be noted, has attacked almost every position ever taken by Boas, but he does so with a sense of immediacy and intensity that one would usually extend only to the living. It might be said that Boas is dead, but this would be irrelevant, for Harris remains inescapably his heir. It could even be added that Harris's sallies against "historical particularism" are part of the ritual that keeps Boas alive, at least in spirit.

The office next to my own has had a wooden bench outside its door ever since the Boasian period. On it, generations of Columbia-trained anthropologists have waited their turn, as in a doctor's office, to see their professors. The bench is still there and students still sit in waiting, just as they used to, but the office's tenants have changed. Julian Steward, a former occupant of the office, died six years before this writing, and he left Columbia twenty years before that, but as a true ancestor his intellectual presence continues. It is noteworthy that the present tenant of the office studied under a Steward student, and the one before him took courses with Steward in the early 1950s. Several of the current members of the Columbia faculty wrote their doctoral theses under Steward's direction, returning to Columbia to teach, and they help continue a perspective, if not a specific theory. Beyond this influence, the department's graduate students seek out Steward's writings year after year, for they still find him lively and topical. It is indeed hard to realize that over a quarter century has passed since he taught at Columbia.

Steward's influence at Columbia is reminiscent of that of A. R. Radcliffe-Brown at the University of Chicago. Both were compelling teachers who attracted devoted followings, and both had an impact upon the local anthropological culture

that was out of proportion to their relatively short tenures—Radcliffe-Brown spent but six years at Chicago, from 1931 to 1937, and Steward was at Columbia only six years too, from 1946 to 1952. The persistence of the tradition in each case was due in part to a tendency of both departments at the time to hire their own graduates, a practice that has ended with the diversification of anthropology and the institution of affirmative-action programs. But the impetus of the ancestral ideas at both universities has been carried on more vigorously by the students than by their mentors, for Steward and Radcliffe-Brown have had something of lasting importance to say to their respective audiences.

Steward's influence at Columbia was great, but so also was Columbia's upon Steward. He fitted into a preexistent theoretical tradition, which he served to shape and crystallize; it was as if he and Columbia's graduate students had sought and found each other. I will review the salient features of Stewardian anthropology in detail in later pages, and I give here only a few highlights to help clarify Steward's preeminence at Columbia. Its primary characteristic was a naturalism through which cultures and societies were viewed, much as a physicist would view matter and energy. Implicit in this positivism was a faith that culture is caused, and causal, and that we can find these chains of determinism, and thus explain culture, through proper methodology. Coupled with this assumption, Steward's anthropology had an earthy and common-sense orientation that regarded the exigencies of work and livelihood as among the most important of these determinants. He was not a systematic philosophical materialist by any means, but more than any anthropologist of his day, he saw the key to much of human culture in food-getting activities. Steward's theories also stressed the active and sensate over the symbolic and conceptual in social life, a logical enough outcome of his preoccupation with work and groupings. The approach was realistic, unetherealized, self-consciously tough-minded, and dynamic. It found its subject matter in the more mundane aspects of culture, and it sought explanation in sinew and sweat.

Steward's anthropology would seem, at least superficially, to be a sharp departure from the Columbia tradition set by Boas. There never was a Boasian "school," however, and Columbia's Ph.D.s were remarkable for their diversity of interest and theory. What they did get from "Papa Franz" was a cultural realism that was shared by his students Alfred Kroeber and Robert Lowie, and by their student Julian Steward. Added to this was Boas's politics, expressed in pacifism during World War I and radicalism in his later years. This orientation was also characteristic of most of his students during the 1920s and 1930s, a community of opinion that had nothing to do with Boas's anthropology but which could only have been encouraged by his politics. One of Boas's students fondly recalls a conversation with him in which the dilemma of being young and starting out in life during the depression '30s was raised. The old man disagreed, saying these were the best times in which to launch a career. Conditions were indeed miserable, he added, "but if I were young, I would do something

about it." To the extent that the anthropological theories of the Columbia students reflected their political orientations, Boas must be considered a primary influence. Ironically, it would appear then that Boas was the spiritual founder of Columbia's so-called materialism.

Boas's influence was strongly reinforced by time and place. The Depression era germinated radicalism of thought and action within anthropology, as within all intellectual pursuits. That some departments of anthropology remained unaffected by the mood of the times was symptomatic of moral and physical isolation from their social milieus, a smug aloofness that used to be paraded as "objectivity." Columbia, however, was located in New York City, then as now a center of intellectual innovation and ferment as well as a staging area for all the malaises of urban society. If the politics of the Columbia students stressed human exigency and struggle, it was because these were a part of their way of life. And it is not surprising that their anthropology was based upon the same premises and worldview. Such was the ambience at the Columbia to which Julian Steward came in 1946.

The specific theoretical heritage of Boas, and that of Ralph Linton and Ruth Benedict, may not have been congenial to the ideas brought by Steward to Columbia, but the basic epistemology was the same and the student culture, as opposed to faculty thought, was even more receptive in 1946 than during the depression '30s. The Columbia students of the late 1940s were a new breed. As in the past, there was a large contingent of New Yorkers, but the postwar generation differed in significant ways. Most of the students had come of age during the Depression and, haunted by the failures of their parents, were prone to economic anxieties and driven to succeed. They had also gone through a war; a majority had been in combat zones and most had been enlisted men. It may be conjectured that the division between officer and enlisted person contributed to the political climate of the student body and to their affinity for the world's losers, and their overseas experience must surely have whetted their appetites for other cultures. The student body of the period also included a higher proportion from the lower class and lower middle class as compared to past decades, for their studies were largely supported by the educational benefits of the G. I. Bill. Finally, the universities of the entire country were inundated by male students who had postponed higher education for up to four or five years, and, as a consequence, the Columbia anthropology department had a lower percentage of female students than during any time in its history. The composite picture that emerges from these traits is of a student group that would have no trouble understanding the compelling motivations of an empty stomach or seeing authority as emerging from the muzzle of a gun.

This short sketch does not, of course, do justice to the variety of interests and personalities of Columbia's students, but the characteristics were sufficiently pervasive to set the tone of the department. The materialism of the postwar students ranged from Marxism of variable orthodoxy through Leslie White's "culturology"

to, more commonly, an eclectic concern for economic and environmental factors. At the very least, it was a view of the world that was both agonistic and realistic. There was some division within the student body between the new students and a few who had entered somewhat earlier. The latter were holdovers from Linton and Benedict, were in varying degrees influenced by Freud, and were more interested in symbolic systems than in concrete social action. Having lost Linton by resignation in 1946 and Benedict by death in 1948, they found themselves with a faculty that was uninterested in their work and fellow students who were antagonistic to it. One of the credos of the evolutionists and materialists of the 1940s and 1950s was that there is a necessary contradiction between cultural explanations and psychological ones; this is not at all true, of course, but the notion persists to this day.

Steward did not have to establish a "school" at Columbia—he found one waiting for him. The student temper combined with Steward's theories and persuasiveness as a teacher to produce an almost instant following. The Columbia faculty at the time were few in number, consisting of Steward, Benedict, William Duncan Strong, Harry Shapiro, George Herzog, Gene Weltfish, Marian Smith, and Charles Wagley. The size of the student group was, however, larger than at the present writing, due to the pressure of the returning veterans and a very loose admissions policy at that time. The result was a badly overworked faculty, a situation that was somewhat ameliorated by the fact that the few undergraduate offerings were taught by the junior members, Weltfish and Wagley. The sheer weight of numbers and the attraction of his ideas placed a heavy teaching burden on Steward, a problem that was complicated by bouts of poor health throughout his Columbia tenure. But even in the semesters in which he was unable to teach, he held seminars and discussions at his house in Alpine, New Jersey.

Steward's classes were large, his audiences attentive. He was neither flamboyant nor charismatic in the classroom, but his lectures were masterpieces of integration of fact and theory. Though noted as a theoretician, he held to the principle that theories must be based on facts and that facts, in turn, were unintelligible without theory. I particularly remember his one-year course "Greater Southwest Culture Sphere," which was an organizational gem. He covered the standard ethnography and archaeology of the Southwest United States in exhaustive detail, but he did it within a framework that he first outlined in his paper "Ecological Aspects of Southwestern Society" (1937). He intertwined and contrasted the concepts of culture area and culture type, analyzed the varieties of society in terms of his method of cultural ecology, and attempted to see the entire picture as episodes in evolutionary process, from pre-Basketmaker horizons to the modern period. The empiricism was painless for being placed in context; this also made it retainable in our minds. Only a few members of the class had a deep interest in the area as such, but the course was a better introduction to Steward's thinking than some of his more theoretically slanted seminars.

There was a certain magnetism in Steward's lectures that was created in large part by his remarkable gift for synthesis. He also radiated a sincerity and conviction that welled from a certainty that he was on the right track. This was conveyed without undue dogmatism or authoritarianism, and the students sensed that he wanted them to join him in the quest. The theory was not complete, the answers were not all collected, but, we thought, if we develop the theoretical structure further and flesh it out with the right kind of data, then we would surely break through to a true science of man. Like another great teacher, Leslie White, his closest intellectual associations were with students and he treated them as partners, albeit junior ones. Steward conveyed a sense of excitement and purpose to us, and he took us seriously; the latter was probably his most important gift to his students.

Columbia University, in common with other institutions, was profoundly affected by the political moods of the postwar period. Want and struggle were part of the life experience of many of the Columbia anthropology students, and they were understandably attracted to a theory that took adequate account of this aspect of society. This basic view of social life also contributed to a political climate that, in terms of the prevalent ideology of the country, was radical. Although eschewing organizational ties that would involve endless meetings and passing out mimeographed circulars at subway stations, many students of the time were influenced to varying degrees by Marxian thought. They found in Steward's ideas a sympathetic resonance, an alternative to a rigid philosophy that would nonetheless preserve an emphasis on the material conditions of life. Moreover, it was a theory developed in the language of anthropology and using anthropological data. The ponderous terminology and obsessive concern with class struggle of orthodox Marxism were replaced by a more open-ended theory dealing with human labor in its natural setting. To his students, Steward was pioneering a new road to the understanding of history, a road that did not lock them into a fixed system.

The growing tensions of the Cold War in the late 1940s and the appearance of McCarthyism in the early 1950s cast a pall over academic inquiry that drove Marxian thought underground and rendered somewhat suspect even the non-Marxian materialisms of Steward and White. It is difficult for today's students to understand the mood of the period or to empathize with its victims, for there has been a profound liberalization of moral and intellectual norms since the early 1960s. The subject is certainly beyond the scope of this essay, and I can only note that thought was constrained by fear for livelihood; the 1950s were chill, narrow, rigid, and fearful years, and this had an inevitable effect upon the discipline of anthropology.

To his credit, Steward did not modify his theories under this pressure. It could be argued that it kept him from taking a more avowedly materialistic position, but this would ignore the fact that he had maintained a pluralistic stance throughout his career, as documented by several of his earlier publications (see, e.g., Steward

1941). Actually, Steward was quite frank about the political implications of his theories and used the Chinese Revolution as an example of an independent evolutionary change, contrasting this to the conspiracy theories of the right, which he saw as a kind of diffusionism. Harmless though this may seem today, it was bold talk in 1951. Contemporary students often note that Steward never referred to Karl Marx's writings, despite some of the common ground between them. This may have been in part a matter of discretion—few other anthropologists of the day mentioned Marx, for that matter—but I believe it arose more from a lack of deep interest in Marxism. He had done his student reading of Marx and was well aware of the theory, but he never gave any indication that he had given it close study. To the best of my knowledge, most of his scholarly reading was restricted to anthropology, and his entire intellectual frame of reference was couched within the discipline. There are, of course, profound differences between Steward's theories and the Marxism of the time, which will come out more clearly in the pages below. It can be stated summarily here, however, that Steward gave greater attention to technology and environment than did Marx, and his view of historical process was wholly nondialectical. The chief area of overlap was a common concern for labor process. In the final analysis, Steward was a liberal, and his anthropology was consistent with his politics.

Steward's Columbia students are now a few years older than was their professor some thirty-odd years ago, a thought that will dismay them. They include Pedro Carrasco, Stanley Diamond, Louis Faron, Morton Fried, Sidney Mintz, Robert Manners, Elena Padilla, Vera Rubin, Elman Service, Elliott Skinner, Eric Wolf, and many more.

Several others were indirectly influenced by Steward. Marshall Sahlins entered the department at about the time Steward was leaving, but his studies with Fried and Service were in the ecological-evolutionary tradition. Marvin Harris was not a Steward follower during his graduate student days, but he later acknowledged him as a principal source of his ideas on "cultural materialism" (Harris 1968, 1979). All of these people have pursued their own courses, their own interests, and their own ideas. What unites them, however, is a basic assumption, a premise and axiom, that social thought emanates from social action and that the imperatives of work, power, and sex are prior to the symbolic forms that encapsulate them. It is this very elemental and general kind of materialism that prevails to this day at Columbia, and not one or another particular theory. Despite the departmental sobriquet as "the cowshit-weighting capital of the Western world" bestowed by one wit, who will remain nameless as well as tasteless, its faculty and students display a variety of talents and inclinations that range over the gamut of current anthropological theory. But beneath this diversity there exists a community of understanding and a common language that was inspired by Franz Boas and crystallized by Julian Steward.

In sketching Steward's influence at Columbia, I do not mean to underplay his important role in the history of the Bureau of American Ethnology and the

Smithsonian Institution, nor am I ignoring his impact upon another generation of students at the University of Illinois.³ I am merely writing about what I know best and hope that others will write of their own experience of the man. Actually, much of the following account of Steward's theories will be based on his work during the Washington years from 1935 to 1946 and his tenure as a research professor at Illinois from 1952 until his death in 1972. Thus, I am not at all suggesting a view of his work seen exclusively from a New Yorker's perspective. (Like the lady who, when asked what route she had taken on her drive from New York to California, replied, "The Lincoln Tunnel.") Columbia, however, was a special high point in Steward's career, for he came into intensive interaction with a large group of involved and supportive students, most of whom have become major figures in the profession. Moreover, fortynish to fiftynish is a good age for anthropologists, though advanced senility for physicists and molecular biologists. By the midforties, the anthropologist has acquired the necessary background in empirical research and is ready to start putting it all together, and by the midfifties his talents are best used in issuing retrospectives and benedictions, such as I am giving here. Steward came to Columbia at the age of forty-four and left when he was fifty. He had come at the right age to the right people in the right place at the right time.

The appeal of Steward's anthropology to his Columbia students derived in good part from his approach to culture. He was not greatly concerned with the concept as such and generally accepted the classic definition of Tylor or its modifications by his teachers Kroeber and Lowie. Rather, it was his basic fieldwork method and the kinds of data he collected that distinguished his research from that of most of his contemporaries. By the 1930s, when Steward did his principal ethnographic research, there were no autonomous native social systems left in the United States. Indian culture was carried about in the memories of the aged and was unevenly and fragmentarily transmitted to the young. However well or poorly remembered, traditional cultures found little expression in concrete social behavior. Many practices, such as those connected with etiquette, child rearing, body usage, household ritual, and the like, continued with only moderate alteration, but other areas of social behavior had lapsed, leaving only disembodied traditions. The economic life of most Indian societies had been disrupted and totally transformed, reducing groups to absolute dependence on the whites. The native political orders had been smashed with the defeat of the Indian nations, and the political patterns remaining were usually either creations of government officials or responses to reservation life. Religious practices had been suppressed by the Bureau of Indian Affairs; the people proselytized by Christian missionaries. What was left of native American life was little more than memories.

Due to the historical situation of the American Indian, most attempts to document aboriginal cultures were based on informant interviews and not on direct

observation. An old man could tell the ethnographer how buffalo were hunted, but there was no way that such an event could ever again be observed. The result was that the researchers collected skewed data. The informants idealized and rationalized past custom, and they also standardized it. What was often elicited was not how a certain practice was done, but how it should have been done. The varieties of situation and expression were reduced to neat normative systems, a process of reduction that was aided and abetted by the investigator's own search for regularity and order. As most ethnographers have learned, it is extremely difficult to achieve even the most rudimentary quantification from an interview and virtually impossible when the informant's culture lacks developed systems of enumeration. This same problem extends to the collection of case material, or slices of real life, from interviews. The informant may know that there was once a preference for cross-cousin marriage, but he will usually be hard-pressed to supply instances of such unions from the past. In short, the entire realm of concrete social behavior was of necessity underreported in Americanist research. But this was exactly the kind of data needed for Steward's theories—and it was the kind he gathered in the field.

The primary characteristics of Steward's classic monograph *Basin-Plateau Aboriginal Sociopolitical Groups* (1938) are its meticulously detailed descriptions of local groups, their subsistence-seeking activities, and seasonal changes in their composition and organization. I retraced some of Steward's footsteps when doing research among the Shoshoni in 1954 and found myself unable to collect the same kind of material. The twenty years that had elapsed had taken their toll of older informants, but I believe that other factors were also responsible for Steward's striking compendium of behavioral data, as opposed to normative information.

The Shoshoni had not all been settled on reservations, and there were small Indian settlements scattered throughout the towns and ranches of Nevada. This not only continued the dispersed settlement pattern of native times, but kept the people in contact with the land. Not all of the old subsistence activities had disappeared. People still went out each fall to gather pine nuts, some of which were sold to be marketed as "Indian nuts." The native wildlife had been reduced by the whites, but deer, rabbit, and antelope were still taken and constituted an important source of meat during the depression '30s. People still knew where to find roots, they still used the old springs for water, and even their work for the whites took them into the land. They had not been as completely uprooted as most American Indians. Moreover, their aboriginal groups had been small, their memberships more easily remembered than would be true of societies based on large bands or villages. All these factors made the behavioral data accessible, but it took hard work to get it.

Steward crisscrossed the Great Basin, visiting every part of it, no matter how remote, and interviewed every single Shoshoni with knowledge of times past. Paradoxically, this geographical exhaustiveness, so essential to the development

of cultural ecology, was made necessary partly because Steward was also doing a culture-element survey of the region as part of Kroeber's distribution studies. Whatever the motive, in doing so he pioneered a brand of social anthropology that was based on behavioral observation and that saw the normative order to be derivative from this matrix of social action.

Steward's concern with behavior, and with the environmental and situational restraints on that behavior, is often overlooked by commentators, though it lies at the heart of his theory. Steward's peers considered the concept of "culture" to be the governing principle of anthropological research and its main contribution to the social sciences. This is fair enough, but most went on to stress the normative, symbolic nature of culture and its unidirectional determination of conduct. There was little scope in the concept for dynamic interplay between norms and behavior, for the path between them was a one-way street. As for the mechanisms by which culture was derived, it was no less a materialist than Leslie White who wrote that "culture causes culture." And if culture is a symbolic and not a behavioral affair, which White also maintained, then symbols are the causes of other symbols. Finally, given the fact that symbols are ideational, we are led into a total idealism. But it was an idealism that was well adapted to the normative memory-data being collected on the American Indian reservations. Paradoxically, the students closest to a dialectic between norm and action, at the time, were those identified with the personality-and-culture school, a group that was commonly charged with idealism. But a basic sociological materialism—as opposed to simple economic determinism—places activity as prior to idea and finds norms to be crystallizations of behavior; Steward's approach did exactly this.

Any focus upon social activity per se must account for the fact that behavior is carried out within the framework of constraints both internal and external to the social system, for failure to place it in context would result in complete nominalism. And it was Steward's isolation and analysis of these constraints—notably the external ones—that produced the theory of cultural ecology. One of the concomitants of anthropological positivism is a view of society and culture that posits inherent tendencies toward stability—if not toward outright homeostasis. The result has been that the roots of most social and cultural evolution and change have been traced to exogenous factors—to diffused technology, population growth, contact with other societies, and relations with the natural environment. Steward's theory was concerned with all of these, for he found a principal source of constraint upon, or determination of, behavior to be the patterns of work called for in the pursuit of subsistence.

The behavioral outcome of any social situation is partially governed by norms and partially by sheer necessity, by accommodation to certain inescapable facts within situational reality. In recent years, anthropologists have come to recognize that actors are by no means blindly impelled by the dictates of culture but, rather, often look to culture for meaning and rationalization of prior acts. Among primitives practicing cross-cousin marriage, it has been discovered that

most everybody has far more cross-cousins than parallel ones, a phenomenon that would be remarkable if it were genetically true. What happens, of course, is that a nice girl meets a suitable boy and the genealogies are brought into line with the romance. In our own society it has been found that, despite clichés to the contrary, morality can be legislated, as in antidiscrimination laws. All of this is to say that people often do what they have to do and then manipulate the symbolic system to give justification and meaning to their actions. In the cultural-ecological method, the necessity of certain ways of behaving is imposed by the absolute imperatives of subsistence and survival and the limited ways in which these imperatives can be satisfied.

Steward's effort to escape the whimsical arbitrariness of cultural relativity and unilateral normative determinism led him to search for the less flexible factors within any social situation. Symbols and groupings are capable of endless permutations, but other elements are given; culture must adjust to them rather than vice versa. One of these given elements is technology. Most of the tools and techniques of any culture are derived from other societies, and the material inventory of a group is highly dependent upon its position along routes of cultural diffusion. The presence or absence of items of technology is not, of course, wholly a matter of historical accident, for every culture screens and selects diffused traits, and some inventions are autochthonous. But how- ever much necessity may be the mother of invention, the reverse proposition is more commonly the case: Needs are defined by available technology—and resources. The resources offered by the natural environment are the second important given category in Steward's theories. Resources and technology cannot, however, be considered separately, for it is through tools and knowledge that natural features become culturally useful and humanly accessible. Thus, if there are no metallic ores in a region, one would hardly expect metallurgy to arise there. On the other hand, unless a group has a knowledge of metallurgy, the ore-bearing strata are just so much useless rock. This is all very rudimentary; of course, but it is well to remember that technology and environment are not completely independent variables and that the two together define the life chances of societies.

The heart of Steward's anthropology is the analysis of the ways that the two givens, technology and resources, are brought together through human labor. Just as the possibilities of any society are promoted or inhibited by the natural environment, and just as technology is contingent upon history, so also are there limited ways in which specific tools can be used on specific resources. That is, certain operations may entail quite delimited and narrowly defined forms of labor, involving characteristic patterns of collectivization and individualization of work, cycling of activities, specialization of tasks, and so on. This does not imply that one and only one form of labor is effective in any operation, but rather that there are limits of variability in patterns of work contingent upon the tools and resources exploited.

A few examples may be in order. There are many kinds of fish and many devices for catching them, but how people go about fishing depends much upon the instruments and the type of fish being sought. In the Amazon basin, the main aboriginal fishing techniques were with the bow and arrow and with fish poisons. Bow-and-arrow fishing is usually done from a canoe and requires at least two people, one to shoot the fish and the other to paddle. The paddling can be done by a woman or young boy, whereas the bow and arrow are always handled by men; this type of fishing can thus entail cooperation within the conjugal family. Fish poisoning, to the contrary, often draws entire villages into cooperation. The poisonous sap of certain vines and roots is released into smaller streams or lagoons by men who beat sheaves of the vines with clubs. The toxic substance drifts downstream with the current and paralyzes the gills of the fish which, dead or stunned, are easily taken from the water by other men and by women and children. The operation involves the coordinated labor of many people. There must be enough men pounding the vines to achieve a certain concentration of the poison in the water, and large numbers of people must be stationed downstream to prevent the stunned fish from escaping. In contrast, hook-and-line fishing, introduced by the whites, can be carried out by solitary individuals. Most species of fish can be caught by any of these means. The giant pirarucu fish, however, is best taken with hook and line, though this technique cannot be used for piranha, which are able to bite through the leaders.

The constraints upon labor imposed by certain technologies when applied to specific resources are evident in hunting as well as in fishing. Herd animals, such as peccary, bison, and antelope, are usually hunted by collective means, for a lone hunter may kill only one animal before frightening away the herd, whereas a group of men may dispatch enough to provide meat for an entire community. The Plains Indians hunted bison from horseback, the riders flanking the running herd and killing the outliers with bow and arrow or lance. Groups of unmounted Shoshoni hunted the fleet antelope by driving herds down valley floors to pens, and hunting parties in the South American forests bring peccary bands to bay with dogs. Deer, mountain sheep, and other non-herd animals, to the contrary, are most commonly taken by individual hunters through stalking and ambush. The weapons used are also critical in determining the organization of work. In almost every instance in which firearms have been introduced, there has been a tendency toward more individualized hunting. The greater range and striking power of guns make it less necessary to bring a concentration of firepower to bear upon the animals. White bison hunters, for example, used powerful rifles to pick off the lead animals in a herd from a long distance; the systematic killing of the lead bison kept the herds from stampeding.

These are not invariant patterns, for collective hunting parties often encountered and killed nonherd game, and individuals could and did prey upon herds. Rather, a strain was set up toward a certain mode and organization of work that

conformed to the practical needs of the task. Steward sometimes referred to this simple process as "adaptation" to the environment, a choice of words unfortunate for its rather biological and Darwinian overtones. We need not, however, posit blind mechanisms of natural selection of the most efficient forms of work, for the matter is more simple. The subjects of anthropological inquiry, be they primitives, peasants, or whatever, must be assumed to possess a good store of pragmatic common sense and a comprehension of their situation at least equal to that enjoyed by the ethnographer. They are perfectly capable of understanding and acting upon the usefulness of certain modes of work. This does not necessarily lead to the single most effective way of getting a job done, but it sets a sharp limit on the alternatives.

In my own experience among the Mundurucú Indians, I watched large collective garden-clearing ventures fritter away time in sociability and side diversions, clearly sacrificing efficiency for social solidarity. But the Mundurucú saw this just as clearly as I did, for they knew very well that in some villages the work of clearing was effectively carried out by two or so men. This was regarded, however, as further evidence of the abandonment of proper traditional ways by such people. What must be remembered in interpreting seemingly wasted time is that the Mundurucú had the time to waste—there was nothing else that they urgently needed to be doing, the job eventually got done, and they had a good time doing it. With the general erosion of social solidarity, however, the pattern of garden-clearing work became more individualized, as befitted the new social system and the technology of steel axe and machete. It also fitted into a new economic order in which work had become intensified due to the demands placed upon labor by trade with the whites. As the Indians' appetites for Western goods increased, "spare time" became scarce and pressure grew to carry out tasks in the most expeditious ways possible. One result has been the attrition of forms of collective activity that had their source in the social system rather than in technical necessity.

That most subsistence operations were seen to allow for a degree of flexibility kept Steward's ecological approach from being monistic and rigid, while empirically demonstrating the limits of this flexibility. Steward recognized that the range of such variation was itself dependent upon environmental and technical factors. A crude technology permits small latitude; conversely, the extent to which a society can control and alter the environment is a measure of freedom from some of its constraints. And environments that offer fewest resources and are most forbidding to human occupation will be those that allow for least flexibility. This was brilliantly demonstrated in Steward's study of the Great Basin Shoshoni, a people possessing only a rudimentary technology and living in a harsh landscape.

Steward's study is too well-known to warrant detailed recapitulation, but its main argument is that the very structure of Shoshoni society was a reflex of its habitat and exploitative patterns. Possessing none of the technology needed to realize the full potential of the environment, the Shoshoni scavenged from it,

subsisting on its sparse game population and its thinly distributed wild vegetation. Steward painted a picture of a people reduced to the bare essentials of life, living in a society that was all infrastructure. The Basin Shoshoni had no stable political organization beyond parental authority and the prestige given by age. There were no chiefs, and leadership was a temporary and ad hoc matter, a situation that was consistent with the fact that there were no stable or formally defined sociopolitical units beyond the conjugal family. The Shoshoni had no tribes, no bands, no villages, no clans, no lineages. They were found scattered in small clusters of a few families each across an enormous terrain extending from southern California to Idaho and Utah; egalitarianism, individuation, and amorphousness characterized the social system.

Steward found the roots of this system in the processes of work, as shaped by tools and resources. The division of labor was along sex lines, the women gathering wild vegetables and grains and the men responsible for the protein in the diet. The female labor of root digging and seed gathering scattered the women out, for the plants were thinly distributed and the work completely individual. No help was needed, nor was there any division of tasks, in unearthing roots with a digging stick or beating seeds from grass with a basketry flail. Most hunting was similarly pursued by individual men. Deer, rodents, and other small game were taken by lone hunters by stalking and ambush. Large-scale, cooperative, and organized hunts for antelope and rabbits took place, but these were infrequent and occurred in shifting locales. Leadership was temporary and the roster of participants always different, and stable cooperating groups did not emerge from these occasional and movable feasts.

The cultural-ecological method took Steward from a consideration of known and useful resources and technology to the labor involved in exploitation of the environment to a final consideration of the causal influence of that labor on other social institutions. The latter involved a kind of functional analysis that, unlike some varieties, always took the organization of work—the cultural-ecological nexus—as its starting point. In the case of the Shoshoni, it would be almost tautological to say that work was a determinant of social structure, for the foraging units were the principal segments of society. Work is a social activity in the strictest sense, and work groups are preeminently social groups. There is interchange and equivalency by definition between the artificial categories of the "social" and the "economic," and it is an artifact of the anthropological imagination that reifies the two classes and attributes a sort of causal energy to them. Of course, they are related! In terms of concrete social behavior, they are the same. Nonetheless, Steward took the trinity of resources, technology, and labor to be a priori, for they were acted upon by elements exogenous to the social system, and they involved a kind of inflexibility and necessity. There were strict limits to the extent that this nexus could be shaped to fit other institutions; in the final analysis, most of the accommodation would be to the cultural-ecological situation and not *by* it.

The key element in Steward's anthropology, then, is not economic or environmental determinism, but a view of social life that sees social behavior as situationally shaped and constrained and that then goes on to derive cultural norms from regularities in that concrete behavior—this is a point I made before, but its importance merits repetition. It is this underlying principle that guided Steward's Shoshoni work and most of his subsequent writing. The approach was not unique to Steward, nor was it originated by him. Germs of it can even be found in the writings of his old teacher, Robert Lowie. For all of Lowie's fulminations against materialism, he was among the first anthropologists to understand the central importance of rules of residence in the formation of descent principles. In a critique of the evolutionists' idea that kinship and territory are mutually exclusive criteria of organization, Lowie declared flatly that kinship may simply be the language in which territorial, or spatial, relationships are phrased. People are kin because they are brought together and not the reverse. And the reasons for bringing them together, or keeping some of them apart, may lie in the necessities of livelihood. Lowie attacked Lewis Henry Morgan's notions that the American Indians were at a higher evolutionary stage than the Hawaiians and that matrilineality preceded bilaterality by citing the bilateral Shoshoni, whom he put at the bottom of the evolutionary ladder, and certainly below the complex societies of Polynesia, for essentially technological reasons. Lowie's thoughts were phrased in cautious and hesitant hints typical of his style, but Steward went far beyond them, erecting a theoretical edifice that was uniquely his.

Labels are ways of aborting thought and of endowing the complex with a counterfeit simplicity. Cultural ecology, from its sound, connotes a kind of environmentalism that was not at all a part of Steward's anthropology. He had little patience for some latter-day ecological approaches that embed their human subjects in ecosystems, rather than sociocultural ones, or that are concerned with biological populations rather than societies. He did not see the ultimate goal of cultural ecology to be a systematic statement of humanity's relation to nature. Rather, he saw cultural ecology as a method for studying and understanding the causal processes by which societies are formed. And the key element in these processes was the very social factor of work.

In a similar vein, Steward's name has been associated with cultural evolutionism, but he was never much concerned with developing general evolutionary schemes; evolutionism, too, was a method of study and not an end in itself. His approach, commonly known as "multilinear" evolutionism, was an extension of anthropology's comparative method into the temporal dimension. Steward was already combining his cultural-ecological method with comparison by the mid-1930s, when he wrote a well-known article on patrilineal hunting bands (1936). In it, he argued that a type of exogamic, territory-holding, patrilineal, and patrilineal band tended to be associated with the hunting of nongregarious, nonmigratory animals. The regular recurrence of this basic pattern

in a number of societies, he argued, demonstrated that a common causality was at work. This was a fair conclusion at the time, although later research showed that most of his representative groups were not formally patrilineal and that some were bilocal or even tended toward matrilocality.

Whatever the results, Steward's methodology was clear. One can examine a series of societies having common social-structural features to see if their ecological situations are similar, and one can also scrutinize societies living in similar environments under similar technological regimes to ascertain whether they have common features of social structure. As in his article on patrilineal bands, Steward preferred the former strategy, for it was consistent with the fact that he considered societies to be the proper units of analysis and regular recurrence of social institutions to be the basis for their scientific study. Moreover, he was not an environmental determinist, a position he sought to stress in "Tappers and Trappers" (Murphy and Steward 1956). In this article, we showed that similar processes of social change had occurred among the Mundurucú Indians of Amazonian Brazil and Canadian Algonkians. Despite radical differences of environment, contact with the whites had resulted in native dependence on trade goods, which were bought by the Mundurucú with latex rubber and by the Algonkians with animal pelts. The exploitation of each resource involved individualized work in small, delimited territories and resulted in both cases in the fragmentation of the population into residence on these holdings and their reorientation from village or band life to the trading stores.

The "Tappers and Trappers" essay sought to show the causal influence in social change of new economic pursuits and new forms of labor. And since the piece was both comparative and historical, it was also an exercise in the methodology of multilinear evolution, although I do not believe we used the term at all. The article most commonly associated with Steward's kind of evolutionism is "Cultural Causality and Law" (Steward 1949), perhaps his single most influential paper. Starting from the writings of Karl Wittfogel, who found in the history of China an association between the practice of irrigation agriculture and the rise of the despotic state, Steward extended the inquiry into other areas in which there had been autochthonous development of the state—that is, of states that were not spin-offs or derivative phenomena from other states—including those of the Indus Valley, the basin of the Tigris and Euphrates Rivers, the Nile Valley, the Valley of Mexico, Yucatán, and coastal Peru. The recurrence of large-scale irrigation agriculture and the intimate association of this mode of production with centralized political power were striking in most of the areas. There has been some evidence that the state preceded irrigation in the Valley of Mexico and it was clearly not a factor in the Mayan development, but the case for the rest of the world was strong.

In its essence, the methodology of "Cultural Causality and Law" followed the same lines as Steward's previous work. A relationship between irrigation and the state was posited for China and found to be recurrent in other parts of the

world—a demonstration, to Steward, of similarity of cause. To clinch the argument, he took each society in his study from its earliest beginnings through the development of agriculture and on to the rise of cities and states, showing in each phase a causal sequence that explained it and paralleled the sequence in the other societies. The actual method of analysis was much the same as that used in his Shoshoni work, however different the subjects; multilinear evolution had merely added new controls and criteria of proof. In his analysis, Steward started from certain characteristics of the natural environment, in these cases climatic aridity and highly fertile soils, which provided the need for irrigation and the potential for great productivity. The technological factor was agriculture, which originated in dry farming and then became coupled with the new techniques of irrigation as cultivation spread into the arid zones. Given these elements, certain forms of organization were needed to carry out production. First, large concentrations of collective labor were required to build canals, weirs, and flood-control works and to maintain these facilities. The control and direction of this labor force, and the expertise needed to undertake these complex engineering projects, called for a specialized leadership group having coercive powers over the population. Such an elite would also have the responsibility of allocating the water to the communities served by the irrigation systems. The process inevitably produced class stratification and the state. It was not the only way the state could evolve, but it was indeed the source of much of civilization.

As in his labeling of the comparative-historical method as “multilinear evolution,” Steward always sought new concepts and nomenclature to delineate what he was doing. Thus, there were “levels of sociocultural integration,” “sociocultural types,” and “culture cores,” all of which had the heuristic value of communicating a complex set of premises or strategies under a single rubric. At times, the terminology acquired an ultimate reality that it never truly had. Steward, Wolf, and I spent hours and days discussing whether the “culture core” contained other institutional features than those relating to the productive system—until we finally recalled that “core” had no meaning other than what we thought useful. Rubrics and labels can block thought as well as facilitate it, but Steward’s methodological lexicon had the merit of glossing and stressing his ideas and making them more communicable. We should not, however, allow the verbiage to overlay and hide the fact that there was an amazing consistency of method and purpose in Steward’s writings, from his first articles on the Shoshoni to the mature reflections of his retirement. This consistency centered on the mode of production as a mediating activity between environment and culture and on the socializing effects of labor.

It is through their ideas that the ancestors live on, and Steward’s legacy has a vitality that derives from its continued relevance to anthropology and to our society in general. But the special quality of excitement and discovery that Steward’s students of thirty years ago experienced has passed and can never be

recaptured—by them. Anthropology’s special deliciousness is based upon a continuing opening up of new vistas, a finding of the totally new or a refinding and a different perspective on something familiar. We are constrained always to press on, to seek progress through research and imagination, but it is necessary to remember that the bases of our concepts were laid in the past. Under every innovative and heady idea lies the shadow of past thought, and some of our greatest discoveries are restatements of what had been known before. Students will be rediscovering Steward, in spite of themselves, for generations to come.

DISCUSSION

GENE WELTFISH: You say that Steward was not a dialectician, but I wonder if you wouldn’t admit some kind of dialectics, that I do see in Steward and that to a certain extent explains Marvin Harris’s unwillingness to let Steward fully into the materialist cadre. If you are going to allow ideology a causative role in a model of causality in culture, then I think that constitutes a trap door—the kind of trap door that Harris would hate to see opened—that once opened allows for all kinds of theoretical jumping around in a dialectical way.

MURPHY: I suppose you could reinterpret or help him along a bit; for example, in “Cultural Causality and Law,” the progression from the Formative Period to the period that he called Cyclical Conquests comes about with the growth and development of irrigation agriculture and with the soaring of population until the previous forms of society are inadequate. But I’m not sure that this is the kind of dialectics that either Hegel or Marx had in mind. For example, the idea of the growth of population being an outcome of increased subsistence causing certain changes can be handled in straight mechanical causal terms. This is the way Steward tried to handle it. There was no necessary development of contradictions within the social systems themselves in Steward’s work, although maybe you could supply it for him.

Incidentally, Steward’s convictions about the causal role of subsistence did not always carry over into his view of things in his own life. During the years that Eric Wolf and I were his research associates at the University of Illinois, we occupied a little suite in a corner of one floor of the College of Agriculture, space released only because the main part of the College of Agriculture had moved into more sumptuous quarters. Down the hall from us was the office of the Department of Meats, which was chaired by a gentleman named Sleeter Bull. Julian had a series of secretaries, none of whom could spell. Finally he got a young lady who could spell beautifully, type perfectly, an absolute gem. He treated her with the greatest of care, he pampered her. But the Department of Meats would cook up rashers of bacon every morning; and as Eric and I would sit in our office going out of our minds at the smells, Sleeter Bull would come

tiptoeing down the hall and say to the secretary, "Why don't you come down and have a little bit of breakfast, dear?" and she would go. Every now and then he'd say, "You look sort of peaked, looks like you could use a steak tonight for dinner," and he would take a great big thick T-bone out of his locker and give it to her. She left Julian for him. Julian was very cut up about this and thought this was manifest disloyalty, poor repayment for all the consideration he had given her. Eric and I tried to tell him, "Look, this fits very well with all of your theories. First things first. There is a bit of the Shoshoni in every one of us." Julian said, "Well, that may very well be, but people should have some principles before thinking of their stomachs all the time."

ALEXANDER LESSER: I would like to go back to the question of what Steward meant by multilinear and unilinear evolution. It seems to me that biological evolution is inevitably multilinear; that's what it was in Darwin, and that's what it always has been. The interpretations of the term "evolution" as unilinear have nothing to do with factual work in biological evolution. Multilinear evolution would mean that evolution takes place differently in relation to different forms; for instance, the bird is an end of evolution, just as man, but the bird is not on the way to man, nor man on the way to the bird. Julian's use of "multilinear," as you have explained it, ends in an effort to find a single cause of cultural evolution. In his studies of the comparisons of high civilizations, or the conception of levels of sociocultural integration, what he seeks is actually a unilinear development, *one way* in which basic evolution is taking place. I can't see that "multilinear evolution" has anything to do with his effort, except insofar as he was trying to get away from oversimplified conceptions of unilinear evolution.

MURPHY: You might see a multilinear evolutionism becoming a unilinear thing, in the phases of the development of civilization. The question then becomes, what are the ways of getting primary states other than through the development of irrigation? I'm not sure that Steward had a hard time dealing with the fact that all the evidence indicated that the influence of irrigation was problematic in the Valley of Mexico, and certainly absent in the Maya case, and that there might be other ways that the state could be reached. I think that the only person in anthropology who used a Darwinian model, a biological evolutionary model, was Fred Eggan. This appears in Eggan's "method of controlled comparison" (1954) even though he doesn't look upon that approach as an evolutionism. In his original attempt to understand Siouan kinship, Eggan always tried to deal with what happened: He started off with a linguistic group, such as the Sioux, and he then tried to see what were the ramifications of Siouan kinship, what various lines it took, how it diverged from what was presumably a uniform proto-Siouan kinship. Some people have called that a genetic model.

LESSER: But the ways in which Steward worked on, say, the development of the state, are unilinear and not multilinear evolution. The concept of multilin-

ear evolution which he developed has really nothing to do with his work on the development of types, because he always looked for a unilinear pattern of development: one way in which bands changed, kinship forms changed, states developed. Maybe his procedures and his derivations are correct, but they are unilinear, and his concept of multilinear evolution is irrelevant.

MURPHY: The nearest that you come to a really unilinear scheme in Steward is in the idea of levels of sociocultural integration, the idea that there are, within any system, certain levels: Within the state, for example, are certain regions, within the regions communities, within the communities kin groups, within the kin groups families. Many people tried to make that into a unilinear evolutionism. He developed the notion of levels of sociocultural integration in his book on area research (1950a), and he proposed it as a means of trying to handle the study of acculturation. The point he made was that the history of contact in South America indicates that those institutions and aspects of culture that went first were the ones related to the broadest levels of integration, while the narrowest, most parochial levels of integration were least affected in the acculturation process. The Spaniards came into Peru and smashed all the idols, completely destroyed the state cult and substituted Catholicism, but afterwards there were left local shrines. As the Spaniards consolidated their control, they eliminated the local shrines; there were still family practices left, and so forth. Three or four hundred years after conquest the people were still being socialized as Indians; they were still sitting and walking like Indians, because these are things that you learn earliest and in the most primary, localized level of integration. Now this was very much like a unilinear evolutionism. I think that the idea of a progression from family level of integration to band level of integration to chiefdoms to states—for example in the work of Elman Service—these evolutionary taxonomies, I think, derived straight from Steward's levels of sociocultural integration. I do believe this is a kind of unilinear evolutionism. But not Steward's other ideas.

ROBERT CARNETIRO: I think Professor Lesser is right in thinking of "Cultural Causality and Law" as essentially the expression of unilinear evolutionism. Even though Steward originally meant it to apply to areas that were arid or semiarid, since he was using the Wittfogel hypothesis, he nevertheless pointed out that he had six or seven stages by which he could accurately characterize cultural development in various areas. He pointed out that in the Maya lowlands and in Southeast Asia, which were not arid areas, these stages still applied. He thus discovered that he had a unilinear sequence. But something surprising happened, and I wonder if you can shed any light on it. The article had created a sensation in 1949, and a few years later there was a symposium at the AAA meeting in Tucson, at which a number of scholars familiar with each of the areas that Steward had dealt with examined his sequence and his theory of causation in detail. The result was as you suggested: In the Valley of Mexico

there did not seem to be evidence of irrigation very early, and in Mesopotamia, too, large-scale irrigation came after the state was formed. The causal basis for Steward's developmental theory was severely undercut. Steward, reacting to this, backed away from the sequence of stages he had proposed to characterize development. One archaeologist on the panel was displeased at this and told Steward, "You should not throw out the baby with the irrigation water." "Cultural Causality and Law" was the high-water mark of Steward's evolutionism. From then on he began a retreat, which became very marked by 1953. He made such an enormous advance. Why did he back away from trying to consolidate it and from finding as much order and coherence as he could? The rest of his life was retreat, as far as cultural evolution was concerned.

MURPHY: I think this gets back to my point that fundamentally he wasn't really interested in evolutionism. He didn't consider himself an evolutionist. To the extent that he found it useful for discovering laws, yes, he was an evolutionist. To the extent that he looked upon history as not being a hodgepodge concatenation of events one after another but as involving some orderly unfolding, yes, he was an evolutionist. He was an evolutionist in many ways. Everybody is a unilinear evolutionist, if you use the basic premise that evolution is characterized by a progression from simplicity to complexity. But it depends on how you define evolutionism.

MICHAEL HARNER: Something that is very distinctive about Steward is his preoccupation with causality, which I think was at least as important, if not more so, as his search for law. I wonder about the origins of this preoccupation. Certainly the Berkeley academic scene may have played a role in this, if only in a negative way. For example, Gifford's famous required course, "World Ethnography." Edward provided the students with the annual rainfall, temperature ranges, and many other environmental facts related to perhaps twenty cultures in the world, and the students were left to absorb it all and were not given any explanation as to why these facts might be significant. This was typical of the Berkeley undergraduate program; there was a tremendous variety of area courses and virtually no theories to explain why all this cultural diversity developed. I wonder if you have any insights into this very salient aspect of Steward's character, his search for causality.

MURPHY: I really don't. I know Gifford's course contained every known fact; people learned more about world ethnography than they ever wanted to know. (After Gifford retired, the course was always given to the youngest faculty member joining the Berkeley department. Very optimistic, bright young people used to be driven into absolute despondency by the information that they were to teach "World Ethnography." Then somebody would take them aside and say, "It consists of the group you're studying and anything else you want to throw in.")

Certainly Steward never got this preoccupation from Kroeber, who was concerned only with culture, not with social institutions qua social and not with social activity qua activity. He didn't really get it from Lowie, who said he was interested in causality but then imposed such strict scientific canons for determining causality that they simply couldn't be met in most of anthropology. But at the same time this aspect of Steward's work began to show up early. It might partially have come out of Steward's work in the natural sciences at Cornell. In part it may have been a reaction to his Christian Science mother. In part it derived from the Shoshoni experience, in which he saw the direct, dramatic, and irrevocable impact of the limitations of environment on an entire way of life.

LESSER: I don't see that an interest in causality needs to be explained. It seems to me that to be a scientist you have to be interested in causal relations, otherwise you're not doing anything. The fact that Steward followed the interest up in a particular way was the particular theory that he had, but even functionalists doing synchronic studies are interested in finding causes and effects and influences and interrelationships, when they look for connections that involve interinfluence. I don't see any problem with an interest in causality. The fact that he—unlike other people—tried to pursue it through broad cultural comparisons is one of the things that he added, but he didn't have to expound multilineality, because that was irrelevant to what he was trying to do. When Lowie used the *Kulturkreise* as an illustration of multilineality he had a good point, because that was supposed to describe different developments. But when you are looking for similar or parallel development you are looking for some kind of unilineality, and you have to use causality; otherwise you've got nothing.

MURPHY: Obviously there are all sorts of causes; Steward was interested especially in a causal sequence that showed some organic and regular unfolding of tendencies. He was interested in those causes that could be adduced from, that were contained in, autochthonous internal developments in societies. It was that interest that led him to engage in long dialogues about diffusion versus independent invention (which was one of the favorite problems of anthropologists of that time, of course). Steward always came down on the side of internal development as the thing to look for in the first place, because diffusion always seemed to him too adventitious and accidental. He wasn't denying its importance, but he always took the attitude that any society that was going to adopt some very important trait was probably ready for it anyhow. It was this attempt to see causal developments as emanating internally, autochthonously, from within the society, as having some sort of inevitability, and as being connected in some organic way with the unfolding of the society, that made him, in a real sense, an evolutionist as well as a student of causality.



Photo by Paul Byers, courtesy of Columbia University Department of Anthropology

ROBERT F. MURPHY was born in 1924 in Rockaway Beach, Queens, where he grew up. He attended Far Rockaway High School—a school that produced an unusually large number of anthropologists (among them Alexander Lesser and three members of Murphy's own graduating class of 1941), although anthropology was not taught (or even mentioned) there. Murphy's first exposure to other cultures, apart from growing up as an Irish Catholic in a Jewish neighborhood, came through naval service in the Pacific during World War II. After the war, he enrolled in Columbia College under the G.I. Bill, and a course in anthropology (chosen because he had heard it was easy) introduced him to the subject. His interest aroused by his teacher, Charles Wagley, he went on in 1949 to do graduate work at Columbia. Although at the time it seemed unlikely that one could make a living out of anthropology, he "found the university to be more agreeable than was either home or the navy."

At Columbia, Murphy worked with both Steward and Wagley (a student of Ruth Bunzel and Ralph Linton, and Marvin Harris's teacher as well). Given the Latin American interests of these professors, Murphy decided to do his initial fieldwork in South America among the Mundurucú Indians of Brazil (1952–1953), whom he describes as the most persuasive of all his teachers. The Mundurucú were the subject of Murphy's Ph.D. dissertation (completed in 1954) and two monographs, *Mundurucú Religion* (1958) and *Headhunter's Heritage* (1960). Beginning in 1953 he spent two years with Steward at the University of Illinois, one of several research associates Steward brought to

work with him there. Murphy's association with Steward in these years led to fieldwork among the Shoshone and Bannock Indians of Idaho and Wyoming (see Murphy and Murphy 1960). He also coauthored with Steward the "Tappers and Trappers" article (Murphy and Steward 1956) comparing Mundurucú and Algonkian responses to white contact, and he later coedited a collection of Steward's essays (Steward 1977).

In 1955 Murphy was appointed as an assistant professor at the University of California at Berkeley. There he came to know Robert Lowie, whose biography he later wrote (Murphy 1972). Among his Berkeley colleagues, David Schneider was the most important for him. In this period, Murphy carried out another major field project, among the Tuareg of Niger and Nigeria (1959–1960). In 1963 he returned to Columbia as a professor and remained a pivotal member of the anthropology department for twenty-seven years. He served as department chairman from 1969 to 1972.

Murphy's field research covered an extremely wide range of interests and areas, including societies in Africa and both North and South America. His work addressed a series of basic problems in ethnology, exploring a number of anthropological contradictions: The Mundurucú were patrilineal and matrilineal, the Tuareg were Muslim and matrilineal, the Shoshoni survived on the brink of starvation. At a time when theories of primitive society focused on cross-cousin marriage, he wrote an important analysis of the structural implications of parallel-cousin marriage (Murphy and Kasdan 1959). Murphy's theoretical orientation was (by his own description) "deliberately and happily eclectic," combining an early and persistent cultural-ecological bent, a strong dash of Freud (who first attracted his attention while he was an undergraduate), a middle period of structural-functional influence, and a later absorption with Lévi-Strauss—interests that "were cumulative, not substitutive" and that resulted in "an intellectual stew, well seasoned with Marx." The combination was expressed in his theoretical synthesis, *The Dialectics of Social Life: Alarms and Excursions in Anthropological Theory* (1971).

Murphy said that he thought of himself first as a teacher and second as an anthropologist, and that he did both because he liked to. He continued to do both into the last years of his life, when he was afflicted with an inoperable tumor of the spinal cord. Even as his malady worsened into quadriplegia, he scarcely slackened in his teaching and research supervision, assisted by a devoted coterie of his students. He remained the anthropological observer of his own experience and wrote analytically about it in a poignant book, *The Body Silent: The Different World of the Disabled* (1987). At the time that he died in 1990, he was preparing what was to be the Distinguished Lecture at the following year's meeting of the American Anthropological Association. He was planning (he wrote to a friend) to do "a number on the postmodern-deconstructive-reflexive-dialogic anthropology of our age."