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# Julian Steward and the Rise of Anthropological Theory

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FOOTNOTES TO THE HISTORY OF ANTHROPOLOGY

## JULIAN STEWARD AND THE RISE OF ANTHROPOLOGICAL THEORY

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Anthropologists have rarely had occasion to relate their work to the history of anthropology. Marvin Harris's book The Rise of Anthropological Theory presented Julian H. Steward with an occasion to do so. His congratulatory letter to Harris, reproduced below, can be taken as Steward's own account of the history of his anthropology. Steward's reminiscences of Kroeber and of the intellectually catalytic effect of his field work confirm two well-circulated anecdotes (Manners 1973:889; Steward and Murphy 1977:6). His dismissal of Carl Sauer eliminates one proposed influence on his thinking (Manners 1964:2). His acquaintance with Marx suggests that the "reinvention" of Marxist principles claimed for Steward merits more systematic scrutiny than it has yet received (Harris 1968:665; see Legros 1977).

The most interesting thing about this letter, though, is its tone. Until rather late in his career Steward saw great continuity in the history of American anthropology. To his mind his work represented "a diversification of the [Boas] tradition, not a break with it" (1955b: 323). But Steward was clearly impressed with Harris's argument that cultural ecology should be placed in opposition to the work of the early Boasians. That this book led him to "ponder some of [his] past statements" suggests that he now saw his work in this light. Indeed, at the very end of his life Steward noted that the "scientific" quality of historical particularism lay in "precise scholarship and constant empiricism rather than in a purpose or method" (1973:viii). Rather than stand as the final word on Steward and the Boasians, this should serve as a reminder of the extent to which the contrast between them may be construed as a retrospective imposition.

The following document (a carbon copy) may be found in Box 3 of the Julian H. Steward papers at the archive of the University Library of the University of Illinois at Urbana-Champaign (see Hanc 1979 for details). Titles are not highlighted in the Urbana draft. Underscoring for books and journals and the citation marks setting off articles have been added. All references to Steward's works (except Steward 1973 and 1977) have been keyed to the bibliography in Manners 1973. Steward's citations of Kroeber's work are amplified in Steward 1973:52. I would like to thank Jane C. Steward for permission to reproduce this letter.

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Fithian, Ill(inois)

March 8, 1969

Dear Marvin:

I just got hold of your monumental book and hasten to congratulate you on a terrific job, even before I have more than scanned it. You are more than generous to me and make me ponder some of my past statements.

I am writing, however, not only to congratulate you on developing the basics of anthropology but to tell you a little of the early background you could not have known, especially to give you a few asides to your section on Kroeber and Steward.

As a student of Kroeber and Lowie, my first year, 1925-6, was loaded with area courses which lacked anything nomothetic. Despite the Boasian orientation of Kroeber and Lowie, which I did not know at that time, I entered anthropology hoping to find a means of explaining cultural development. At the end of the first year I asked Kroeber when I would learn about explanation, upon which he said in some horror, "What do you mean? I deal with cultural phenomena, not explanations." Lowie was actually far more sympathetic to my interests, as shown by his support of my still unpublished thesis "The Ceremonial Buffoon of the American Indian" [1931b] which ventured reductionism in tracing recurrent themes of humor to inherent human psychological constants or potentials. Kroeber argued vigorously against my endeavor.

It was not until the early thirties, when I turned attention to primitive bands and did field work in the Great Basin that cultural ecology became an inescapable concept. It took years to sell this, however, and I well remember a long evening with Linton attempting to explain it only to be answered with "Environmental Determinism has long since been abandoned."

I was very much alone in my view during this period, and when the Handbook of South American Indians, 1946c] fell into my lap there was not a chance of organizing it in other than traditional area terms. In fact, this organization carried over into Native People of South America [1959c] far more than I recognized, and many teachers, I am told, have trouble with it as a text because students try to see it organized in evolutionary terms. I am trying to clear up this matter in a Festschrift article [1970b].

Of course I was overwhelmed by Kroeber's erudition and in my OB(ituary) [1962a] I tried to treat him kindly. My point about his anticipation of problems and even hypotheses is that he did again and again amass data only to stop short of drawing any conclusions. For example, in his "Primary and Secondary Features of Australian Social Organization" [possibly Kroeber 1938] the nature of his distinction implies causality. His several pages comparing Old and New World achievements in Anthropology [1948] lays out rather precise parallels but then stops. I think you would say of these and other cases that the pull of the Boasian relativism was too great.

Your speculation about how I got that way is only partly correct. First, I was interested in causes before I really got into anthropology, and was quite disturbed that Kroeber repudiated this interest. Second, the key factor of the national intellectual climate was the depression, which started after I finished my studies at Berkeley in 1928. I had taught at Michigan two years, 1928-30, and Utah 3 years (1930-33), by the time the depression became so acute that everyone was asking Why?, and thinking generally took a sharp Marxist turn. It was during the

thirties that Columbia became a communist cell far more than people knew, and, curiously, many adopted the political and economic orientations yet remained thorough-going relativists in their anthropological work. I too read Marx and others but it was dangerous to proclaim a Marxian po(s)ition.

Carl Sauer contributed nothing to my thinking. He has always been no more than an intellectual iconoclast, bent on baiting anthropologists, whatever their views. In fact, geography has never gotten off the ground intellectually.

I should add that I am still unhappy about evolutionism, mainly because it is still fraught with confusion. I did not think of myself as a cultural evolutionist until Kroeber suggested a paper on the subject for the Wenner-Gren World Conference of 1952 [1953d]. I accepted the designation but tried to adapt the concept. How many is "multi" [see Harris 1968:656] is unimportant, for two or more different lines is more than one. The important thing, as I have suggested in an article in Christian Century [1967b] is that qualitatively new forms emerge from old ones for potentially identifiable reasons.

Some time ago I abandoned Wittfogel's irrigation hypothesis. My review [1966] in Science of Adams' Urban Society [1966] was one statement on this. In an unpublished paper I have gone beyond Adams in attempting to formulate the preconditions in terms of closely placed and interrelated, interdependent microenvironments [1977].

Enough for now. I wish I had the chance to d(i)scuss this with you, especially some of the substantive applications. Your book cheers me up because the confusions of the New Anthropology, which seem to me to consist mostly of a jargon, are rather depressin(g).

Best

Julian H. Steward

Forgive the typing. A stroke a few years ago raise(d) hell with my coordination

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CLIO'S FANCY: DOCUMENTS TO PIQUE THE HISTORICAL IMAGINATION

SAPIR'S LAST TESTAMENT ON CULTURE AND PERSONALITY

Less than four months before his death in February, 1939, the brilliant American linguistic anthropologist Edward Sapir wrote what may be regarded as his last will and testament on the study of culture and personality--a subject to which he himself had contributed much of the fundamental theoretical groundwork over the preceding two decades. The occasion itself is indicative: then Sterling Professor at Yale, Sapir was responding to an unsolicited manuscript on culture and personality theory sent to him by a nineteen year old graduate of City College--the honors essay of Philip Selznick, now professor of Law and Sociology at the University of California, Berkeley. Sapir nevertheless took time for a considered response which expressed in a condensed and almost epigrammatic fashion viewpoints that might have gone into his never-finished book on "The Psychology of Culture." Although the methodological points were more extensively sketched (in some cases in very similar language) in an article published the preceding year in the American Journal of Sociology on "The Contribution of Psychiatry to an Understanding of Behavior in Society," the more informal context of the letter elicited reflections on related matters which are extremely suggestive. Sapir's comments on the unconscious psychological motivation of more extreme advocates of cultural relativity, as well as his thoughts on "the law of diminishing returns" in anthropology, may still today provoke both the historian's imagination and the anthropologist's self-reflective consciousness of the historical development of the discipline.

The letter is reproduced here (with the elision of one personal passage) by the kind permission of Professor Selznick and Professor J. David Sapir. (G.W.S.)