Schneider on Kluckhohn, 1964: Myth and Memory, the Oral and the Written, Historical Retrospect and Self-Representation in the Historiography of Modern American and British Anthropology

George W. Stocking Jr.

David Schneider
Dear Talcott:

Many thanks for sending me the ditto copy of your paper on Clyde and the Integration of the Social Sciences. I found it very interesting, of course, and I have a couple of comments on it. Some of these comments have to do with Clyde, some with your view of the history of the Social Sciences, and some of the comments may be irrelevant to both of these points.

On page 14 you recognize the very great importance that “language” and linguistics assumed both in general and for Clyde in particular; on page 15 you say, “it is interesting to speculate on the basis for the greater British emphasis on the ‘social’ aspects of Anthropology and for the greater American emphasis on its ‘cultural’ aspects.” On page 16, “Kluckhohn was certainly one of his generation of Americans who minimized the central significance of social organization rather much and were rather cold to the important contributions of a number of British structuralists in such fields as kinship.” Then on page 19 in your footnote you say “I find it somewhat difficult to understand why Kluckhohn’s attitude toward Malinowski’s contribution was so persistently negative.”

I think that some of these different points can be brought together very easily. First, Malinowski. If you recall Clyde’s obituary of Malinowski (published in the Journal of American Folklore), you will remember that Clyde in effect called Malinowski a “charlatan.” The history of Malinowski’s and A. R. Radcliffe-Brown’s relations with American Anthropology is a particularly dismal one. At one extreme [here follows the Malinowski anecdote]. At the other end there is a famous anecdote which tells of the great triumph which Linton announced when he was able to completely block the allocation of $2,000,000 of research funds which the Rockefeller Foundation had offered because Radcliffe-Brown would have had something to do with that money (the details of this anecdote are not entirely clear; Fred Eggan has documentation on it; whether the total amount was $2,000,000 I am not certain, but it was at that time a very large sum which the Rockefeller Foundation had offered to Anthropology because it had been deeply moved by the belief that if we did not all go out and study primitive tribes right now there would be none left to study within 50 years) [cf. Stocking 1995: 391-406]. I remember often hearing Clyde remark quite caustically that the only reason that Radcliffe-Brown was read was that he (Radcliffe-Brown) could read French and most other
Anthropologists could not. Clyde made the same sort of remark against Malinowski. Clyde told me a couple of times how much both had plagiarized from Durkheim without ever acknowledging it. Clyde felt, I believe, that Malinowski's whole notion of reciprocity had come straight from Durkheim and Mauss without a single acknowledgment while he had taken from Freud while publically denying any relevance to what Freud had written.

In certain respects at least most of Clyde's feelings against Malinowski, of a personal sort, were well grounded and not without foundation intellectually. The fact is that Malinowski borrowed, stole, and distorted material from Durkheim, Freud, and others and did in fact offer it on the intellectual market as his own. Your comment in the footnote on page 19, "It is interesting to note that Malinowski was a member of the Yale group in his last years and attempted to ground his theory of culture with the learning theory bridge. Though I personally do not really think this was really successful." As you may remember, I spent my first years of graduate study at Yale during the year that Malinowski was there. Your very straight sentences, therefore, seem particularly hollow to me. In Clyde's view, Malinowski was an adroit intellectual as well as personal parasite. I share this view.

In this connection [I offer] a single tangential note with reference to your page 19, the part before the footnote.

The significance of Yale, for Clyde, did not lie alone in the fact that his friend John Dollard was there. Nor that it also represented in the Institute for Human Relations a new and on the whole important trend with which Clyde himself was associated. It lay also, first, in the fact that Edward Sapir ran a very, very important seminar at Yale which Clyde attended and which also included Ruth Benedict, Margaret Mead, and a number of others. Over and over his common participation in this seminar (I do not remember the dates; I could perhaps locate them if it would be important to you) second, was a very special relationship which he had with Edward Sapir. Occasionally when Clyde had had a drink or two we students would sometimes accuse him of being "Judeaphilic"[---] that is to say, he shared a non-rational belief that there was something about Jews that made them superior to all other people. And in his cups Clyde would confess that this was so at least so far as Edward Sapir was concerned. Clyde had the deepest, warmest, and gravest affection for Sapir personally and the most profound admiration for Sapir's genius. Still another element in his relationship to Sapir (third) and therefore to Yale was the fact that Sapir had made some of the most important linguistic advances through the use of Navajo. Clyde spoke Navajo and the common interest which Clyde and Sapir had in Navajo, in linguistic structure, in problems of psychology and language, in problems of culture and personality all went far to binding them in many different ways. Let me add one more final note on this. Again, occasionally with Clyde in his cups, some of us heard him very bitterly assail the Yale Department of Anthropology and Yale in general for having, and I quote from memory, "killed Edward Sapir." Again you will note the peculiar Jewish note here. Clyde felt, I think, that Edward Sapir's fatal heart attack had been brought on by the fact that the Yale Faculty Club had explicitly denied membership in the Club on the ground that he was Jewish [cf. Darnell 1990:401]. You recall, of course, that Edward Sapir was at this time the Sterling Professor of Anthropology at Yale University[,] having come from the University of Chicago to assume this highest honor that Yale had to offer him. Here again, you will see the very special place which linguistics occupied in Clyde's life.

But to go back now to some of the problems of English Anthropology. As I say, the relationship between Radcliffe-Brown, Malinowski, and American Anthropology was on the whole extremely poor. Certain notable exceptions occurred, for instance here—Radcliffe-Brown and Fred Eggan were very close[,] but the situation at Chicago, insofar as I can understand it now, did not differ radically from the situation at Yale when I was a graduate student there and Malinowski had just come from England to assume the Sterling Professorship.

There is now a belief among Anthropologists that all Sterling Professors die early in office—Sapir held the Chair and died in it, Malinowski held the Chair and died in it, and Linton assumed that Chair and died in it—it has not been occupied by an Anthropologist since.
Murdock was Chairman of the Department at that time, having a three-cornered feud with Osgood and Spier (Spier went to New Mexico). Malinowski had the gravest public contempt for Murdock and exhibited it freely. Geoffrey Gore was at Yale at that time as a visitor, too, having gone in part at least to work with John Dollard.

The Anthropology practised in England was very highly regarded here. It was certainly read with interest and with care and I think that there can be no possibility that there was any serious or grave misunderstanding as to what the English Anthropologists were doing. Neither has there been any since. Although I have often said facetiously that the reason English Anthropologists are, on the whole, unaware of what Americans are doing is that it is written in a foreign language which is not ordinarily taught in English schools, and although it may very well be that much of English Anthropology loses something in its translation into American, the fact is that the only Anthropology which we seriously fail to read, (but which we should read) because of language difficulties remains the Anthropology from France.

So your page 15 troubles me very much. On the one hand, relations between some English anthropologists and many American anthropologists were extraordinarily strained personally. On the other hand, the work of the English anthropologists was seriously read, seriously considered and I believe quite clearly understood. It is certainly true that Radcliffe-Brown made a very sharp distinction between "culture" and "social structure," treating culture as having to do with ornaments, styles of hair-dress and skirt-length fashions (as Kroeber and Richardson studied and reported them). It is certainly true that Radcliffe-Brown claimed that he was studying "social structure" and that he formally defined this as the concrete network of actual social relations which existed in a tribe!! It is also true that Malinowski wrote a long and vivid paper for the Encyclopedia of the Social Sciences on "culture" in which he spent much time on interaction, the meeting of biological needs as functions of "culture."

I offer a simply "political" hypothesis: the word "culture" was used by Malinowski as his intellectual "trade mark" and respected as such by Radcliffe-Brown. The words "social structure" were used by Radcliffe-Brown as his intellectual trade-mark and respected as such by Malinowski. There was no love lost between these two, and their antipathy gained them strong allegiance and close followings. But one should certainly not confuse trade-marks with important analytic concepts. Any reasonably close study of the work of the "structuralists" shows, I think, that up to the point recently when Gluckman seized it and retreated full steam ahead, "social structure" was quite properly described as consisting of a potpourri of things, some of which was "social structure"[,] some "culture"[,] and mostly the largely undifferentiated treatment of primitive societies [cf. Stocking 1995: passim]

Only gradually in the work of Evans-Pritchard, Fortes, and then Leach and others, has the notion of "social structure" come to have been refined into what you (and many of us) regard as "culture." That is, Evans-Pritchard in his Zande Witchcraft, Magic and Oracles, in his classic The Nuer, in his Nuer Religion is more and more concerned with what the English sometimes denegatingly refer to as "ideology"—i.e., systems of norms, beliefs and values as they are stated in the norms for the composition of non-existent groups, norms for the understanding of non-existent supernatural forces, and so on.

And the point of central intellectual concern here is that Evans-Pritchard took his inspiration mainly from Levy-Bruhl, Durkheim, Mauss and much less from Radcliffe-Brown than has generally been supposed. At this time, today that is, the intellectual connection between Levi-Strauss, Lienhardt, Needham, Pocock, Evans-Pritchard, Leach and their intellectual progenitors Durkheim, Mauss, Levy-Bruhl, Granet is very close indeed, and clearly acknowledged to be so. On the other hand, Fortes stands firmly with Radcliffe-Brown and Malinowski, so close were they, while Gluckman reverts, as I said, to very early undifferentiated interaction and so feels close kinship with Margaret Mead. Firth has maintained his uncommitted, intellectually eclectic position unsullied throughout.

I repeat by picking up a different line. A piece of most important mythology which is widely believed is that American anthropology has to do mainly with the study of culture; English with the study of social structure, these
two being contrasted categories. This is explained, the myth states, by the fact that American anthropology lived on long dead Indian tribes, where the culture was recovered from the last surviving senile old man who remembered what his father had told him had been the state of affairs when the father had been a boy. English anthropology, studying "living peoples, and going concerns" (have you ever read the introduction to The Nuer and discovered how they had been "pacified" before E-P got there?) working in their own Colonys [sic], studies "whole live societies and their social structure." Evans-Pritchard's little book Social Anthropology states this in its nicest form even to an allusion to a school of American Anthropology called "the wind in the palm trees school" (Margaret Mead of course!). But this is all nonsense, except of course, as myth stands for the kinds of relations which American and English anthropologists have to each other. The exchange between Murdock and Firth in 1951 (American Anthropologist) makes the historical facts quite clear.

If one reads American anthropology and English anthropology with the simple guide "culture = social structure" one can read it quite as well as if one reads it "culture ≠ social structure." It is a matter of utter indifference to the sense of the works.

It is only very recently—within the last 10-15 years [—] that the culture-social structure distinction as an analytic distinction of self-consciously employed utility has made its appearance among some of the younger, theoretically more sophisticated anthropologists (Geertz, Fallers, Leach, Levi-Strauss, Dumont, etc.)

It is at this point that linguistics becomes relevant to the discussion again. You must remember, first, that David Aberle wrote a very good little paper for the Leslie White festschrift in which he described the role which linguistic theory and the study of language played in the development of culture-personality theory, This is an excellent and much neglected little paper. It has its counterpart in the summary paper which Dell Hymes wrote for the Transcultural Studies of Cognition recently issued by the American Anthropologist as a supplement.

For the fact is that from its earliest period, language as an "institution" has consistantly provided certain of the theoretical advances which anthropology has applied to other areas of culture. The early philologists (Grimm et al) complemented the Burkhardts; each dealt with symbol-value systems, these framed within sets of normative "rules" for their manipulation. Then a shot in the arm from the biologically oriented, the "human needs" people, and stagnation again. Now again linguistic theory—much of it developed in the 1920-30's (by Clyde[,]s much admired Roman Jacobson among others at that small, eastern sectarian university, Harvard).

The new surge in anthropological theory has been from structural linguistics via Levi-Strauss in France and England, and via Goodenough, Lounsbury and their followers, Conklin, Frake, Hammel, Metzger etc. The French-English read a few Americans, but not many. Levi Strauss was fully in prolific production by 1945-46.

Hence the problem at Harvard when I was there in 1951-55 over whether a structural linguist was or was not needed on the social relations staff was no mere gambit in a game of empire which Clyde was playing. Clyde believed and believed so on firm, well established evidence, that a constant source of theoretical innovation in what he took to be Anthropology's fundamental intellectual task, the study of culture, was from linguistics. It had been in the time of the early philologists, it continued to be during the reign of Culture-Personality theory, it remained so for his own intellectual hero, Edward Sapir—the master linguist and genius—. When Alex Inkeles walked around and kept saying "Why should social relations hire a linguist? Can't anthropology students learn languages in the field without having a course in linguistics?" he was just simply, wholly and abysmally off the point which Clyde was firmly committed to.

Language was in microcosm "culture" in its entirety. If the theory of language, and linguistic structure, could solve some of the problems of language, the same theory might well work on other parts of culture. And so Jacobson
showed, and Clyde read it and saw it. And when he came back from the Wenner-Gren conference Anthropology Today in New York in 1951 he said that there was only one person there who was really brilliant—who really had something important to say—and that was Levi-Strauss.

You put it correctly: Clyde's affinity was with linguistics and clinical psychology, and against sociology and British anthropology. He was against certain British anthropologists, but not against the anthropology which certain Britains practiced. I hope that is clear now.

But you are in error, I believe, in equating certain work in British Anthropology with that of sociology and contrasting this with the work of American anthropology. I hope I have made that clear now. Clyde's antipathy was to these few Englishmen, and this was not the ground from which it was "generalized" to sociology at all—simply because there is not the link you suggest, there is not the path along which it could be generalized.

May I repeat here and in confidence, I hope, another anecdote which I hope will make clear what I think is in part at work in this particular matter. Clyde had the deepest respect for and urged all of his students to read Evans-Pritchard's monumental work on Zande witchcraft. I think you will agree that this is certainly one of the landmarks in the study of Religion. But E.E. Evans-Pritchard himself was simply an unmitigated, unqualified, no good, son of a so and so. You remember that Clyde went to England once around 1951[,] and he had written ahead to Evans-Pritchard saying that he would like very much to visit him at Oxford, that he had been a Rhodes scholar at Oxford, and so on. E-P wrote back and said please come to tea at the Institute for Social Anthropology on such and such a day, at such and such a time. Clyde appeared at the appointed day at the appointed time and the secretary said that she was sorry that Professor E-P would be somewhat late, and would he please help himself to some tea. Fortes was there and at the end of the first hour quite uncomfortable, went to the phone, called E-P, came back looking quite perturbed, and said that although E-P had been at home, he had mumbled something incoherent apd it did not seem that he cared to come have tea with Clyde. Meyer Fortes[,] who has always at least been courteous[,] was very put out and very distraught at this obviously staged insult.

When I first came to Harvard in 1947, one of the first series of books that Clyde insisted that I sit down and read was Nadel['s] Black Byzantium and Evans-Pritchard's The Nuer along with Witchcraft, Oracles and Magic Among the Zande.

Quite distinct, however, from his relations with the "structuralists" were Clyde's very warm friendly relations with Raymond Firth and Audrey Richards from the London School of Economics.

Two problems remain here. First, why were Clyde's relations with Sociology in general luke warm at best, and secondly, why did he have such a passionate aversion to kinship and family as objects of intellectual inquiry.

On the second I will not hazard any guesses of a personal nature. I do not think, however, that one can overlook the fact that "the family" has always been one of Florence [Kluckhohn]'s central intellectual and professional concerns. I also remember very vividly one time having a discussion with Clyde on this subject. I had, you remember, "discovered" kinship for myself in my own fieldwork on Yap [cf. Bashkow 1991]. When I came back it was one of the central elements of my thesis. At one point prior to the completion of the thesis I had a talk with Clyde in which I tried to convince him (having just discovered it myself) that kinship like anything else had a cultural as well as a social aspect and that one need not confound the two or identify them and that in an important sense my thesis was concerned with the cultural aspects of kinship and had very little to do with the social aspects of kinship. I knew that Clyde hated ["]kinship" and I was trying to convince him that after all it was precisely the object that he himself said he was passionately interested in—namely culture. Having delivered what I thought was a perfectly rational, perfectly clear argument, I looked to see Clyde change his mind. (One of my own forms of pathology is to
believe that once people see the facts correctly they will then necessarily change their mind.) Clyde just looked up at me and said, "Of course, did you think I didn't know that?" And I was somewhat crushed since I thought I had brought to him a brand new insight.

On page 16, therefore, when you say that Clyde minimized the central significance of social organization I think that this statement covers too much. And when you say that he was rather cold to the important contributions of a number of British structuralists, I think that you are factually in error. I do not think Clyde missed these contributions at all. But he was certainly not given to loudly proclaiming the praise of individuals who had treated him personally quite badly.

It may be that Clyde’s relations to Sociology and the problems of quantification are not entirely unrelated. Certainly much of modern Sociology, particularly in America, has devoted itself to quantitative problems, scales, measurements, sampling, and statistics of all sorts. Equally certainly, as you will recall from Clyde’s very important paper on the “Kulturkreise Lehre” in the Anthropologist for about 193[1], Clyde’s distaste for quantification did not arise from his inability to handle these matters as it does with some of us. Clyde could handle a considerable sophistication of higher mathematics and when he was at the Center 1954-55, took a course that one of the fellows was offering there in higher mathematics which could be of use to Social Scientists. His ambivalence, therefore, to quantification, I suspect, had a great deal to do with the fact that quantification was so closely associated with Sociology.

Quantification was antithetical to another one of Clyde’s fundamental values and that was his insistence that Anthropology in its methodology centered upon the “clinical” relationship between the Anthropologist and informant or (as in the Benedict) his data. This was a relationship tied in with the notions of insight, understanding, empathy, etc. and this in turn is related to the fact that he saw culture as the central and major dominating problem of Anthropology. There was only one way in which the material and data that was necessary to understand or study culture could be obtained and that was by this “clinical” relationship between the student and the object of study.

Here then is still another link, I think, between Clyde’s affinity for personality and clinical psychology and Anthropology. I recall many, many times in the discussions of field work, the nature of field work and why field work was central to Anthropology as it was clearly not central to Sociology. Clyde’s insistence that the field experience was in itself a kind of psychoanalysis that the field experience was fundamentally revealing not only about the other culture but about the instrument of study i.e. the Anthropologist himself.

The fundamental problem of Anthropology for Clyde was the problem of culture. The fundamental methodology of Anthropology was field work and the field-clinical relationship between the Anthropologist and his informant.

Here then is another aspect to the problem of meaning and understanding which I think dominated Clyde’s view of the nature of Anthropology and most important in this connection with the nature of the difference between Anthropology and Sociology.

It should be unnecessary for me to go on in detail any more about what I recall to have been Clyde’s main commitments in Anthropology. Certainly, his concern with culture, and values as an aspect of culture became, as you say, the object of most intense concern for him. And for me the key phrases have always been ones that Clyde used, that is that culture is “patterns for behavior and patterns of behavior.” Although I frequently argued with him that patterns of behavior did not deal with exactly the same thing, he insisted that this remained to be seen and could not be omitted.
It is here, I think, that on page 28 when you quote Clyde's dissenting opinion [cf. Parsons & Shils 1951: 26-27] that I read into his words somewhat more than you do. I am not certain I am right in this matter, but I would suggest that there were two things involved in his ambivalence to the theoretical status of Sociology as a discipline. The first was his somewhat different interpretation of the biological element than appears readily in much of the Sociological literature. Here [on] page 18 the word you use is “de-emphasize” when you say “in this context it is not surprising that there has been an increasingly broad trend among Anthropologists to de-emphasize the biological especially the hereditary concerns of psychoanalysis in favor of a cultureology.” It is less, I think, a matter of de-emphasizing biological than of emphasizing the fundamentally biologically determined plasticity and open endedness that is and was for Clyde the central point of biology. I have a feeling that it is in this sense that Clyde was suggesting in the dissent which you [quote] “that there may have been a more clearly culturally patterned aspect to the interactive social processes” than you seemed to be willing to conceded. His dissent was, therefore, of the order of leaving this more in the nature of an open problem than anything else.

The second source, I think, of his dissent arises from first principles. I think that he felt, as others have felt, that if one takes action theory as one’s fundamental problem then culture becomes very much a question of its role in determining action. There are, however, other questions that can be asked about culture and in Kroeber’s famous phrase “the nature of culture,” than those entirely confined to its role in determining action. The nature of the patterns, ways in which patterns become determined, etc. are not immediately relevant as determinants of action and I suspect that Clyde felt that questions of this order and questions of the order that Kroeber continually raised along with those of Redfield and others were in some way slighted or in some sense treated as secondary. And in this some of his ambivalence toward Sociology is explained. To repeat and put it simply he felt that there were many Anthropological problems which Sociology as a theoretical discipline simply did not face or attempt to cope with and, therefore, his own relations with it were correspondingly uncertain.

Finally, of course, one should not neglect the fact that in the Department of Social Relations there was an unequal distribution of charisma. At times some people suspect that Clyde’s dissent was in fact aimed at precisely that problem. I know that I had a very long and very intense, slightly bitter[,] argument with Cliff Geertz in the middle of Harvard Square one day in 1953 or 1954 over this very problem. I think that I was able to convince him at that time that this did not explain anywhere near all of Clyde’s ambivalence with Sociology. And I would insist that it only accounts for, in fact, a small portion of it.

Let me try to summarize some of these points. Clyde was primarily committed to Anthropology in both its senses: as a discipline of certain intellectual questions, and as a profession. The history of that discipline was certainly closely related to the members of the profession. Clyde held Boas, Kroeber, Sapir and Redfield dear above other men. You do not mention these men and perhaps should. Clyde held these men dear for both their intellectual and their personal worth. The intellectual problem was seen as the study of culture—sometimes phrased as the study of man, and man’s distinctive species, and specific characteristic, culture.

In the history of both the discipline and the profession, linguistics and linguists and language played a central role. The linguists (the early philologists) had been among the first to state the symbolic, “patterns for behavior” aspects of culture—in the 1800’s, though often the late 1800’s. The source of much of “anthropological theory” had been and continues to be linguistics. Boas and his texts came long before Malinowski and his. And Boas and his texts now bridge to the next point, namely, that to study culture takes an empathetic, intimate, soul-shaking experience in the field in relationship with informants. One learns to think and see the world as “they” do. So the kinship Clyde felt for both linguistics, language, and psychology (clinical). Finally, to cap it off, there was the special relationship to a particular linguist, Edward Sapir, who within himself contained all of the “good” things—genius at linguistics, relating it to personality theory, master of Navajo as a substantive body of material which Clyde shared, and so on and so forth. So too the link with Yale.
May I add one, final criticism: you have all the elements to state Clyde's central intellectual problem but you do not put it clearly enough for me. The alignment of the study of culture with clinical psychology and linguistics makes sense, as I say, for the link they have through field work as the basic setting for the informant-anthropologist relationship as the methodological tool of Anthropology.

But it is precisely the confusion between the "systems" of culture and personality, rather than their systematic separation[, which wrecks so much of Clyde's work. His books with Leighton and his Navajo Witchcraft are all marred by this. It was only as he began to go back to structural linguistics, to formal analytic techniques and consider these as "cognitive" that this dilemma and confusion began to diminish. Focusing on values left too much "personality" kicking around; but he had begun to fight free of it I think. His death cut that development short.

In the copy I have, Schneider's text breaks off at this point, at the bottom of its tenth page, without any formal phrases of closing. There was, however, a two and a half page sequel (not reproduced here), on January 18, 1965, in which Schneider, responding to a letter from Parsons, backed off just a bit from certain formulations where "I think now I overstated the case"—but insisted that "so much of Clyde's intellectual work can best be seen and understood with reference to his personal situation—the object of Peabody's venom [i.e., the more traditional anthropologists in the Peabody Museum], the thin-skinned butt of E-P's snubs, needing desperately to have an intellectual identity and an intellectual commitment, but having much of it located in aspects of his own personal relations with Sapir." Parson's draft (of which I have no copy) is presumably an early version of an essay that eventually appeared in the 1973 Kluckhohn commemorative volume [Taylor et al. 1973] as "Clyde Kluckhohn and the Integration of Social Science." As published, it appears to have been substantially modified—although the only explicit reference to comments made by Schneider ("in personal correspondence") refers to a specific theoretical issue on which Parsons "was entirely ready to accede": namely, that "another basis of Kluckhohn's dissenting note [in Parsons & Shils 1951] was probably his conviction that the relevance of culture to the theory of action by no means exhausts its importance in human affairs." [Taylor et al. 1973:55] It is worth noting that the January 18th letter to Parsons was signed by Schneider as "Chairman" (of the Anthropology Department), and was typed by "pb," which suggests that both letters to Parsons may have been dictated or transcribed from a handwritten draft—and raising thus yet another question about the historiographical issues suggested in the title of this re-presentation, including especially the relation of oral and written sources.

References Cited


