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Anthropological Paradigms and Intellectual Personalities: Radcliffe-Brown and Lowie on the History of Ethnological Theory, 1938

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FOOTNOTES TO THE HISTORY OF ANTHROPOLOGY

Anthropological Paradigms and Intellectual Personalities: Radcliffe-Brown and Lowie on The History of Ethnological Theory, 1938.

In late December, 1937, the American Boasian anthropologist Robert Lowie published what was until that time (and remained so until the late 1960s) the only substantial English language history of what might be called today "socio-cultural" anthropology (Stocking 1966. Reflecting the usage of Franz Boas and the elder generation of his students, Lowie called his work The History of Ethnological Theory, and regarded it as his "most mature book, formulating the scientific position in ethnology" and "on the whole eminently fair and wise....". Many agreed, including Elsie Clews Parsons, who described it as "a model of fairness"-- save for Lowie's two brief references to Margaret Mead, once in a list of women anthropologists whom Boas had encouraged, and later as "strangely unaware" of previous efforts toward "systematic observation": "Our nonhistorically minded younger generation often rediscover America, and it is perhaps cruel to disturb their illusions" (1937:134, 275). Two leading figures who felt themselves misrepresented were the competing titans of British anthropology, Bronislaw Malinowski and A. R. Radcliffe-Brown. Malinowski, a kindred spirit with whom Lowie had for some years carried on a sometimes earthily colorful correspondence, took it in good humor, professing to have "hugely enjoyed and appreciated" the "wonderful caricature silhouette drawn of me" (quoted in Lowie 1959: 141). Radcliffe-Brown, however, was not amused. Over the preceding decade and a half of his peripatetic career, he had spent a great deal of scholarly energy insisting that there was a fundamental theoretical and historical distinction between "ethnology" and the "social anthropology" that he himself practiced and advocated (GS 1996:298-366). On May 6th, 1938, writing from All Souls College, Oxford, to which he had migrated from the University of Chicago the preceding fall, R-B took up his pen, and in eight pages of his characteristically neat and precise hand, wrote an irate, even outraged, letter to Lowie:

Dear Lowie,

The students of my seminar have asked me to explain how it is that you give such a distorted account of my views in your new book. So I have just read the section devoted to me and feel impelled to write and ask you two questions.

The first is why you suggest in a number of passages that I have recently changed my opinions on several points?

(1) page 237. "this... is now freely conceded by R.B." So far as I am aware my views on the nature of the unity (consistency or congruity of parts) of a social system are not in any important way different from what they were in 1909 or even 1905. Moreover the idea as I now hold it and as I have held it for a long time is not a new one but has been a commonplace of social science even since it was expressed by Montesquieu.

(2) page 227. "R-B now overtly recognizes study of the individual" [&] page 230. "R-B's recent inclusion of the individual as a legitimate object of enquiry". In the Andamans in 1906-8, as many people know, I carried out systematic investigations on individuals, having taken with me a full equipment for experimental psychology. It is true that I have not published and shall never publish my observations. They were part of a systematic

investigation of variability in physical characters (anthropometric measurements, hair, etc.), in physiological characters and in mental character (sense acuity, illusions, reaction-time, colour vision, emotional type, etc). I do not think that any anthropologist had at that date ever done what I did, namely collect and record the dreams of a number of individuals with the aim of determining variability of type. I gave a paper at Cambridge before I went to the Andamans on the study of individual variability in communities of different types and presented a sketch of my results in 1909. This particular part of my work was not relevant to the subject of my book and so was not included.

From 1901 when I began my four years work in a psychological laboratory and in a psychiatric clinic to 1934 when I was President of the Chicago Society for Individual Psychology, I have never ceased to be a psychologist. As such I think that a good deal of the work at present being done on individuals in non-European communities is thoroughly unscientific.

Moreover my students know that I have always taught (1) that the function of a social institution can only be seen in its effects in <u>individuals</u>. (2) that the only data in social anthropology are observations of acts of behaviour (including speech), of <u>individuals</u>, or products of such acts and (3) that culture is something that only exists <u>in an individual</u>. One of my criticisms of those who belong to the Boas tradition is that they talk about culture acting on individuals and individuals acting on culture, which seems to me pure nonsense. Individuals act on individuals and on materials. If anthropologists would only come down out of that Cloud-Cuckoo land where "cultural-traits" and "culture complexes" and "patterns" and "configurations" diffuse, move about, spread (whether on wings or feet I know not), act and interact, and realise that what they ought to deal with are actual human individuals and their relations I should hail that revolution with joy. It is one that I have been working for all my life, but I fear it will not come about.

(3) Page 227, line 4. "RB's <u>present</u> position.[RH 1937: especially his concept of function]" Again the implication, the suggestion, that I am a shifting or even shifty sort of person. In 1905 I wrote a long essay on the use of the concept of function in science (physiology, psychology, sociology). It was part of a work on scientific method which I was preparing as a fellowship thesis and which was never completed because I went to the Andamans. My views on function as then written are in all important respects identical with those written down thirty years later. There may be slight differences of phraseology. See, for instance the definition of social function on page 234 of the Andaman Islanders which was written about 1909.

(4) page 227 line 6 from bottom. "historical explorations are <u>now</u> regarded as complementary to those of the functionalist order." Again an innuendo that I have shifted my position.

Here I ask my second question. Why do you misrepresent my views by entirely excluding any reference to what I have written about ethnology? Since you seem to have read some of my writings[,] an impartial reader would certainly conclude that your misrepresentation is deliberate. I gather from reports that this is the impression that your book has made on some persons and that your action is attributed to personal spite and personal dislike. Your book proclaims itself as concerned with ethnology. You are perfectly aware that I regard ethnology as a <u>purely historical study</u> and that I distinguish it from sociology or social anthropology and that my views on this subject have been in print since 1923. You know also that the same distinction is made by a great number of other anthropologists not only in England and France but also in America (eg. Redfield). No one in England would dream of calling Balfour a social anthropologist or Frazer or Durkheim an ethnologist. [RL marginal note: Definition] You may yourself prefer to muddle together what others distinguish as ethnology, social anthropology and psychology and call the fantastic mixture ethnology, but in writing about other people you have absolutely no right to attribute to them the same love of muddle and ambiguity and to misrepresent their views by deliberately ignoring something which is fundamental in all their thinking, writing and teaching. If this is not dishonesty on your part, I wish you would tell me what it is.

I have taught ethnology as a history of peoples[,] including not only the history of 'culture' or civilisation but also racial history, archaeology, and the history of language for fifteen years in Cape Town, Sydney and Chicago. (Fred Eggan now carries on the Chicago course, Elkin the Sydney course and Schapera the Cape Town course). If you do not know this, you know singularly little of my work. In my case, you have only to turn to pages 407-494, of the Andaman Islanders to find a specifically ethnological treatment of the technical culture of the Andamans, with an exemplification of certain methodological considerations relating to historical reconstruction. Why should you ignore, apparently deliberately, the little that I have published on ethnology, and my own definite statement as to how I conceive the subject?

Page 228. The distinction between what I now call "synchronic" and "diachronic" sociological problems and laws was made in my Cambridge lectures in 1910, though I then used the terms "static" and "dynamic" which you will find in Rivers' 'Social Organization.' The distinction is of course a commonplace of European thought. See, for example Durkheim, Le Socialisme p. 141 and for the terms "synchronic" and "diachronic", de Saussure, Linguistique générale. But the making of diachronic generalizations is <u>not</u> history, it is science. It is part of sociology (social anthropology) not of ethnology.

If Elsie Claus Parsons was studying the early records of the Hopi in 1922, I was lecturing the same year on the history of the native tribes of South Africa. Why drag in this reference to Elsie as though plenty of people had not done that sort of thing before either of us and as though it was not the sort of thing that every competent and conscientious anthropologist would do as a matter of course if he was studying a native people?

In all these passages you employ the method of innuendo (by the use of the words <u>now</u>, <u>present</u>, <u>recent</u>) to imply that I have changed my opinions on important points. If this is what you intend people to think why not have the courage and decency to say so outright instead of relying on innuendos and then why not indicate at least in a footnote, some scrap of evidence?

In my lectures on Sociology (<u>not</u> Ethnology) in 1910 I expounded the views that I still hold on such matters as the classification of social types, evolution, functions, the unity of a social system, etc. Glance at the list of lectures [Stocking 1995:309-10]. My work in ethnology has always been carried on entirely independently.

I should like to believe that what you have written about me was written in good faith, but the evidence is against it. Why this extensive use of the method of innuendo? There are other examples. Eg. on p. 228 the implied suggestion that I had put forward the distinction between synchronic and diachronic problems as something new. You must have known that what you were suggesting was false. You knew also that one cannot reply to attacks of this sort when made in such a book.

The conclusion you draw seems to be that I do not differ very much from Boas and yourself except that my work is more restricted in scope. You are finding comfort in something that is patently false to everyone else except yourself. There is an absolutely fundamental point in which I differ from Boas and you and have always differed and shall always differ. In common with the great majority of scholars, at any rate in this country, I hold that history and science are different things. In ethnology I try to do history in the same way that reputable historians do it, and in social anthropology I try to do science in the way that physicists and biologists do it. You do neither but wish everyone to muddle history and science together, whereby they become pseudo-history and pseudo-science. Your antagonism to me seems to be due to the fact that I insist on a rigid scientific method in science and a rigid historical method in history. While I am sincerely sorry that this has made an enemy of you, I cannot cease to uphold what I believe to be the necessities of good scholarship.

Yours Sincerely
A.R. Radcliffe-Brown

Not one to back off from an intellectual challenge, Lowie picked up Radcliffe-Brown's gauntlet on May 24th, 1938, using marginal markings he had made on the text as the basis for a point to point response:

Dear Radcliffe-Brown: .

Your letter of May 6th requires an extended reply, for it voices some regrettable misunderstandings. However, I must thank you for your candor, which I shall try to requite in kind.

First of all, I hope you will disabuse "some persons" of the grotesque notion that my remarks are due to "personal spite or personal dislike." Nothing in our past relations warrants this odd assumption. I have always recognized your work on social organization, and your appreciative note about my Crow book was all that I could wish. At least once I mad efforts to lure you to our Summer School; and your willingness to take [W.Lloyd] Warner under your wing on my recommendation suggests some sense of common aims at the time. In short, I have no personal grievance whatsoever.

Ignoring, then, the gratuitous suggestions of bad faith with which your letter teems (and which may charitably be supposed to result from a temporary confusion of my identity with that of some other controversialist of yours), I shall answer your two queries and try to define the real nature of the difficulty.

(1) I used the terms "now,' "recent," etc. to express the idea that in my opinion you <u>had</u> changed your position as to the value of history (see your S. African address, 1923, p. 140f) and as to the logical status of the integralist view, which is now treated as an hypothesis rather than as axiom (BAAS, 1931, reprint p. 19)

Conceivably, minute textual criticism may reveal that I misinterpreted the intent of the relevant passages. What I cannot understand is why this has other than biographical importance <u>now</u>. What you overlook is the <u>commendatory</u> character of the pertinent comments. Since I have repeatedly called attention to my errors in print—e.g. in the matter of an instinct against incest or even in the description of Crow kinship terms, it does not readily occur to me that praise of open-mindedness could be construed as a charge of "shiftiness." There simply are <u>no</u> innuendoes: I ask that my statements be taken literally as they stand; proved errors in all of them will naturally be acknowledged as such.

Until receipt of your letter I had not fully realized the value you attach to inflexibility. It suggests a claim to congenital infallibility; I can extract no other meaning from your exposition.

(2) I have not willingly suppressed references to your ideas or work on "ethnology"; the very footnotes (pp. 223, 224) indicate where your definition can be found. Incidentally, unprinted essays, lectures, or data of yours obviously could not be considered.

My book early defines the universe of discourse; I was not bound by your or anyone else's definition of terms. Your views accordingly figure so far as t they seemed significant for the subject discussed. Your letter speaks of "the little that I (R.B.) have published" on "ethnology." Is it unfair to concentrate on aspects of your work that you yourself regard as more vital?

Having answered your questions, which I regard as petty, I proceed to expound my difficulties in coming to terms with you. Essentially they are twofold: (1) you refuse to admit agreement or even to credit others with elementary knowledge where it exists: and (2) you refuse to answer inconvenient questions, preferring even in your letter to lavish space on personal and terminological trivialities.

(1)

You make a great point about the use of such terms as "traits," "patterns," etc. If any followers of the "Boas tradition" believe that a bow is a "trait" existing apart from the maker or user, they should certainly be incarcerated in an asylum. As the term is commonly used, it is quite innocuous. Similarly, when I speak of a "pattern" for Crow visions, I do not say that the Platonic idea of a vision floats along the Little Big Horn and somehow lodges in a Crow brain. In short, I assent without ado to your proposition that culture exists only in individuals. However, a Crow visionary does not create the image of rocks turning into enemies and shooting his visitant out of his unaided consciousness, for the identical episode appears in myth and in the reports of other visionaries. This fact requires formulation, and in this context "pattern" has the instrumental value of other shorthand expressions. Durkheim, by the way, uses the concept, even though not the label, and I have given him credit for it.

Again: why do I drag in Parsons' study of the Hopi? Because, regardless of what she achieves, she is trying to determine "the process of cultural change." What, pray, is diachronic generalization if it is not that? What is Eggan trying to do that Elsie is not? I mention her simply as an example of your failure to credit others. Had you stated in print what you now state in your letters, viz. That "every competent and conscientious anthropologist" attempts something of the sort, there would have bee no need for emphasis.

The distinction between history and physical science has been familiar to me since my student days, when I read [Wilhelm] Windelband and [Heinrich] Rickert. The point has not the importance for anthropological practitioners which you attach to it. I can recognize no watertight compartments in the pursuit of knowledge. Some problems of culture require recourse to geography, for others we must consult history, psychology, even metallurgy if we are to understand the development of bronze. In the ordering of data our procedure likewise varies with the task: stratigraphy is a help in prehistory, not in the study of visions. Naturally, the procedure is different when I prove by critical examination of a source that in 1680 [Louis] Hennepin met a buffalo police party among the Santee and when I compare various police organizations of the Plains tribes. (Not that I recognize such comparison as equivalent to the physicist's procedure, see below).

Since the investigator is one person, not usually a split personality, I am not clear what you would like him to do. My instinct is to get what I can from the phenomenon studies that might add to my understanding. See below.

(2)

You loftily decline to explain why your undocumented reconstruction of Australian history is true history in contrast to Kroeber or my "pseudo-history. Worse, you evade discussion--even in your letter—of my persistent query (p. 224 and in "Queries") of why you claim to do "scientific" work in the manner of the physicist. Whether you have demonstrated a law comparable, I will not say to Newton's, but the simple rule

pv=constant [i.e.Boyle's Law: pressure x volume=a constant]]

is a crucial matter. The personal and scholastic issues of textual criticism are trivial in comparison. Do you thrust them forward because you really assign greater importance to them? In that case, discussion between us is indeed futile.

I do not feel that you have expressed the significant differences between us and shall accordingly formulate them from my point of view.

(1) You approach culture with a ready-made set of principles designed to work miracles. You resemble the literary critic who declared he could write better dramas than the French classics merely by applying Aristotle's canons. This seems to me a vicious and naive procedure.

I try to saturate myself with those facts that attract me and then get what enlightenment I can concerning them. I do not import the principles of mechanics or of any other extraneous discipline, but merely follow the rules of logic that characterize all scholarly endeavor, adapting them to the specific requirements of the problem.

(2) You enunciate universal laws, which seem to me blatant bombast.

On the other hand, I appreciate some of your correlations as sound and hope for more of them.

(3) You recognize a single mode of scientific collaboration—that of master and disciple.

I have no disciples and want none. I am not a "leader" and I do not want my students to be led by the nose. Our present group in California are largely "functionalists" at heart, and we permit them to tear our "pseudo-historical" reconstructions to pieces in our seminars. This does not affect our personal relations with them.

As for older colleagues, I try to understand them, but attach no importance to agreement or disagreement. My book contains laudatory references to Tylor, Maine, Morgan, Boas, [Eduard] Hahn, Durkheim, [Wilhelm] Schmidt, [Richard] Thurnwald, Malinowski, and yourself. The appreciation is, however, invariably tempered with reservations, which naturally are more pronounced in some cases than in others.

Your assumption that I approve only of Boas, or wholly of him, would be dispelled by a glance at my book. Actually I have not even been especially close to him: Harlan I. Smith, Wm. Jones, Tozzer, Goldenweiser, Sapir, [Hermann] Haeberlin, [Pliny] Goddard, Ruth Benedict—representing different periods—have been, or are, all much closer to Boas.

(4) You have a gospel to proclaim; I make it clear to any students who seek inexpensive solutions for the riddles of the cultural universe that I do not hawk such commodities.

Functionally correlated with this difference and that noted above is my aversion from any claim to infallibility. Further, I do not conceive scientific work as an adolescent's game for individual aggrandizement, but a cooperative effort that gives scope to many diverse talents and temperaments. Neither in my book nor in this letter am I at all concerned about "scoring" against you: I am interested in separating dross from gold for a common exchequer.

Having reread the pages devoted to you in my book, I suggest submitting them to some friendly layman remote from the scene of anthropological feuds. Such a reader will not, I predict, gather that you have "made an enemy" of me. Malice does not refer to its victim as doing some "exemplary" or "brilliant" work, nor does it express "genuine respect." I do not retract any of these laudatory statements under the provocation of your letter; they were as sincere as my repudiation of what I brand as pretentious claptrap. The friendly layman will probably infer that you crave the servile adulation of henchmen, not the discriminating appreciation of peers, which to me is the only desirable form of recognition from fellow-workers.

Even this attitude, however, though thoroughly repugnant to me, does not in any sense make me your enemy. I meet it with another shrug at the quaint idiosyncrasies within the probability curve of anthropological mentalities.

Sincerely yours,
[Robert H. Lowie]

Although Lowie rejected the charge that his treatment of Radcliffe-Brown's work reflected "personal spite or personal dislike," there was a certain hint of relish in his retrospective comments on the exchange in 1959, when he recalled Daryll Forde's congratulations that he had "deflated" R-B's "pretentiousness," and quoted Malinowski's remark that R-B's "pedestal shook under the blows of the chisel." (RL 1959: 141). From Lowie's point of view, however, the issue was not "personal dislike." His history was frankly a critical (one might also say "presentist") history, written to "indicate the course of theoretical progress" (1937:vii). It was full of evaluations, both negative and positive, in which what might be called "intellectual personality" was frequently a matter of comment. John McLennan's was "a vigorous intellect," "too enamored with his reasoning to entertain rival hypotheses" (44, 48). Henry Maine, by contrast, was "the embodiment of serene wisdom coupled with unusual subtlety"—"astonishing" in his day "for a thoroughgoing historical-mindedness" (50-51). Although John Lubbock was largely dismissed for his ethnocentric "egocentrism" and his "abandonment of objective criteria" (24-25), in most cases Lowie struck a balance. If Lewis Henry Morgan"s "amazing inferences" illustrated his deficiency in "historical sensitiveness," his empirically based study of kinship systems was nevertheless a "colossal" achievement (61, 63).

From Lowie's point of view, his treatment of Radcliffe-Brown, although critical, was also "balanced." Suggesting the R-B was not "a field man by temperament," but "at heart an armchair anthropologist," he divided R-B's work into two categories: "a series of professions of faith outlining the proper procedure of anthropological research" (notably, his "grandiloquent use of the term 'law'"), and "his actual contributions," which in the field of Australian social organization had led "to a brilliant discovery" (the correlation of two types of kinship nomenclature with two types of cross-cousin marriage). The former he greeted "with a shrug; the latter, "with genuine respect" (1937:221, 225, 229).

While many might accept Lowie's notion of balanced critical evaluation as a scholarly ideal, some of them might also chafe at its application to their own work. Others, more systematically inclined, might object (directly or by implication) to the very notion itself, particularly when it was applied to their own work. Radcliffe-Brown fell among the latter. Although the now no longer fashionable but still provocative term "paradigm" had not at the time of the exchange been given the meaning/s it was subsequently to have in Thomas Kuhn's Structure of Scientific Revolutions, it is clear that Radcliffe-Brown thought of his own work as paradigmatic (Kuhn 1962, 1977; Masterman 1970). He sought to establish a scientific community within anthropology, the members of which would share assumptions about the character and goal of their inquiry, the empirical phenomena relevant to its pursuit, and the concepts in terms of which those phenomena would be investigated—all of this in sharp contradistinction to the orientations of others who might mistakenly consider themselves or be considered as involved in the same inquiry. In establishing these boundaries, naming and definition were of critical importance: what R-B did was social anthropology, not ethnology, it was scientific, not historical, it dealt with social structure, not culture. From his point of view, simply to have been included in a book entitled The History of Ethnological Theory was a challenge, the more so since Lowie (without noting a terminological slippage) spoke of "Ethnography [sic] as the science which deals with the 'cultures' of human groups." (1937:3).

In defending the boundaries of his presumptive paradigm to Lowie, Radcliffe-Brown appealed to his own intellectual autobiography to apparently contrary ends. On the one hand he insisted that his fundamental point of view had been defined very early in his career (and had in fact a genealogy extending back to the Enlightenment), and indignantly rejected Lowie's "innuendo" that he had recently changed his position on important points. On the other hand, he acknowledged, indeed insisted, on his own personal experience in the fields on the other side of the boundary lines; ethnology, physical anthropology, and psychology were not terra incognita, but lands he had personally explored. What he was less inclined to acknowledge was that some of the boundaries on which he insisted had been demarcated only in the course of his career, in tension if not controversy with the anthropological orientations of important significant others, including Rivers, Malinowski, and the Boasian anthropologists. He had waited until 1923, after Rivers was dead, to publicly insist on the boundary between "ethnology" and "social anthropology", and it was only in the 1930s that his differences with Malinowski over the concept "function" were clearly articulated Separating himself from the Boasians was especially problematic (Stocking 1974), and his transition from an early cultural psychology project (still evident in the second edition of the Andaman Islanders) to the renunciation of the possibility of a "science of culture" was not fully marked until 1937 in the seminar he gave at Chicago later published as A Natural Science of Society (R-B 1932, 1956; Stocking 1995: 331-32, 359)

The problem with the younger Boasians was that they did not take boundaries with the paradigmatic seriousness Radcliffe-Brown required. There were some who clearly saw themselves as workers in his prospective scientific community—notably Lloyd Warner, who spoke of his various projects as "cases" for R-B's comparative science of society. But at Chicago, Sol Tax and Fred Eggan, both of whom were strongly influenced by R-B, remained in important respects fundamentally Boasian. So also did Margaret Mead, though she had been greatly stimulated by R-B's lectures at Columbia in the summer of 1931 (HAN 20 #2: 3-11) Others, like Alexander Lesser, attempted to build bridges between the "historical" and the "functional schools" by advocating a "functional historicity" (Stocking 1995: 355-56). In this context of blurring boundaries, the task of boundary drawing was made all the harder by the fact that Lowie was in many respects the Boasian elder closest to R-B's project (notably in his long term interest in social organization), and was not himself sympathetic to the extreme cultural determinism of R-B's imagined Boasian "cloud-cuckoo land." But precisely because it came from a leading elder of the Boasian tradition, Lowie's more pragmatic approach to issues of method and theory was all the more threatening to Radcliffe-Brown's paradigmatic project. To imply that the only difference between the Boasian project and his own was that "my work is more restricted in scope" was in fact to call into question all that R-B had been seeking to accomplish at least since 1923: to establish social anthropology as clearly demarcated disciplinary paradigm, in which there would be a clear distinction between a "rigid scientific method and a rigid historical method."

All of the interpretive possibilities of this exchange cannot be explored—or even touched on--in this brief commentary. However, there is one further aspect of the paradigm analogy worthy of comment. Aside from the likenesses, there are differences between Radcliffe-Brown's paradigmatic project and the paradigm/s concept as it was enunciated by Kuhn five years after R-B's death, and debated for several decades thereafter. In that discussion, the concept of "paradigm" was often thought of (and critiqued) as relativistic (albeit to Kuhn's occasional intellectual discomfort); lurking in the background was the more general question of whether the new historicity of science could be thought of in terms of "progress." From this point of view, it is worth noting the limits of relativism and historicity in Radcliffe-Brown's project. While critical aspects of the social anthropological project could be traced to the Enlightenment, the boundaries between social anthropology and ethnology were not clearly articulated prior to R-B's paper of 1923. Once enunciated, however, they were relativist only in the sense that the methods of ethnology and social anthropology might each be appropriate to their separate purposes. Once established, the distinction between them was timeless, and the boundary between them had to be rigorously maintained. And although R-B's letter did not speak of the "progress of anthropological science," it seems a plausible inference that such a motivating assumption lurked in the background of his project. From this point of view, however, the difference between the two men was not that great, since Lowie, as his "balanced evaluations" suggested, was also (in his own eclectic and flexible pragmatic way) a believer in something that might be called "scientific progress."

With the issue of temporal process in mind, one might ask, sixty-five years on, how the two have been and are perceived in the disciplinary memory since the time of their exchange. One very rough indicator is the number of page references to each in the series of review volumes that followed in the wake of Anthropology Today, the disciplinary "inventory" symposium held in New York in 1952. These included the follow-up volume entitled Current Anthropology [CA] of 1956, the Biennial Review[s] of Anthropology [BRA] published from 1959-1971, and its still continuing successor series, the Annual Review of Anthropology [ARA], which began appearing in 1972. In CA, which appeared while both disciplinary elder statesmen were still alive, the ratio is almost the same: 21 references for R-B, 22 for Lowie. By the time the first volume of BRA appeared in 1959, "culture history" was excluded as a topical category, and there was only one reference to Lowie, while "social organization" was one of the four "core chapters," in which R-B was referred to on 12 pages; by the time the BRA ceased publication in 1971, the ratio was 27 to 1. Over the first three decades of the ARA, the ratios were 13/10, 16/7, and 13/4—which might be taken to suggest that paradigmatic presumption is a better buffer than "balanced evaluation" against the vicissitudes of disciplinary fashion. On the other hand, it is worth noting that in a retrospective view of "British Social Anthropology" published in ARA for the year 2000 (in which Radcliffe-Brown is referred to only once), the boundary between British "social" and American "cultural" anthropology is one of several "formerly crucial boundaries" that have "withered away" over the last thirty years. What remains distinctive in British anthropology is instead traced institutionally (rather than intellectually) to the seminar tradition established by

Radcliffe-Brown's antagonist (and Lowie's kindred spirit), Bronislaw Malinowski (Spencer 2000)

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Bibliography

Kuhn, T. 1962. The structure of scientific revolutions. University of Chicago Press.

_______ 1977. Second thoughts on paradigms In The essential tension, 293-319. University of Chicago Press.

Lowie, R. 1937. The history of ethnological theory. New York

_______ 1959. Robert H. Lowie, ethnologist: A personal record. Berkeley, CA.

Masterman, M. 1970. The nature of a paradigm. In I. Lakatos & A. Musgrave, Criticism and the growth of knowledge, 59-90. Cambridge University Press.

Radcliffe-Brown. 1923. The methods of ethnology and social anthropology. In Method in social anthropology, 3-38. University of Chicago Press (1958).

Spencer, Jonathan. 2000. "British social anthropology: A retrospective" Annual Review of Anthropology 29:1-24.

Stocking, G. W. 1966. The history of anthropology: Where, whence, whither? Journal of the History of the Behavioral Sciences 2: 281-90.

___________ 1974. The basic assumptions of Boasian anthropology. In Delimiting anthropology: Occasional essays and reflections, 24-48. (Madison, Wis, 2001)

___________ 1995. After Tylor: British social anthropology, 1888-1951. Madison, Wis.

SOURCES FOR THE HISTORY OF ANTHROPOLOGY

Pamela Jane Smith, with support from the Wenner-Gren Foundation Oral History Project, has carried on interviews with more than two dozen leading figures in African archaeology from the early 20th century "fathers" (the late Desmond Clark, Thurstan Shaw, and Peter Shinnie) through the intermediate generation of pioneer African Africanists to the present leaders represented by Bayo Folorunso and Innocent Pikirayi. The interviews and transcripts are stored at the Society of Antiquaries of London, Burlington House, Piccadilly, London, WIJ OBE. Four hundred pages of transcripts are also at Cambridge University's Haddon Library. Copies are available from Pamela Jane Smith, Lucy Cavendish College, Cambridge University, Cambridge, England, CB3 0BU (pjs1001@cam.ac.uk)