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FOOTNOTES FOR THE HISTORY OF ANTHROPOLOGY

Margaret Mead and Radcliffe-Brown: Society, Social System, Cultural Character, and the Idea of Culture, 1931-35

In the summer of 1931, during the transitional period between his appointments at Sydney and Chicago, A. R. Radcliffe-Brown spent some time in New York, where he consulted with Rockefeller Foundation academic bureaucrats about how best to salvage the ethnographic data on which to build what at that point he still spoke of as a "science of culture" (GS 1985). While in New York, he gave a series of twenty-nine lectures at Columbia University, in which he presented an overview of his theoretical and methodological approach to social anthropology. First employed at Cambridge in 1910, the lecture series was a format that Radcliffe-Brown found extremely congenial. The various formulations of his evolving viewpoint are preserved here and there in notes taken by students, including those of Isaac Schapera at Cape Town, and Sol Tax and several others at Chicago (GS 1985).

Among those present in the audience for the Columbia series was Margaret Mead. It was not the first time she had come directly under the influence of Radcliffe-Brown. They had met previously in Sydney, after her marriage to Reo Fortune, when she and Fortune were on their way to fieldwork in Manus--a site which Radcliffe-Brown had in fact proposed. According to Mead's much later reminiscence, Radcliffe-Brown--"always indolent and good at improvising"--had "not planned to work very hard" that summer. But she and Reo "took his course, sat in the front row, and expecting the best, we got it" (1972:193). They spent time with him outside of class as well: "in the evenings I wore my prettiest dresses and prepared the kind of dinners Radcliffe-Brown enjoyed." And apparently he did: that November, when he had arrived in Chicago and they were departing for more fieldwork in Melanesia, he wrote to thank "you and Reo for all your kind hospitality," which made "very pleasant what would otherwise, I think, have been a rather tedious six weeks."

The notes Mead took of Radcliffe-Brown's lectures are preserved in her papers in the Library of Congress, along with the correspondence between them. At the time, however, she also prepared for Ruth Benedict, her teacher at Barnard and then her close personal and intellectual friend, a two page summary of "Radcliffe-Brown's Point of View as I see it," the text of which follows:

He calls himself a comparative sociologist. Comparative because he believes no conclusion based on the knowledge of one society can possibly be valid, sociologist, because he wishes to generalize, not about the conjectural history of primitive societies--this he defines as ethnology--nor about the whole problem of humanity, which the word anthropologist implies, but simply about the nature of society. He regards the structure of society as the most important point for study, but feels that this can be studied best only when the function of any particular part of the structure of a given society is known. He regards no two societies as identical in structure: different structures may perform similar but not the same functions, the similar structures which perform different functions may not be regarded as the same structures. He argues the advisability of attempting to generalize about the nature of society--regarding culture as an adaptive mechanism by which the human race has survived, a mechanism which has been substituted for biological evolution--from the fruitfulness of generalizations in the other sciences. He admits that history can never generalize but can, if all the facts are known, explain. He sees clearly the issue as to whether social anthropology is to be a historical or a generalizing science. He does not allow that there is anything valid or useful in conjectural reconstructions of the pasts

of unlettered peoples, and puts such work on a par with conjectural constructions dealing with the future. He does not claim to have found any general laws, but to be merely advancing tentative hypotheses, which must be tested out in the field. One of the hypotheses with which he deals is that for every society there is an optimum state, given its existing structure. This optimum state--which is simply a matter of smoothly functioning mechanism, not involving any values such as the happiness of the individual--he calls a state of euphoria, or well being. Events, differing for each society, occur which produce a state of disequilibrium or disphoria. To this state of disphoria the society reacts, makes some sort of adjustment. It is from this point of view that he discusses private vengeance, organised vengeance, and warfare, as a sequence on different levels of social integration, in which the normal stability of a group is disturbed by another group, disphoria results and vengeance or warfare is the reaction. He discusses punishment as the way in which an affronted society is restored to a state of equilibrium again after having punished the criminal. The function of the punishment of crime--its general function--would thus be to keep alive in the society the sentiments against which the criminal had offended and restore the euphoria which the criminal act had dispelled. Obviously in a complex modern society, the punishment of crime has become so ramified and complex that this function is often not fulfilled and many other functions may be. But this treatment of the subject of crime illustrates Brown's method of dealing with elementary forms. He believes in the sort of evolution which no one objects to, i.e. that societies were once simple and some have become complex, sometimes this history has repeated itself, sometimes not. He discusses this evolution primarily from the standpoint of what he calls levels of integration, or the size of the social group. On this basis the Andamanese and the Australians come at the bottom, so to speak, but need not necessarily ever have worked into larger units. But throughout societies at these different levels of integration, many of the same structural relationships occur (There is the universal pattern, as the Americanists would say.) Brown says, study the simplest forms, not for origins but for clarity in understanding how they work. This assumes of course that society does work, or rather that societies do, that they must have a certain minimal stability to function well enough to keep their members alive and reproducing. Brown says, we will find what this minimal stability is, and how the structures which support it work. He claims that no one society can be understood by itself, but only when a large number of varieties of the same type have been studied. This will give one a norm for that type; by type he means Australian, or Polynesian or Eskimo, not societies with similar economic base or social form, but the type of society which has developed within a given culture area. In many ways he draws more from ~~Ratzel~~ Frobenius than from anyone else. Only for ~~Ratzel's~~ Frobenius' elementar gedanken he substituted elementary social forms, forms common to all culture, not to the human mind.

It seems to me an excellent approach. I can't see where it leads in anyway to a mutilation or distortion of the facts, to ignoring of important points. It avoids the danger of resting on the explanation that they just do it that way, and insists on looking further. It is doubtful, as Brown admits freely now, whether this method can be very fruitfully applied in North America. It is too late for field work and the reported material looks too complicated by historical cross currents to yield anything like the results which can be obtained in cultures which have had time to shake down their borrowings.

He thinks that it will be possible to find very definite correlations between levels of integration and social structure, that for instance organized vengeance is only compatible with a certain rather rudimentary political organisation, and that if organised vengeance--as an institution--becomes too firmly entrenched and ramified--then the further political integration of that society will be

obstructed. Similarly, a loose organisation without even clan lines can deal with only a certain sized population, but there comes a point at which clan organisation may [also] become an obstruction. Also, strengths of societies may be expressed in terms of the unity of their structural emphases--in S[outh] Africa, the political system was an integral part of the kinship system--as was also the religion--so the chief was the oldest son of the eldest son of the founder of the tribe--as such he was also chief priest of the tribe. But in East Africa, the political kinships were superimposed on the kinship organization without real unity, and so crumbled, but left the kinship organisation untouched.

He is primarily interested in social structure, and views the rest of society--i.e., art, religion, etc., from the standpoint of the structure. While this may leave untouched special problems within these fields, it does not rule them out by any too narrow definition.

He admits that at present we don't know what euphoria is nor what disphoria is, we are simply using these concepts as working hypotheses as a way of studying the problems: What is the form of this society? how does each institution within it function to preserve that form?

It was during the same summer that Mead also completed her "technical monograph" on Manus kinship, Kinship in the Admiralty Islands (1934; cf. 1972:193). Although it contained more footnotes to the work of Malinowski, the debt to Radcliffe-Brown was explicit in the preface: "I have to thank Professor A. R. Radcliffe Brown [sic] and Professor Ruth Benedict for most illuminating and stimulating assistance in the understanding of the general problems of kinship and social organization, and particularly, for a criticism of this manuscript" (1934:184)

Mead and Radcliffe-Brown (or R-B, as he came to be called during his Chicago years) continued to correspond sporadically through the 1930s. The most interesting exchange took place in 1935, after conversations Mead had with R-B while she was lecturing at Chicago. Then at work on the study of "cooperation and competition among primitive peoples," Mead raised a question regarding the definition of a "society"--or as R-B suggested, a "corporate group." Responding in a long undated letter, R-B suggested that it was a problem that he himself "found very difficult":

I do not think that we should say that the simplest peoples have no society but that they have no corporate society. Perhaps we can say that the two essentials for a corporate group of any kind are (1) capacity for corporate action as a group (action of all members or through representatives) and (2) continuity of existence over a period such that at any moment it is possible to distinguish who is and who is not a member of the group. I think that changes of membership, by which an individual leaves one group and joins another (as in the instance of the Manus villages), even if such changes occur frequently or in considerable numbers, are not to be regarded as destroying the corporate nature of the group.

Although he granted that there were "difficulties about this definition" (e.g., the problem of "factions and parties" among North American tribes), R-B went on to suggest that what Mead had called "a society" might better be called a "political society"--that is, a corporate group with "territorial continuity," as well as corporate action in "maintaining its rights against invasion from outside" and in "exercising control over its own members." Such a society might or might not coincide with linguistic or cultural groupings.

Characterizing as "excellent" Mead's distinction between those who studied "culture" and those who studied "society," R-B thought he might "go a step farther":

I prefer to say that the entities with which I am concerned are 'social systems'. A system here, as in physics or chemistry or physiology, is a complex of a number of components in relations of mutual interdependence, which, for purposes of study, is susceptible of relative isolation (experimental or conceptual). A social system (such as that of an Australian tribe) consists of (a) individual human beings with their acts and states of mind (b) material objects involved in human interests or activities (c) a set of relations between individuals (the social structure) (d) relations of individuals to material things. The whole system is described in terms of social usages which are descriptive generalisations (i.e. abstractions) about the usual behaviour of individuals, and it is, of course, only in the behaviour of individuals that we can observe their relations to other individuals and to material things.

What he was trying to do was "system analysis"--as exemplified by Walter Cannon's discussion of the homeostasis of the bloodstream in his "Wisdom of the Body." Such analysis might also be applied to a particular aspect of a social system; indeed, Mead's account of Manus kinship was "an excellent piece of system analysis":

You deal with the behaviour of individuals as individuals, you deal with the social usages, which are uniformities observable in such behaviour (culture, I take it, is the sum total of social usages of a given time and place) and you deal with the social structure (the totality of relations of individuals) and you show all of these in a set of relations of mutual interdependence.

Having found a common ground, R-B went on to mark a boundary:

For me the most important thing in science is to study realities (real objects and real events) and to talk about them in terms of abstractions but not to treat abstractions as though they were realities and say we are studying them.

Individuals, their actions, and the relations between them were realities; a "culture-trait" was an abstraction, and "culture" was "an abstraction from abstractions"--though many anthropologists treated cultures as though they were "existent realities." Specifically, he suggested that "nowadays," readers of Ruth Benedict were beginning to treat "configurations" and "patterns" as if they were realities. In contrast, his "approval" of Mead's work was due to the fact that it was "scientific"--that it kept "close to existent realities"; his "admiration" was "due to the fact that it is also excellently done."

Ignoring the challenge to a cultural viewpoint she had in fact helped to form, Mead responded on July 1, 1935:

Thanks so much for your long letter. It is a big help. I didn't get it in time to incorporate your definition in the summary of the Competition and Cooperation study so you will have to tolerate a less precise definition there. I think it is more useful to speak of a political society, and then the less organized form as a corporate society. But I am curious still as to where you would classify the Eskimo. It hardly seems to me that "the group of people who happen to be wintering this year at a given spot" can even be called a corporate society, let alone a political one. Has it laws of behaviour which are as well stated as forms of the behaviour of the constituent individuals? Anyway, when you get the C and C study, perhaps you'll remember this point and

tell me what you think should be said, about Eskimo and Ojibwa particularly.

I wish that I were going to see you in the next few months. There are so many points which we could thrash out. When I left [Chicago] in January you phrased it, I think, that I had said that the laws of functional consistency in individuals and in societies were the same, and that you would think it over. I do not think that is what I want to say. I think I want to say that the laws of functional consistency of an individual's psychology and of a given culture are of the same order. It is the point that you can [say] that a culture has a built up system of ideas which are similar to the constructs of the paranoiac, but you can not say that a society is paranoid. Could we say that society may be likened to an organism, but not to a personality? I found in talking with Gregory this time, that he seems to depend upon the lower animal forms, forms which make colonies, etc. for his analogies between society and an organism and that keeps the point clear of any personality or mental content issue.

Then I would say that each society uses a method of education, of character formation, which is more or less congruent with (a) those emphases in the culture which may be regarded as institutionalizations of individual traits, and (b) the social structure, using the social structure in its widest sense. Then we can look at the functioning of any given society as dependent upon the degree of agreement between these mutually dependent but not absolutely correlated systems, which together go to make up the part of the social system which, together with the environment and material things, make up the total system which you designate as your unit of study. In North America we have for instance, (a) a premium upon personal religious experience (excluding the Pueblos of course) (b) a democratic social structure which lacks ideas of absolutism, and of a state controlled army, and of a separate judicial system, etc. [(c)] a system of education which results in the development of an enormously strong sense of shame. The confluence of these sub-systems gives the peculiar character to North American social systems, but no one of these three necessarily, I think, presupposes the other. One can imagine a democratic society which was controlled by fear rather than shame, and both existing without the emphasis upon individual religious experience. I don't think that in isolating these three aspects, I have made anything like an exhaustive analysis; but I do think the isolating has to proceed along these lines. Then a functional analysis will reveal certain more perfect fits than others between these systems, and perhaps also very bad fits in some. One of the problems which has always interested me is why the Samoans were such pleasant people and the Zunis such nasty ones; all observers agree on those points and yet the cultures have a great number of ethological [sic] elements in common. Now I think I have found a partial answer. The Zuni method of education is to push children towards adulthood, to make it a point of shame to be childish, while the Samoans do, of course, the exact opposite and make it a point of shame to presume above one's years. But both societies demand of the adult individual, a conformity to the pattern, a lack of individual purpose or planning, or putting the self forward[,] which is extreme. But the Samoan child has been better prepared for it than has the Zuni child, who is first pushed and then sat upon. Pushing children, on the other hand, is perfectly congruent with the Manus scheme of things. I think the assumption, which I have come very near making, that the system of education of a society is perfectly adjusted to the ethos and structure of that society, is naive, also that the assumption that ethos and structure are well adjusted is untenable. All of these aspects of a social system change at different rates, in response to different stimuli, and have, in changing[,] different effects upon other parts. In the terms of the developmental physiologist, a society may be in or out of phase, as these various aspects are better or worse adjusted.

A neurotic society would be a society which had suffered some very bad and crippling blow in its early development[, but] which was still able to give direction to all future development--as for instance a city which had planned in such a way as to hamper and hurt all future development--e.g. the [Chicago] loop.

I have been rushing through these points, but I hope they are clear enough to make sense. And I would very much value your reaction to them.

You said you wished that I had expanded more on the function of daydreaming to retard social change in Mundugumor. What kind of expansion would you like and I'll see if I have the material to do it.

Once and so often I receive from one of your students the stereotyped comment that the method of Ruth Bunzel's Pueblo Potter is wrong, and you made the same point briefly to me at Christmas. Is it too involved a point to write, because I should be very interested to know what you mean. I haven't an idea, and you evidently have analyzed it in great detail, methodologically.

Jeannette Mirsky says she had a most delightful time with you. I expect her to do very good work. She has started a little late, but she has great energy, ease in dealing with people, ability to see problems, and altogether, I think should be good.

I am having a mixed time writing my Arapesh monograph, at a stint of 20,000 words a week. One week I do material culture, and then when I can bear it no longer, I do something else for a change.

How are those mimeographed notes coming on which the chosen few were to have?

R-B's answer to this letter came from the Miyako Hotel in Kyoto, Japan, where he was stopping on the way to a visiting teaching post at Yenching University in Peiping (Beijing). To begin, with he offered a clarification (apparently requested by Mead in an intervening note) of the concept *dysnomia*. Rather than being interchangeable with *dysphoria*, the two referred to two different degrees of homeostatic disturbance: *dysphoria*, to a "condition of emotional disturbance or unrest" (analogous to that in America after the Lindburgh kidnapping) which could be "eliminated by some appropriate social action" (e.g. punishment) without "any change in the social type"; *dysnomia*, to a "degree of disorder or internal inconsistency in the social system" which could only be eliminated by "a change of type" of the system or "some part of it" (such as, "in the opinion of some," the economic system of the United States "at this period of history"). Putting off the issues of Mead's letter of July 1 as "too big to deal with in any reasonable space," R-B offered instead reflections on the lectures Mead had given in Chicago, which had set him thinking "on problems that I had neglected":

The thinking revolves around the question of the relation of culture to what I call a social system. I think I find a satisfactory solution by redefining 'culture,' or rather, since it is generally not exactly defined, by defining it. A unit of culture is a mode of mental activity in an individual which is derived in him from the social environment. The culture of an individual is the totality of the units of culture present in his make-up. The culture of a society is simply the sum of the cultures of the individuals, and I am inclined to regard it as an arithmetical sum and not some sort of vector sum. On this view any mode of mental behavior that is characteristic of a

preponderant number of persons in a society can be said to be characteristic of the culture. The necessity of social integration compels the individual towards some sort of adjustment to the culture of the persons with whom he is in intimate social contact. The adjustment is internal to the individual, therefore a psychological process subject to the laws of psycho-physical consistency.

Granting that this might seem a "truism and commonplace, known to everybody," R-B suggested that it was not consistently appreciated by "ethnologists," who characteristically committed "the fallacy of misplaced concreteness," speaking of the culture of a society as an entity and not simply a sum of individual cultures. Linton, after reading Benedict, had written of "configurations acting on individuals." Others--"and in the past I have sometimes been one of them"--substituted for the sum the "concept of the mean or average of the cultures of the individuals." Pleased with his clarification, R-B saw it as a bridge between his work and Mead's: "this very simple statement of the nature of culture enables me to fit together as parts of one systematic whole all that you are doing and all my own analyses of social structures":

A social system includes the following components: (a) a number of human beings (b) their relations to one another (the social structure) (c) their activities (the social life, which is what we actually can directly observe) (d) the natures of the individuals (1. the intrinsic psycho-physical human nature, derived by heredity; 2. culture, derived from the social environment) (e) the natural environment (as modified by social action of the past) and the relations of the individuals to it.

Although he feared this would not "convey very much" to Mead, R-B felt it a "satisfactory solution to certain difficulties that I found in your work and in that of Ruth Benedict."

In a letter the following month from China, R-B suggested that his purpose in coming there had been "laying the foundations for a sociological survey both of China and Japan, starting in the first instance with village life." Responding to a hope Mead had expressed that a job might be found for Reo Fortune, he wondered if Reo might be able to come to China for two or three years to carry on this work, and hoped that Mead might accompany him. Replying on December 4, 1935, Mead informed R-B that she and Reo had been separated since 1933 and were now divorced--but that, in order to make it easier for her to look after Reo's affairs, she had not spread the news about. Telling R-B that she soon was leaving for Bali for fieldwork (but without mentioning that Gregory Bateson would be joining her), she offered a kind of coda to their recent exchanges:

On the boat I am going to write a new introduction and conclusion to Comp[etition] and Coop[eration], and in working it over, your letters and the Anthropologist articles have been Godsend. I think the way in which you have solved the point of internal functional consistency in individual psychology and in culture, by the way in which you define culture, is beautiful. I am still a little confused about social structure, whether it is the actual relations obtaining between actual individuals, or the social forms which prescribe those relationships. If you ever have time to clear that up for me, I'd appreciate it.

As published, Mead's concluding "Interpretive Statement" in the collective volume Cooperation and Competition contained a definition of social system which incorporated elements (often in the same words) of both the definitions R-B had offered her in correspondence (1937:458). There was also a similarly derivative discussion of the three degrees of social integration: simple, corporate, political (467),

as well as comments on dysnomia in the educational systems of several groups (495)--in each case, with appropriate acknowledgement.

Much later in life, Mead recalled that after Gregory Bateson, her third husband, came to the United States "in the spring of 1935," the two of them, "working together with Radcliffe-Brown," made "a further attempt to define what is meant by society, culture, and cultural character" (BW:222). From the evidence here, it would seem that the "working together" is perhaps not to be taken in a literal sense. In response to a query in 1973, she said that Bateson had come to New York and Chicago for a series of lectures "in the winter of 1934": "Gregory and I talked some of the problems over in New York, and then he discussed them with Radcliffe-Brown in Chicago and came back with the formulation that I used in the book" (Mead to GWS, 2/22/73)--a memory which does not fit precisely with the evidence here presented. Be this as it may, the letters do suggest a significant theoretical interaction--which is all the more interesting in view of the widespread attitude of later British social anthropologists toward Margaret Mead, Ruth Benedict, and the culture and personality school. Although the concern with such issues manifest in this correspondence is very much on Mead's side, rather than Radcliffe-Brown's, it was part of an exchange which was in significant respects mutual.

There are several other brief comments that may be made about this exchange. In what may be regarded in a certain sense as one offshoot of the British social anthropological disparagement of Mead, Derek Freeman, in criticizing her work on Samoa, has taken her as representative--if not in a sense constitutive--of Boasian cultural anthropology (Freeman 1983). But in fact there was a dualism in Mead's anthropology--as indeed there was in Boas, in Boasian anthropology, and in anthropology generally. A product of the Boasian historical school--indeed a favorite of Boas himself--Mead was nonetheless attracted to nomothetic approaches: rigorous method, the comparative study of social structures, systems analysis, the study of determinate regularities of human behavior, even to the point of speculating on their biological underpinning. Despite her cultural relativism, she was, in certain moments and contexts, quite capable of cultural evaluation (witness the discussion here of dysnomia, and of Zuni childrearing); and in the end (as in the beginning), she was interested in problems of human evolution. Although her encompassing rubric was "culture," she was seriously interested in its relation to "social structure," and there was much in Radcliffe-Brown's thinking that she found attractive--as did a number of American anthropologists in this period, including Ralph Linton (GS 1978).

At the same time, it is worth noting again, in a different context, the way in which Radcliffe-Brown's thinking--which some have seen as born directly from the brow of Emile Durkheim and thence simply polished for forty years (a view which his own retrospective self-systematization did not discourage)--was itself in significant respects an evolving historical product. Evolving is perhaps not the word, since there was indeed a fundamental structural consistency. But there were changes nonetheless, and a number of them took place while he was living among Boasians. From this point of view, his interchange with Mead is very suggestive. In form, it was, even in correspondence, more lecture than discussion. But in responding to questions that Mead raised, Radcliffe-Brown was forced to think further about some aspects of his own position. Notable among these was what he thought about the idea of "culture"--in regard to which his thinking was in some respects quite similar to that of Edward Sapir [G.W.S.]

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The letters of Margaret Mead are reproduced here by courtesy of the Institute for Intercultural Studies, Inc. New York.

SOURCES FOR THE HISTORY OF ANTHROPOLOGY

I. National Anthropological Archives: The Waldo R. Wedel Collection

As an intern at the National Anthropological Archives last summer, I began processing the Waldo R. Wedel Papers--a collection of 117 boxes and a dozen map cases. There is now a partial finding aid surveying the materials and documenting Wedel's correspondence, administrative and organizational files. These materials span Wedel's career from the early 1930s into the 1980s. Heavily influenced by William Duncan Strong, Alfred Kroeber and Carl Sauer, Wedel's career was primarily, although not exclusively, associated with his archeological fieldwork in the Great Plains, especially in Nebraska, Kansas and the Missouri River Basin. After working for the Nebraska Historical Society and Gila Pueblo Foundation, Wedel became affiliated with the Smithsonian Institution in 1936, and continued there until retirement, when he held the position of Curator of Archaeology and Senior Archaeologist. In the summer of 1946 Wedel became director of the Missouri Basin Project of the Bureau of American Ethnology's River Basin Surveys, a position he held until 1950. Throughout the 1950s, he continued to participate in the excavation of a number of River Basin Survey sites. His meticulous and careful fieldnotes have already begun to assist the Smithsonian Office of Repatriation's attempts to identify bones and grave goods for return to appropriate descendant groups of Great Basin Native Americans.

Wedel's papers are an invaluable source for reconstructing the history of Plains archeology. They include careful documentation of the annual meetings of such groups as the Society for American Archeology and the Plains Conferences. The Guide to the National Anthropological Archives lists among his correspondents David Barreis, Frank Calhoun, Phillip Drucker, William Fenton, Jesse Jennings, Emil Hauray, A. T. Hill, A.V. Kidder, George Metcalf, Dennis Stanford, W. D. Strong, William Sturtevant, A. Wetmore, and George F. Will. The extraordinary levels of detail, legibility and

inherent organization of the collection will enable historians of archaeology to pursue more narrowly bounded questions, including the gradual incorporation of carbon-14 dating into Wedel's research methodology. Another set of papers reflect his critical position on "new archeology." Other documents pertain to the incorporation of the B.A.E. into the Smithsonian Office of Anthropology in 1965. The collection also contains a large number of drafts of texts, descriptions, and floor plans relating to museum exhibits.

The collection has been organized into eight series: Correspondence of Waldo Wedel; Correspondence of Mildred M. Wedel; Organizational and Administrative Material; Exhibit Materials; Publications; Manuscripts; Research and Fieldwork Materials; and Maps and Large Drawings. Photographs have not yet been removed for separate preservation, and are retained throughout the collection as they existed among the papers. (Robert McLaughlin)

II. Archives of the Laboratory of Anthropology/Museum of Indian Arts and Culture

The Laboratory of Anthropology/Museum of Indian Arts and Culture, a division of the Museum of New Mexico, has set up an archives of the papers and photographs in its possession. The papers of the original Laboratory of Anthropology constitute one of the major collections. Other collections of interest include the field notes of A. V. Kidder's Pecos work; the papers of Erik K. Reed, Dorothy Dunn Kramer, Mabel Morrow, Sylvanus G. Morley, Jesse Nusbaum and others; the field notes of the Ramah projects; and photographs of archeological survey and excavation done at the Laboratory of Anthropology in the 1930s. Also included are the Site Survey Files of all archeological work done on state lands. Most of the collections have been processed, and a list of them is available, as well as guides. Researchers are welcomed, by appointment. For more information contact Willow Roberts Powers, Archivist, Laboratory of Anthropology/Museum of Indian Arts and Culture, P.O. Box 2087, Santa Fe, N.M. 87504-2087; Tel: (505) 827-6344; Fax: (505) 827-6497.

III. ANSS currents

ANSS currents, the newsletter of the Association of College and Research Libraries Anthropology and Sociology Section, under the editorship of James Williams (University of Illinois Library) and Fred J. Hay (Tozzer Library, Harvard) includes materials on sources and reference materials in anthropology, some of them of interest to historians of the discipline. Among those listed in recent numbers are:

Images of the Other: A Guide to Microform Manuscripts on Indian-White Relations. (By Polly Swift Grimshaw. Urbana: University of Illinois Press, 1991.) This guide identifies, describes, and evaluates sixty-five manuscript collections in microform which document the earliest contacts of Europeans with native inhabitants of the Americas.

The American Monographs Program, 1876-1949, Unit Two: Anthropology. (Further information available from Chadwyck-Healey Inc., 1101 King St., Alexandria, VA 22314.) This program was designed to identify, film, and republish on microfiche all monographs published in the U.S. between 1876 and 1949. These titles, which reveal the growth of anthropology as an academic discipline in the U.S., will be available as a set or as individual titles.

RESEARCH IN PROGRESS

Claude Blanckaert (Muséum national d'histoire naturelle, Paris) has organized a research group on "Anthropological Institutions in France from 1820-1940" which will prepare an inventory of sources, a collection of prosopographic information, case studies, the articulation of theoretical models, international comparisons, etc.

Valerie Pinsky has been working in the National Anthropological Archives as a Smithsonian Postdoctoral Research Fellow, where she is researching the history of government/Smithsonian archaeology since the New Deal, and its relationships with university anthropology. This research includes a focus on the development of survey and salvage projects carried out under WPA archaeology and the River Basin Surveys, and their implications for interdisciplinary boundary-formation.

Willow Roberts Powers (Laboratory of Anthropology, Museum of New Mexico) has embarked on a history of the Harvard Five Cultures Values Study. In addition to research in the Harvard University Archives, she is interviewing as many of the project personnel as possible. She is also working in the field notes, a set of which are in the Laboratory of Anthropology/Museum of Indian Arts and Culture Archives, with the further hope of making them useful to future anthropologists. The research will form the basis for a dissertation in anthropology at the University of New Mexico, under the direction of Louise Lamphere.

Filippo Zerilli (University of Rome) is writing a dissertation entitled "'L'etnologia 'au sens large': elementi per una biografia intellettuale di P. Rivet, 1900-1928"

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Smith, Pamela, "Sir Grahame Clark: A passionate connoisseur of flints (An internalist study of Clark's early publications)." (M.A., University of Victoria, Vancouver, 1993).

Stilling, Rodney L. , "The diminishing deluge: Noah's flood in nineteenth century American thought." (University of Wisconsin, Madison, 1991)

II. Recent Work by Subscribers

[Except in the case of new subscribers, for whom we will include one or two orienting items, "recent" is taken to mean within the last two years. Please note that we do not list "forthcoming" items. To be certain of dates and page numbers, please wait until your works have actually appeared before sending offprints (preferably) or citations in the style used in History of Anthropology and most anthropological journals. Please include page numbers.]

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III. Suggested by Our Readers

[Although the subtitle does not indicate it, the assumption here is the same as in the preceding section: we list "recent" work--i.e., items appearing in the last several years. Entries without initials were contributed by G.W.S.]

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A.L.C. = Andrew L. Christenson R.D.F. = Raymond D. Fogelson
D.R.G. = Douglas R. Givens R.H. = Richard Handler
M.B. = Matti Bunzl W.C.S. = William C. Sturtevant
R.B.W. = Richard B. Woodbury

GLEANINGS FROM ACADEMIC GATHERINGS

I. History of anthropology in Germany--At the biennial conference of the German Ethnological Society DGV (Deutsche Gesellschaft für Völkerkunde), which met in Leipzig from 4 to 8 Oct. 1993, two sessions were held on the history of anthropology. Under the general theme '*Ethnologieggeschichte - wozu?*', 16 papers and a historical film were presented. The contributors came from West- and East-Germany, Austria, France, Danmark, Slovenia and The Netherlands. It was the second time that such a symposium was held at the conferences of the DGV. The first one was held in October. 1991 in Munich, organised by Annemarie Fiedermutz (Munich) and Han F. Vermeulen (Leiden) and consisted of 9 papers. This time the organisation was done by a team consisting of Hans-Jürgen Hildebrandt (Mainz), Lothar Pützstück (Cologne) and Han F. Vermeulen (Leiden), on the basis of a questionnaire distributed to some 200 individuals and 80 institutes in the German-speaking world, which had a response of some 90 scholars.

The papers presented on October 6 discussed the following topics: Bozidar Jezernik (Ljubljana), The eyes that would not see. The Balkans in the eyes of European travellers in the second half of the 19th and the beginning of the 20th century; Britta Rupp-Eisenreich (Paris), The history of German-speaking anthropology: views from Paris; Bernhard Streck (Heidelberg), The ancestors of the 'scientific community', or: why we study the history of anthropology; Michael Harbsmeier (Copenhagen), The prehistory of ethnology; Han F. Vermeulen (Leiden), Ethnology and ethnography in the 18th century: the relevance of historiography; Friedrich Valjavec (Tübingen), Émile Durkheim - preconditions and effects; Hans-Jürgen Hildebrandt (Mainz), Early anthropology of law and its importance for current discussions on the theory of law; Rolf Husmann (Göttingen), First experience with a film project with Sir Raymond Firth, followed by the presentation of a film by Peter Loizos, Rolf Husmann and Werner Sperschneider, entitled *Firth on Firth. Reflections of an Anthropologist* (1992, 49 min.).

On October 6, the following papers were presented: Hilke Thode-Arora (Hamburg/Cologne), Research report on the firm and family Umlauff as collectors of ethnographic objects; Felicitas Bergner (Hannover), Africa in German museums - on the acquisition history of German ethnological museums during the colonial period; Peter Linimayr (Vienna), Ethnology in Vienna and its relation with German ethnology during national-socialism; Ute Michel (Hamburg), Research proposals for an ethnology of Europe in the national-socialist period: the case of W. E. Mühlmann; Birgit Scheps (Leipzig), German ethnologists in German-New-Guinea (1870-1914); Dietrich Schleip (Stuttgart), Early ethnographies as historical sources; Andrea Schmidt (Freiburg), Paul Wirz and ethnographical collecting: ethnological research as classical science and personal experience; Stephan Augustin (Herrnhut), The beginnings of ethnographical collecting within the Evangelische Brüder Unität; Gundrun Meier (Dresden), Historico-critical edition of a manuscript: C. G. A. Oldendorp on the West-Indian islands in the 18th century. All papers were presented in German, except for the paper by Dr. Jezernik.

The last session was followed by a meeting, in which some 15 present decided to constitute a working group on the history of anthropology: Arbeits-Gruppe (AG) *Geschichte der Ethnologie*, which earlier has been approved of by the general members meeting of the society. The committee of this AG consists of: Adam Jones (Frankfurt/Leipzig), Bernhard Streck (Heidelberg), Han F. Vermeulen (Leiden). The primary tasks of this committee are the organising of four sessions at the next DGV-conference in Vienna (October 1995), and of a smaller session on the history of anthropological research in Africa (in Leipzig, early 1995). The address of the chairman is: Dr. Adam Jones, Institut für Historische Ethnologie, Liebigstraße 41, 60323 Frankfurt am Main, Germany.

III. The History of Anthropology in Mexico--From July 5-7, in Mexico City, a colloquium organized largely by Mechthild Rutsch and sponsored by INAH (the National Institute of Anthropology and History) was held on "The History of Anthropology in Mexico: Sources and Transmission." Curtis Hinsley gave an opening talk on "Anthropology and National Identity: Reflections on the United States in the Nineteenth Century." The session on July 5 ("The Importance of History in Mexican Anthropology") included papers by Guillermo de la Peña ("Nationals and Foreigners in the History of Mexican Anthropology"); Ignacio Rodriguez ("Ideological Resources of the Mexican State: The Case of Archeology"); Luis Vargas Guadarrama ("The Teaching of the History of Medicine: Reflections on Physical Anthropology"); and Francisco Javier Guerrero ("The Disillusionment of Marxism and Anthropology in Mexico, 1970-90"). The session July 6 ("Sources for the History of Mexican Anthropology") included papers by Rodolfo Coronado ("Oral Sources in the History of Mexican Anthropology"); Guillermo Alfaro ("The Use of Photography and Film in Anthropology"); Columba Quiñones ("Data Bases in Mexican Anthropology"); Luis Vasquez Leon ("The National Archive of Anthropologists"); Jesus Monjarras, Elena Limon, and Ma. de la Cruz Paillos (The Archive of Robert H. Barlow"); Erendira Nansen ("The Humboldt Manuscripts"); Carlos Garcia Mora ("The Paul Kirchoff Archive"). The final session ("Formal and Informal Transmission of Historical Memory in Mexican Anthropology") included papers by Raymundo Mier ("Taxonomies of Contempt"); Jaime Litvak King ("The Anecdotal in the Formation of Anthropologists"); Otto Schumann ("The Extramural Formation of Linguists"); Carlos Navarrete ("Teaching Anthropology"); Fernanda Camara ("Anthropology Inside and Outside Classrooms"); Francisco Barriga ("The Magnified Classroom." It is planned to publish the conference papers.

IV. American Society for Ethnohistory--The 1993 annual meeting, November 4-6, at Indiana University, included a plenary session on "Forty Years of Ethnohistory," in which Raymond Fogelson, William Fenton, William Sturtevant, Helen Tanner and James W. vanStone participated, as well as papers by Fred Gleach on Frank Speck, by Jennifer Brown on A. I. Hallowell, by Douglas Parks and by Raymond DeMallie on James R. Murie. Scott Forsythe gave a paper on "Records Concerning Native Americans in the National Archives."

ANNOUNCEMENTS

History of anthropology in Europe

At the 3rd biennial conference of EASA (European Association of Social Anthropologists), at Oslo, 24-27 June 1994, there will be two sessions on the History of European Anthropology. These sessions are organized by Dr. Jan de Wolf on behalf of the History of European Anthropology Network (HEAN). The address is: Dr. Jan de Wolf, Dept. of Cultural Anthropology, University of Utrecht, P.O. Box 80.140, 3508 TC Utrecht, The Netherlands.

Cheiron

Cheiron: The International Society for the History of Behavioral and Social Sciences, will hold its twenty-sixth annual meeting June 2-5, 1994 at the Université du Québec in Montreal. Program submissions (symposia, papers, and posters), which may include topics in the history of anthropology, must be postmarked by January 15, 1994. Travel awards are available to assist students. For further information, contact Dr. Andrew S. Winston, Program Chair, Department of Psychology, University of Guelph, Guelph, Ontario N1G2W1 (phone:519-824-4120; fax:519-837-8629).