

and about a scholar), journals (especially extinct ones), photographs, material artefacts collected during fieldwork, and so on. It was pointed out that M N Srinivas's original field notes were still extant and needed to be preserved, while, on the disheartening side, we learnt that S C Roy's original papers had disappeared. The idea of a newsletter to sustain the momentum of research on the history of the disciplines was also mooted, and again there was discussion on whether this should be (in whole or part) Internet based. [For a fuller account of the IEG Workshop, see Nandini Sundar, Satish Deshpande and Patricia Uberoi, 'Indian Sociology and Anthropology: Towards a History' in the *Economic and Political Weekly*, June 10-16, 2000, from which the previous two paragraphs have been taken. Also available on the EPW website (<http://www.epw.org.in>) in its Archives section]

One measure of the depth of interest in disciplinary history witnessed at the IEG Workshop is the number of outcomes it has produced. Pursuant to a unanimous resolution passed at the final session of the Workshop, a permanent Research Committee on disciplinary history has now been set up under the auspices of the Indian Sociological Society (under the more inclusive title of "Research Committee on the Sociology of Knowledge" at the suggestion of the Society). The Committee held its first meetings at the 26th Annual All-India Sociological Conference at Trivandrum in December 2000, and its next meeting to be held at the 27th AISC in Amritsar will be devoted to discussing college curricula in sociology and anthropology and questions of pedagogy. A collection of opinion pieces on institutional issues in Indian sociology/anthropology (revised versions of presentation made at the Workshop) has been published in the journal *Seminar* (No.495, November 2000). An edited volume of essays on founding figures in Indian sociology and anthropology is also under preparation.

[While this note draws on collaborative work with several colleagues, including Nandini Sundar, Patricia Uberoi and Satish Saberwal, I am solely responsible for the opinions expressed here.]

FOOTNOTES TO THE HISTORY OF ANTHROPOLOGY

Glimpses of Impending Generational Change: A Franz Boas Miscellany

Searching my research files in the absence of submitted documentary materials for this number of HAN, I came across a folder labeled "Boas Letters—Am. Anthro. in the 30's." Although the five fading copies it contained were at first glance rather diverse, upon inspection they seemed perhaps to hang together on a thematic string: the responses of Franz Boas to generational changes in anthropology during the last decade of his own life, in a period of theoretical and institutional diversification. Well into his seventies, and no longer wielding so much disciplinary and institutional influence as he once had, even on those who had been his students (cf. Darnell 1990:319-32), Boas was encouraging of some changes, discouraging of others, and with mixed success. Although diverse in content, and reprinted here with minimal contextualization, the five letters that follow do suggest something of the range of Boas' concern, and the growing limitations of his ability to assert a coordinating influence over a range of institutional matters--funding decisions, editorial control, and departmental personnel--each of which, and all, collectively, suggested the direction of change in American anthropology in the years after his death in 1942

The first in the sequence was written by Boas on April 11, 1932, to Rev. John M. Cooper, Professor of Anthropology at the Catholic University of America, ethnographer of Native America, and secretary of the American Anthropological Association in the early 1930s:

I learned quite recently that there is some consideration in the Rockefeller Foundation in regard to a fund for ethnological research among vanishing tribes; I believe intended as an international affair. I believe [Alfred] Tozzer and [Edward] Sapir could tell you more about the details.

You probably also know that the American Philosophical Society in Philadelphia has received a very large bequest. They have appointed a Committee to consider what to do with the money and [George Grant] MacCurdy [then president of the A.A.A.] is the representative of anthropology. He wrote to me recently for advice and I suggested a meeting of a number of anthropologists to discuss and formulate plans. I do not think this ought to be done by a single person. I also understand that the Anthropological Association has, from the Social Science Research Council, some funds for planning research work. Might it not be well to try to bring about a joint meeting for these two purposes? It seems such an unusual opportunity we ought to do everything we can to lead it into proper channels. Perhaps you might talk it over with Swanton, who is now president of the Association and if you think well of the plan, communicate with MacCurdy as representative of the Philosophical Association and perhaps with] Tozzer and Sapir in regard to the feasibility of cooperating with the Rockefeller Foundation.

At the time Boas wrote, the American Philosophical Society had just received a major bequest from Dr. R. A. F. Penrose, Jr. "for the development of a broad and useful programme that will be a great stimulus to learning" which was not specifically directed to anthropology. The Rockefeller Foundation, however, was then considering major support for cultural anthropological research, and a "research committee" of the A.A.A. was appointed to formulate plans. The major issue was the relative emphasis on "source" or "field" research, with Tozzer and Sapir favoring the former, and Alfred Kroeber and Clark Wissler (with input from Boas), the latter. Although in June 1932, the committee did forward a twelve year, \$5,000,000 plan for field work among three hundred groups throughout the world, the Foundation, in the context of economic collapse and internal dissension among anthropologists, decided to terminate support for research in cultural anthropology (cf. Stocking 1985: 196-201: "The Biggest Anthropological Pie Ever Concocted"; Procs. APS 70:xviii).

The second letter in the series, written on November 24, 1933 to Boas' longtime Harvard ally, Alfred Tozzer, dealt with the succession problem in the discipline's major publication, the American Anthropologist, which since 1915 had been edited by a succession of Boas students or allies: Pliny Goddard, John Swanton, and Robert Lowie, who had been in charge since 1924.

Many thanks for your letter. I am afraid that I cannot attend the meeting in Boston. The thing is too strenuous for me and I want the time for work here. Thank you very much for your invitation. I do wish you would come down some time and stay over night so that we can talk over matters.

Thank you very much for your reports. I will try to get them copied and return the originals to you. I notice that Harvard, Chicago and Yale are not on the list. Haven't you gotten those?

You know that we have to get a new editor for the American Anthropologist. We have talked over the matter here and it seems to me the man best fitted for it would be [Leslie] Spier. There is a little doubt in my mind whether he might be too dogmatic in his selection of material, but I think he will do as well as Lowie did of late years.

I understand that [Robert] Redfield has been proposed by some people. I do not think that he would be the right man. I have not seen anything by him that seems to me encouraging and besides that, it would mean we might just as well turn over the Anthropologist to Radcliffe-Brown.

[Ralph] Linton has also been suggested, but I consider Spier decidedly preferable. If you do not go to Columbus I wish you would at least express yourself in regard to your preference.

How heavily Boas' influence weighed in the decision is unclear, but in the event the threat of Radcliffe-Brownian "functionalism," second hand through Redfield, or of an embracive eclecticism, through Linton, were forestalled. Spier, a conservative Boasian, was chosen, and did indeed prove "dogmatic in his selection of material"—discouraging several of the newer trends, including the study of acculturation (cf. Baron 1994).

As Boas' comment suggests, however, he himself was more open to the "new" tendencies in anthropology than were some of the elder generation of his students. On October 15, 1935, he wrote to William Ogburn, of the University of Chicago sociology department, whom Margaret Mead had served as research assistant during his earlier tenure at Columbia, defending her work against the criticism of Robert Lowie:

Lowie sent me a copy of his letter to you in regard to Margaret Mead's book. I really do not know what to think about Lowie. For some time past he has taken the attitude that he has the only method of field work that is of any use, and that nobody can do anything that he has not done or that he cannot do. If you will look at the introduction to his book on "The Crow" you will find his claim that nobody can use native languages to advantage which, in my opinion, simply means that he cannot do it [Lowie 1935: xix]. The points that he makes are perfectly irrelevant because it is quite clear that anyone who can listen to a discourse and understand what is being said will do ever so much better work than another who relies upon an interpreter. I know a sufficient number of people who do understand perfectly what is being said, myself included. He simply will not believe that a student who sets out to really master a language, and who devotes a few months to this is able to accomplish it. It so happens that the languages of Melanesia and Polynesia are, on the whole, much easier than American Indian languages so that the task is much more easily accomplished.

Now in regard to Margaret Mead's book. First of all Lowie does not seem to appreciate that the whole attitude of Melanesia, as he might know from literature, is absolutely different from what we find among our American Indians. When the American Indian does anything new, he always tries to give it the halo of antiquity. Everything is valued according to the idea that it is traditional; the Melanesians are just the opposite. They are always eager for something new and are constantly remodelling, and if they do something that is old they try to make believe that it is new. Hence, I presume, the enormous local divergences which occur.

Margaret Mead's book [1935, Part I] is based on a carefully kept diary of everything that happened in the locality during a period of six months, painstakingly done and the material is going to be published by the American Museum of Natural History, as Lowie might easily have found out if he had taken the trouble (Mead 1938-49). There was time enough to launch such a fierce attack if he found her material insufficient. It is just the

same as Kroeber's criticism of her Manus work, when he reproached her for not having done any ethnology and never took any notice of her ethnology when it was published by the Bishop Museum. The present book is intended for sociologists and anthropologists who have a reasonable amount of faith in the honesty of the observer. I encouraged her to bring it out because the monograph with all the detail evidence will not be generally read anyway, while I consider her point of view important.

I take the responsibility for the general trend of her work. The first time when she went out to Samoa I sent her with the particular task of investigating in how far the difficulties of puberty were found in a culture that has not the inhibitions of ours. I am perfectly aware that subjective elements must appear in the presentation of things of this kind, and ideally it would be well to have a number of people of entirely different disposition doing this kind of work in the same culture, but this ideal is hard to realize. Still it seems to me significant that two people so fundamentally different like [Reo] Fortune and [Géza] Roheim, should come back with essentially the same impressions of the character of Melanesian culture.

I hope you will consider my judgment as equally worth consideration as Lowie's.

Without present access to Lowie's letter (which is not in the Ogburn papers here in Chicago), one cannot be certain of the specifics of his critique, although one might assume that it bore some relation to the later exchange between Lowie and Mead on the "use" of native languages in the field (Lowie 1940). Sex and Temperament was, perhaps, one of the more controversial of her ethnographic interpretations. The interesting point, however, is that in the case of Mead, Boas displayed a greater openness to the newer tendencies in anthropology than several of his elder male students were willing to allow—not, however, without acknowledging the possibility of a distorting "personal equation."

When it came to personnel issues in his own department, Boas (who previously had unsuccessfully sought Alfred Kroeber and Edward Sapir as his potential successors) resisted the appointment of Ralph Linton, whose eclectic but theoretically forward-looking Study of Man (1936) was for some years to come perhaps the most widely used introduction to anthropology. On January 28, 1937, Boas wrote Dean George Pegram of Columbia discouraging Linton's appointment as his successor:

I have no official notification of what the Committee which is considering the Department of Anthropology is doing but I hear indirectly that they recommend the appointment of Dr. Linton and Dr. Strong, the former as head of the Department.

In my opinion such an arrangement would be most unfortunate. Dr. Linton is good enough in his place in Madison, but I consider him a mediocre man without any original ideas who would go on in a routine way, unable to give that stimulation which is required in the development of the Department. Furthermore it seems to me a waste of money to spend a full professor's salary at a time when all the social sciences are being deprived of the support heretofore rendered by the Rockefeller Foundation. The work of the Department requires funds for the conduct of researches which in other departments are spent on laboratories. Geologists are able to furnish such opportunities through the help of geological surveys but anthropology is denied such opportunities. The preparation of research works requires training in ethnology, archaeology and linguistics as needed for anthropological field work. As I understand it the plan of the Committee provides for archaeology but with the appointment of Linton and Strong the necessary fields would not be covered.

In my opinion a reasonable set-up would be Dr. Benedict, Dr. Strong, Dr. Lesser and Dr. Herzog. These can provide for a satisfactory all around training, Dr. Benedict, general ethnology, Dr. Strong, archaeology and ethnology, Dr. Lesser, ethnology, Dr. Herzog, ethnology in Africa and linguistics with special emphasis upon primitive literature and music. I should like to look forward to the appointment of a younger man to take charge of ethnology of South America, a subject to which we have been giving attention to in the last few years.

Despite Boas' objections, Linton received the Columbia appointment, and in 1938 succeeded Leslie Spier as editor of the American Anthropologist.

On April 6th of that same year, Spier wrote to Boas announcing his departure from the Yale University Department of Anthropology:

I want you to know that I deeply appreciate your friendly letter. It helps at a difficult time to know that you are concerned and that we can count on your understanding. As I look back on one turning point and another, I know that your advice at the time was wise and your help real. But please do not be too seriously disturbed about my decision to leave here. It isn't as though I were losing an opportunity to go on effectively working at anthropology. On the contrary I hope to make clear that I can do much better by leaving.

Resentment is naturally an ingredient in the decision, but the major factor is the knowledge that the more sensible mode of life Ann and I intend to follow will permit us both more time and opportunity to give to our anthropological work. This has been in our minds for several years, but the decision to go was precipitated by the change here somewhat earlier than we intended. We have carefully weighed the advantages of staying or going and decided to leave at the end of next year (June 1939). When they learned of this the administration offered me permanency and promotion but after a month's thought about it, we concluded that the offer was not significant and perhaps not genuine.

The situation here is such that it will be next to impossible to continue an unhampered life of training students and research. Sapir and I are in agreement that there is nothing to be salvaged here. He and I have been ruthlessly stripped of authority and influence, so far as the formal situation is concerned, and we are relegated to the status of underlings to Murdock's plans and ambitions. While outwardly the formal situation looks as though we had opportunity to go on without hindrance, it will not be at all possible, because of Murdock's characteristics.

Historically the affair is old: its roots go back seven or eight years to the day when the administration played a characteristic trick on [Albert] Keller by bringing in Sapir in Keller's absence ~~or consent~~ and without his consent -- a situation of which Sapir was wholly unaware. This, today, is Keller's triumph.

Now then as to what Ann and I intend. We have long planned to live primarily at our home in California, working away at our research jobs. We have a most minute income on which we can live very simply, but it is enough to permit us to give all our time and attention to these things without hindrance. For the past five years I have had next to no time for research but from now on it promises to be different. This plan leaves me free to give a half year at a time to teaching, and it also makes it possible for me to choose just what that will be. Naturally I continue to have a very real interest in training students. So

as soon as we had reached our final decision I made arrangements with one of the western universities to operate on just this plan, at an institution where the possibilities for carrying on the kind of research and raining in which I am interested are real. I regret that I cannot be more definite until formalities are concluded.

In short, I want you to believe that I am being thoroughly realistic. I haven't flown off the handle and don't intend to. I will get through the next year in as amiable a fashion as I can and then go about my own concerns. There is nothing to stay here for. On the contrary the plan that is now developing will offer a better chance to stick to anthropology and avoid fussing with complicated political situations than if I stayed. I can assure you that there is nothing precipitate or rash about what I am doing; it represents a sober judgment to lead a calmer and more sensible life than any I could find here.

Let me say again that I appreciate what you wrote me.

Spier was at this time acting chair of the Yale department during Sapir's sabbatical year, and had been negotiating with Dean Furness about the future of the department, and its relation to Murdock's Institute of Human Relations. Disagreeing strongly "about the necessity of a link between sociology and anthropology," Spier "defined the department program in terms of traditional Boasian subject matter, not in terms of interdisciplinary collaboration." Murdock, however, was able to convince the dean that Spier had "bungled the job of chairman," to the point where "Furness asked Murdock to take it on." Murdock "rapidly dismantled the program in primitive linguistics and reestablished ties to the IHR" (Darnell 1990 351-53).

Taken as a group, these five letters may be seen as foreshadowing changes in American anthropology that were to become more clearly manifest in the several decades after Boas' death late in 1942. Those changes, however, did not efface the evidence of Boas' foundational influence, which was clearly manifest in many of the post-war generation—and is still manifest a half century on.

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[The letters included above are reproduced with the permission of the American Philosophical Society, Philadelphia, PA., where the Franz Boas papers are housed in the Society's Library]

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RESEARCH IN PROGRESS

Anjeli Arondekar (Women's Studies, University of California, Santa Cruz), is doing research for a book to be entitled "A Perverse Empire: Victorian Sexuality and India."

Martin Couttenier (Catholic University of Leuven) is doing research on the history of anthropology in Belgium, with special reference to the rise of physical and colonial anthropology, focussing on the Colonial Museum in Tervuren, founded in 1897, and on the collection of material culture in the then Belgian Congo.

Todd Kontje (German & Comparative Literature, UC San Diego) has had a Guggenheim Fellowship for research on German Orientalisms.

Daniel Sherman (French Studies & History, Rice U.) has had a Guggenheim Fellowship for research on "The French and their 'Others', 1945-1975."

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Cooper, Mary (History of Science, Harvard, 1998) "Inventing the Indigenous: Local knowledge and natural history in the early modern German territories"

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