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Margaret Mead, Franz Boas, and the Ogburns of Science: The Statistical and the Clinical Models in the Presentation of Mead's Samoan Ethnography

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SOURCES FOR THE HISTORY OF ANTHROPOLOGY

Don D. Fowler (University of Nevada, Reno) reports that, in response to a petition of those attending the history of archeology symposium at Carbondale last May, the Society for American Antiquity has formed a committee to investigate the problem of getting materials relating to the history of New World archeology archived and made accessible. The members of the committee include Fowler (chair), Jeremy Sabloff (Pittsburgh, ex officio as SAA president-elect), Curtis Hinsley, Jr. (Colgate, as advisor), Susan Bender (Skidmore), Douglas Givens (St. Louis Community College), Edwin Lyon (Corps of Engineers), David Meltzer (Southern Methodist), and Jonathan Reyman (Northern Illinois). The charge of the committee is to inventory existing archives of personal papers, as well as field notes, maps and photographs relating to the history of New World archeology; to work with archival depositories in identifying, collecting and inventorying other collections of materials; ultimately to produce a "union catalogue" of these materials. The committee will be seeking grants to carry out this work.

FOOTNOTES FOR THE HISTORY OF ANTHROPOLOGY

Margaret Mead, Franz Boas, and the Ogburns of Science: The Statistical and the Clinical Models in the Presentation of Mead's Samoan Ethnography

(G.W.S.)

One of the central paradoxes of the career of Margaret Mead relates to the problem of ethnographic method. Constantly experimenting with new methodologies, frequently reflecting in print on problems of method, she was perhaps more self-consciously and consistently concerned with methodological matters than any anthropologist of her generation (e.g., Mead 1933). At the same time, many of the criticisms that have been directed against her work have focussed on methodological issues. This has been especially the case in the recent controversy surrounding her early Samoan research. One of the focal topics of that debate has been the role of quantitative evidence in ethnographic argument. Basing his critique in part on arguments about the numerical rates of certain behaviors, Derek Freeman has attacked the evidential basis for Mead's generalizations concerning adolescence. In contrast, defenders of Mead have questioned the utility of simple quantitative measures in the interpretation of ethnographic phenomena. Furthermore, it has been suggested by some that her alleged ethnographic failures must be understood in relation to the state of ethnographic method in the 1920s, and the advances that may have taken place since that time. In this context, it is of considerable historical interest to note that there is evidence in the Mead papers in the Library of Congress that while she was still in Samoa, Mead was quite explicitly concerned with how to handle quantitative data in the presentation of the results of her research. While the present brief documentary note cannot hope to resolve the paradox noted above, it may cast some light on certain methodological concerns of Mead's early ethnography.

Coming from a background in psychology, which by this time was already under the sway of quantitative methods (Hornstein 1988), Mead had been a student (and research assistant) of William Ogburn, who was perhaps the leading proponent of quantitative methods among his generation of sociologists. (It was Ogburn, in 1930, who had carved into the facade of the new Social Science Research Building at the University of Chicago a version of the famous scientific prescription of Lord Kelvin: "When you cannot measure, your knowledge is meager and unsatisfactory"). Mead's master's thesis had been a quantitative study of "Group Intelligence Tests and Linguistic Disability among Italian Children" (1927), and she had actually considered as a field technique the use of the psycho-galvanometer to test "the possibility of measuring the relative affective strength of old and new elements in the culture as manifested by the responses of individuals"--a problem arising from her library dissertation on the relative stability of cultural elements among different groups in Polynesia (Mead 1928a). However, the "technical difficulties" proved "insuperable"; furthermore, Boas (himself an adept in statistical methods) regarded such efforts as "premature." It was in this context that he encouraged Mead to undertake "the study of the relative strength of biological puberty and cultural pattern" (1962:122).

The story of how she accepted his problem, bargained to do her fieldwork in Polynesia, and ended up on the island of Ta'u in the Manu'a region of American Samoa has by now been told a number of times, and since the publication of Mead's <u>Letters From the Field</u> in the year before her death, it has been easy to glean even more about that experience from contemporary materials. However, as Mead indicated in the introduction of that book, she included "only a fraction" of the letters that she had written during her first fieldwork expedition (1977:15). Although she incorporated portions of two to Franz Boas, these were by no means the most interesting of the short series they exchanged during the period of her fieldwork. Among those that were not included are two that help to illuminate the way Boas and Mead perceived the problems of ethnographic methodology in a venture which Boas described as "the first serious attempt to enter into the mental attitude of a group in a primitive society," the success of which would "mark a beginning of a new era of methodological investigation of native tribes" (Boas to Mead, 11/7/25). The first letter was written by Mead during a period of methodological <u>angst</u> of a kind that must be experienced by many young ethnographers (as well as, if my own case is a model, by apprentices in other scholarly crafts):

Tau, Manu'a, American Samoa
January 5, 1925 [1926]

Dr. Franz Boas
Columbia University,
New York City

Dear Dr. Boas:

This will acknowledge your letter of November 7th. That for all your generous haste in answering it it reached me after I had been settled in Manu'a for six weeks [sic]. Which very neatly demonstrates the hopelessness of trying to correspond about anything down here.

I am enclosing my report to the Research Council which is required by the first of March. I have sent them two copies under separate cover and registered. If by any chance they should fail to reach the Council and they should advise you of that fact would you see that they get a copy, please. I have to take endless precautions because there is no regular mail service here and we have to entrust our mail to the good nature of a series of irresponsible individuals. I realize how irregular it is for you not to have had an opportunity to criticise and approve this report. But if I had sent it earlier, I should have two weeks [fieldwork in Ta'u] instead of five to report on and furthermore there would have been no time for your criticisms to have reached me. It therefore seemed advisable to send my report directly to the Council will [with?] a definite statement that you had neither seen nor approved it in any way. And I hope that I said nothing in the report of which you would actively disapprove.

As to the content of the report, I have, as you see, made it exceedingly brief and tentative. While making absolutely no showing in conclusions at all, I could hardly enlarge further than I have done. Every conclusion I draw is subject to almost certain modification within the next ten days and is therefore pretty valueless. If the report satisfies the Council that I am working with passable efficiency, it will have accomplished as much as it could under the circumstances.

And now what I need most is advice as to method of presentation of results when I finally get them. Ideally, no reader should have to trust my word for anything, except of course in as much as he trusted my honesty and averagely intelligent observation. I ought to be able to marshall an array of facts from which another would be able to draw independent conlusions. And I don't see how in the world I can do that. Only two possibilities occur to me and both seem inadequate. First I could present my material in semi-statistical fashion. It would be fairly misleading at that because I can't see how any sort of statistical technique would be of value. But I could say "Fifty adolescents between such and such ages were observed. Of these ten had step-mothers, and five of ten didn't love their step-mothers, two were indifferent and three were devoted.

Fifteen had some sex experience, five of the fifteen before puberty, etc." All of which would be quite valueless, because whether fifty is a fair sample or not, could be determined only on the basis of my personal judgment. And saying you don't love your step-mother, or that you rebel against your grandfather but mind your older sister, or any of the thousand little details on the observation of which will depend my final conclusions as to submission and rebellion within the family circle, are all meaningless when they are treated as isolated facts. And yet I doubt whether the Ogburns of science will take any other sort of result as valid.

Then I could use case histories, like this. "X L7-3 is a girl of 12 or 13 (Ages have to be as doubtly [sic] as that). She is just on the verge of puberty. Her father is a young man with no title and a general reputation for shiftlessness. Her mother is likewise young and irresponsible, given to going off visiting and leaving X with the care of her five younger brothers and sisters. X is nevertheless excessively devoted to her mother, showing an unusual amount of demonstrative affection for her. The girl is decidedly overworked and is always carrying a baby. They are quite poor and she never has even any passable respectable clothes. Her mother is a relative of the high chief of Y, and as poor relations a great [deal] of unpleasant work falls to the share of X and her sister of 9. Her younger sister is much prettier and more attractive and is the mother's favorite (The father is neglible.) X is tall, angular, loud voiced and awkward, domineering towards all her younger relatives, obstinate, sulky, quick to take offense. She regards her playmates as so many obstacles to be beaten over the head. She has no interest in boys whatsoever, except as extra antagonists. All her devotion seems to be reserved for her mother and the pretty little sister" (etc.). I can probably write two or three times as much about each one of them before I leave. But to fill such case histories with all the minutiae which make them significant to me when they are passing before my eyes is next to impossible. And the smaller the details become, the more dangerous they become if they are to be taken just as so many separate facts which can be added up to prove a point. For instance, how many other little girls carry babies all the time, and how many other mothers go visiting. Facts which possess significance in one case but which are mere bagatelles of externality in another would have to be included in each case history or they would not be comparable.

As I indicated in my report I am making a thorough personnel study of the whole community. These provided me with a tremendous background of detail. I will quote here the information contained on one household card to give you an idea of just what this means.

L30 [Here follows a detailed listing of the household members, by name, with comments on their personal histories and personalities. In accordance with Margaret Mead's wish that informants still living, or recognizable to those still living, should not be identifiable, this material is here deleted].

. . . There are several more. As rank does not depend on primogeniture nor necessarily upon being the son of a chief rather than a relation, one must know in addition who are the favorites, and why, etc. But you see what type of information this gives me, and the numerous questions I can answer on the basis of it. I had to have it anyhow in order to thread my way through the mass of gossip and village happening.

But how to use it. If I simply write conclusions and use my cases as illustrative material will it be acceptable? Would it be more acceptable if I could devise some method of testing the similiarity of attitudes among the girls, in a quantitative way? For instance no Samoa[n] who knows I'm married ever fails to say "Have you any children?" NO. Talofai. Poor you. This is the universal response from men and women, except in the case of the boarding school girls. Now would it be more convincing if I could present an array of such responses indicating attitudes with actual numbers and questions---as "Of the fifty girls questioned, 47 said they hoped to marry soon and 45 wanted at least five children." I wouldn't feel any wiser after collecting information in that style but maybe the results would be strengthened. It will of course be fairly easy to demonstrate a fairly dead level of background and information.

I am sorry to bother you with so much detail, but this a point on which I am very much at sea. I think I should be able to get an answer in time to get some help. On second thought, I'll not enclose my report in this letter (The report contains nothing which I haven't written you) but send it air mail. If you could dash off an airmail answer I might get it sometime in March. You see that is quite late and will perhaps forgive my importunity.

The hurricane, no. II, has messed everything up nicely, but as Tutuila and Western Samoa were equally wrecked, I shan't make any change in my plans. It will considerably lessen my chances of getting ethnological information by observation, as nothing important can occur without a feast and there will be a famine here for months where every morsel of food will have to be hoarded. My health continues to withstand the onslaughts of the tropics.

With very best wishes,
Sincerely yours,

Although cast here in terms of the statistical versus the case history method, the issue recalls the epistemological dichotomy Boas posed at the very beginning of his career between the "physical" and the "historical" methods (Boas 1885), and the advice he offered is quite consistent with his increasing scepticism of the applicability of the former to anthropological research (Stocking 1974):

February 15, 1926

Miss Margaret Mead
Pago, Pago
Samoa

My dear Margaret:

I was very glad to receive today, three letters from you: a little personal note, a letter with the enclosure to Dr. Lillie also containing the report and your letter in which you asked me a few questions.

I have written to Washington and told them that you expect to be here in New York next year and that you will have an opportunity to work up your material here and that it would be unwise to have you interrupt your work now in order to write a report. Considering this I do not think that you need worry just at present about the question of the final formulation of your results.

However, I am anxious to answer your questions as well as I can, although I am quite aware that I think in the progress of your work you will find yourself the best way of presentation and that some of the difficulties that upset you in the beginning will have disappeared.

I am very decidedly of the opinion that a statistical treatment of such an intricate behaviour as the one that you are studying, will not have very much meaning and that the characterization of a selected number of cases must necessarily be the material with which you have to operate. Statistical work will require the tearing out of its natural setting, some particular aspect of behaviour which, without that setting, may have no meaning whatever. A complete elimination of the subjective attitude of the investigator is of course quite impossible in a matter of this kind but undoubtedly you will try to overcome this so far as that is at all possible. I rather imagine that you might like to give a somewhat summarized description of the behaviour of the whole group or rather of the conditions under which the behaviour develops as you have indicated in your letter to the Research council and then set off the individual against the background.

If you should give a purely statistical treatment I fear that the description would resemble the results of a questionnaire which I personally consider of doubtful value.

I am under the impression that you have to follow somewhat the method that is used by medical men in their analysis of individual cases on which is built up the general picture of the pathological cases that they want to describe. There would be no difficulty in guarding yourself by referring to the variety of personal behaviour that you will find.

I hope that the hurricane has not disturbed your work too much. Perhaps it is quite interesting to see how the people behave under stress.

I wonder whether you will not find, when this letter arrives, that you have answered your own question better than I can do it from here. However, I want to help you as much as I can.

With kindest regards,

Yours very sincerely,

Franz Boas

FB:B

Although there is no record of Mead's reply, the much more upbeat tone of her next letter in the series (2/5/26) suggests that Mead's moment of methodological angst had by that time passed. In the event, she was to adopt a presentational strategy that, in a general way, seems to reflect Boas' advice--although another influence supporting this choice may have been the editorial suggestions passed on to her by her publisher, William Morrow, at whose request she added the two final chapters of Coming of Age in Samoa (in which the comparative data on her individual cases are relegated to one of several appendices [Mead 1928b]). By the time she prepared the more scholarly version of her Samoan ethnography, the issue of quantitative data was no longer in evidence. Although Mead referred there to the opportunity "to measure the width and strength of the discrepancy between the ideal and the actual" (Mead 1930:5) as "the most valuable part of my ethnological research," we may assume that she was using the word measure in a non-quantitative sense, since neither the "rounded picture of Manuan society" offered in the first section of that book nor the appended chapters dealing with some topics in "more conventional fashion" show any explicitly quantitative concern with the frequency of behavior. In this respect, of course, Mead was in the main line of modern ethnographic methodology.

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RESEARCH IN PROGRESS

Richard Blench (Environmental Research Group, Oxford) and Richard Slobodin (Anthropology, McMaster) are researching the life and career of Northcote W. Thomas (1868-1936), folklorist, linguistic scholar, and colonial anthropologist.

Don D. Fowler (University of Nevada, Reno) is writing a book on the history of anthropology in the American Southwest (1846-1930).

Dong H. Ko (Washington University, St. Louis) is writing a doctoral dissertation on Eiichiro Ishida (1902-68), an important Japanese cultural anthropologist who was influenced by Marxism, culture-circle historical anthropology, and (after World War II), by Alfred Kroeber and Leiden structuralism.

Melbourne Tapper (University of Connecticut) is writing a doctoral dissertation on the cultural history of the S-hemoglobin gene with special emphasis on how such genetic syndromes are discursively constructed.

Maurice Mauviel (Laboratoire de psychologie appliquée aux Phénomènes Culturels, Université Paris V René Descartes) is pursuing research topics relating to the cultural anthropology of the idéologues, the culture concept in the work of A. Niceforo, and the general question of the relation of anthropology and literature, and would like to know of the work of other researchers who may be working on such questions.