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In addition to previously noted sources in the Bancroft Library of the University of California, Berkeley (HAN IV:2; I:1) there are several hundred letters relating to anthropology in the Phoebe Apperson Hearst Correspondence and Papers. The large collection (sixty boxes of correspondence and twenty cartons of subject files) was given to the library in 1972. A detailed guide has been prepared for the correspondence, most of which is incoming. The most relevant items seem to be:

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Although the reorganization of the American Anthropological Association in 1946 has been seen simply as the restructuring of a scholarly society along more professional lines, it had implications for anthropology's extra-disciplinary relations (Stocking, 1976). Julian H. Steward, chairman of the AAA Committee on Reorganization, is generally recognized as the principle author of that Committee's recommendations and in the letter reproduced below, he addresses a defense of these institutional changes to Alfred L. Kroeber, his old teacher. As it explicitly links reorganization to Steward's interpretation of "fundamental trends" of anthropology "in relation to the world," this letter contributes significantly to our understanding of this event.

World War II had brought an unprecedented flow of federal support to science, and the immediate postwar years saw the scientific community attempt to establish comparable support on a permanent peacetime basis through the establishment of a National Research Foundation (legislative forerunner of the National Science Foundation). From this the social sciences were initially excluded by the dominant physical sciences. Obliged to protest anthropology's status as a science, some members of the discipline saw the humanistic, historical and reformist orientations represented by Redfield, Kidder and Mead as a genuine liability. Steward had worked privately "to give anthropology a respected place as a basic research science with respect to the National Research Foundation"; the institutional changes he authored allowed anthropology more confidently to claim its support. Questioning the scientific merit of non-university research in general and of government research in particular, Kroeber sent a separate letter for Steward along with his response to the Committee on Reorganization's request for comments. Presumably intending to distinguish scientifically-motivated initiatives from those prompted by the mere presence of research opportunities, he characterized the reorganization effort as "mean-notived." As a native Washingtonian with a family history of federal service, Steward was clearly unembarrassed by his government connections. Protesting his own disinterestedness and documenting job openings in government, he drafted a response arguing that reorganization would benefit the entire discipline. Nevertheless, the ultimate justifi-
cation of his plan to "mobilize anthropology" did lie in the opportunities it might take advantage of. Perhaps feeling that a defense based ultimately on "bacon brought home" would not satisfy his mentor, Steward never mailed his letter. Undated and without a closing, it was placed in Steward's "Personal Correspondence" folder, rather than with the other reorganization materials (which are now at the National Anthropological Archives in Washington, D.C.). It may now be found in Box 13 of the Julian H. Steward papers at the University of Illinois Urbana-Champaign Archives (for details and general references see Hanc 1979). I would like to thank Mrs. Jane C. Steward, now of Waikiki, for permission to publish this letter.

[ca. August 1946]

Dear Kroeber:

Thanks for your very personal letter, and for the other about the reorganization of anthropology, which I have not yet answered. You seem to look at me askance, which I can well understand. When I wrote that I long for the simpler research and teaching niche, I stated a personal preference, not a conviction about fundamental trends of our science in relation to the world. As a matter of fact, my real trouble is that I am doing too much: a major research job in the Handbook [of South American Indians, BAE Bulletin 143] (my own and that of others); a major promotional and research planning job in the Institute; a fair teaching job, in that I not only spend a vast amount of time on the problems of my own personnel but actually devote 5 to 10 hours a week to odds and ends of other peoples' students who drift through; and a large number of miscellaneous chores on behalf of the profession because I happen to be a guy with a conscience about taking advantage of opportunities and with a little too much imagination to stop seeing opportunities. I may be destined for an occlusion; probably I'll get ulcers first, except that I can cut down on this too-full life, when the Handbook is finished, and I probably shall.

I give you this about myself partly because I know your sympathetic interest, partly to explain these "mean-motived" situations and to ask how one could do otherwise. A bunch of scholars running a journal and handing out honors have to be financed. The better they succeed in their scholastic niches, the greater the need for an outlet for their students. I figure that if anthropology is going to be effective, it should be brought into all possible situations. You who have taught it these many years have done such a good job that it is in far wider demand than anyone could have dreamed 10 or 15 years ago. And yet, people now rising to administrative positions who know enough about it to want it in research and other jobs are continually turned back because they cannot find anyone who can help them put it over. Naturally, I see the situation from the point of view of the Federal Government. It happens that the government has
become an important employer, whatever one feels about the prop(ri)ety of the fact. But I daresay that the federal recognition of anthropology extends also to state and private spheres.

Why then mobilize anthropology? If my vindication is proportionate to the amount of bacon brought home, let me sketch a few developments that have already taken place: situations where I feel I have already brought the bacon home without any real help from the profession, but could have done an infinitely better job with help, and at less expense to my digestion. First, the Valley Authority archeology. A year ago last December it occurred to me that 9/10 of the best archeology in the U.S. would be lost forever if something were not done. Where was the AAA or the SAA [Society for American Archaeology]? The one had not the tradition of looking ahead; the other was dependent on amateurs who had to be kept out of the projects, lest we have a repetition of WPA [Works Progress Administration].

First, I prodded the SI [Smithsonian Institution], but it was dead; then the Basic Needs Committee of the NRC, but it couldn't do the necessary lobbying. Withal, it took me five months of prodding and manouevering to set up a Committee that could act: it finally covered the SAA, the ACLS [American Council of Learned Societies], the AAA and the SI. The SI being involved, I had to fade from the picture. Nonetheless, the net result is that the Committee is about to get some $100,000 for this archeology for next year, with further sums in the future. The Valley Authority archeology is of no particular moment to me, and by now no one even identifies me with it, except the Committee members. Perhaps I should have tended my own knitting. But what does one do when an opportunity arises? Had there been a mobilized profession to which I could have referred it—a profession organized to take responsibility on behalf of the profession, rather than on behalf of individuals, as at present—I would have had little trouble. In retrospect, now that the situation is well under control, I know that many valleys will be flooded before the material is dug precisely because of that five months delay in getting started. In such situations, my inclination is to be the cloistered scholar, and that is why I would like to get into a more routine life.

I could also cite you the work I went to last summer to give anthropology a respected place as a basic research science with respect to the National Research Foundation bill, but had to give it up because it was one more job than I could carry. I groped for support from the profession, but it was not there. I could cite the requests from the State Department's Office of the Geographer [for] anthropological help, from every division of Agriculture for assistance in introducing anthropologists and their techniques, from State and War in helping develop anthropology as the core of regional training for their foreign personnel, and from the Pan-American Institute of Geography and History that wants to make a major place for anthropology, not only because of its basic research value but because it recognizes anthropology's value respecting its immediate project which is that of working toward the 1950 census. I might even cite the Institute of Social Anthropology, which I created with my bare
hands, despite the decadence of the SI and without the least help from the profession, though I sought it repeatedly.

Naturally, I speak of government situations, for these are the ones I know. If anthropology in the future had only to deal with government, there would still be a place for a professional group that could relieve individuals like myself from these chores. But I feel quite certain that one cannot distinguish government from the rest of the country. What I am pleading for is a professional basis for our future; a group that can choose its own representatives and charge them with the responsibility of looking ahead, planning, and working on behalf of their colleagues rather than on their own behalf. I think I have not lost my perspective so much as you may believe. I am protesting the archaic organization that elects presidents like Redfield, who doesn't attend meetings, like Kidder, who doesn't give a damn because he has security in his own corner, and like Cooper, who protests democracy but won't give up one little bit of power because he doesn't really trust younger men. I am protesting the kind of reaction we got from New Mexico, which said, "Why bother us with this nonsense? We have good jobs and don't need to worry. Besides, we think government anthropology stinks," and to which I had to reply, "If you are not interested in helping develop jobs to get your students employment, don't you think you had better tell them that before they become anthropology majors?" I am protesting the kind of personal promotion that anthropology, like everything else in this life, is so easily given to, exemplified by Margaret Mead and certain others, who were all with us at first, trying to use this movement as a device to develop their own special interests. You would probably call the last "pressure groups." Of course they are. It would be very naive to suppose that anthropology does not have its pressure groups. The sole difference between the existing situation and what I am trying to achieve for the profession is to iron out the pressure groups to a slight extent and to line up these groups on behalf of common interest.

As the situation now stands, we have a certain support beyond that which individual institutions can give their own. You know quite well that that support is subject to existing pressure groups. If I could spend an evening with you I would like to relate what I have picked up about the institutions that have come to dominate the SSRC, the Rockefeller, the Viking Fund, the Indian Office, and the various other government agencies; the morbid sense of enmity and competition between Linton and Chicago, between Yale and Columbia, etc. If our proposal is so ill-advised that it will actually enhance these pressure groups and enmities, we are all glad to discuss particulars. At worst, any new organization of anthropologists will not create pressure groups; it will merely give them expression. At best, it will cut across such groups and provide a means whereby the youngsters—not those in power with too much to lose—can elect, without being told by a committee whom to elect and without embarrassment, those whom they trust.

You undoubtedly see in this letter the pattern of the government. Naturally, I recognize that I look at the situation from this point of
view. Still, I can't believe that the government pattern is wholly out of step with private and local patterns. Least of all am I willing to concede that motives must be mean. In that case, we would have to say that the opponents are not better motivated than the proponents.

This letter has undoubtedly assigned me to a certain role: that of being a terrific busy-body, who, living in Washington, can't help but get mixed up in all sorts of things. Very true, and it has taken a certain toll. But practically every day I am faced with the question of whether I shall say, "To hell with it," at the expense, I can conservatively say for the past year, of about $300,000 for anthropological work or jobs for anthropologists, or cry out for help from the profession. Our esteemed colleagues work at these situations privately, for themselves or their institutions. I think the Valley Authority example exonerates me from such motives. I simply want help that does not come from special pressure groups. If I am on the wrong track, I would like to know in very specific terms how you answer these problems, how you get these jobs done, and how you avoid pressure groups, be they local or otherwise, without selling out to those which exist.

At this point in time anthropology's major generational cleavage concerned its practical value. For historically-minded elders this lay in criticism of current popular assumption, while the more scientific junior generation felt anthropology had something more directly useful to offer. Steward assumed that anthropology could be "effective," though he in fact would have restricted it to an advisory role. By taking nominations out of committee and opening them to the fellows of the Association, he hoped to enable "the youngsters" to elect "those whom they please," and give control of the AAA to a generation eager to put anthropology to work.

Steward was clearly unembarrassed by his unalloyed attention to jobs and funds. Measured against an ideal of scientific disinterestedness this may seem mean-motived indeed, but in appraising this letter it should be recognized that such candid concern was quite common in the immediate postwar years. The proposed NSF called up a prospect of unprecedented progress and congressional attempts to make it politically "responsive" (rather than "insular") were perceived as threats to science itself (Kevles 1977). Organized science's political interests were conflated with the advancement of science and many scientists became open and active partisans. In this context Steward's preoccupation with support for anthropology seems neither exceptional nor excessive.

Although the vigorous non-academic anthropology envisioned by Steward did not materialize, the federal support he hoped for eventually did. True, the great expansion of academic opportunity in the 1950s submerged anthropologists' status as professionals in their identity as scholars (Steward eventually found his own scholastic niche in rural central Illinois). Anthropological research, however, was largely sustained by an interrelated system of government, universities and organized science into which anthropologists (and other social scientists) were integrated as professionals. Recently, events external to anthropology have
prompted criticism of this relationship. Professionalism per se, however, has not been attacked, and professional identity may be strengthened as a new crisis in jobs leads to a reconsideration of the non-academic uses of anthropology.

From this perspective, Steward's concerns in pushing the reorganization of the Association seems much more significant than perhaps they did to Kroeber.


CLIO'S FANCY: DOCUMENTS TO PIQUE THE HISTORICAL IMAGINATION

"THE INTENSIVE STUDY OF LIMITED AREAS"--TOWARD AN ETHNOGRAPHIC CONTEXT FOR THE MALINOWSKIAN INNOVATION

Although American anthropologists might contest the honor, in favor of Boas or Cushing, the founding of the modern fieldwork tradition in anthropology is still--despite the shocked reaction to his diaries--usually attributed to Bronislaw Malinowski. True, there is general recognition that Alfred Cort Haddon's Torres Straits Expedition and Williams Rivers' "genealogical method" had previously established an international reputation for "the Cambridge School" of anthropology. However, the ethnographic context of Malinowski's innovation has not been investigated in detail. As the following draft of a testimonial letter by Haddon in 1908 suggests, Malinowski's work in the Trobriand Islands between 1915 and 1918 was as much the culmination of a Torres Straits ethnographic tradition as it was the starting point of a modern functionalist one. (The original is in the Haddon papers in the Cambridge University Library and is reproduced with the permission of Haddon's son, Ernest.)

The investigation of the uncivilized races is now a matter of urgent necessity, owing to their contact with Europeans and others, which results either in their extermination or in the modification of
their former handicrafts, customs and beliefs. Such investigations are essential for ethnology, sociology, psychology, comparative religion, linguistics and other sciences, and they cannot fail to throw light, by analogy, or history in general. The information to be obtained is of great value to all who are interested in the culture, customs, ideas and ideals of mankind. The work must be done immediately as the data are becoming modified or obliterated. The investigations must be thoroughly made by trained and competent men.

The time has passed when students were satisfied with general accounts of native races made by the passing traveller or the untrained and frequently unsympathetic resident. Our watchword must now be "the intensive study of limited areas." We require to know all the conditions of existence of a given people. How the environment affects them, how they react on it. But above all we need an accurate and exhaustive study of the psychology, sociology and religion of the people studied. In the genealogical method of investigation introduced by Dr. Rivers we have a most valuable instrument for the recording of kinship terms and relationships, social structure, social functions and other data, which has already yielded extraordinarily fruitful results.

I am of the opinion that Dr. Gunnar Landtman and Dr. Rafael Karsten are by their training and ability thoroughly qualified to undertake investigations of this nature, and I feel sure that, given the opportunity, they will make memorable contributions to science.

The need for such investigation is so pressing everywhere that it is difficult to advise where it should be undertaken. Perhaps the best general rule to follow is to determine where the modification and disintegration due to the contact of civilisation are most pronounced and rapid. This is usually the case in numerically small communities—especially in islands. This process has been most marked in Oceania, and over nearly the whole of Polynesia and Micronesia it is practically too late to do much in the way of recording new ethnological data. Melanesia is becoming rapidly modified, and I would suggest that parts of Melanesia should be selected—for example the Echiquier, Hermit, or parts of the Admiralty islands, or the northern Solomon Islands, would probably be favourable fields for enquiry, or anywhere in the Bismarck Archipelago. In the South, New Caledonia is very little known. Very much remains to be done in New Guinea. Western Australia is a virgin field. The sociology and religion of all jungle tribes are worth study. Much has yet to be learnt about the Semang and Sakai of the Malay Peninsula—the Funans, etc. of Borneo, and about many of the jungle tribes of India. We really know nothing of importance about the pygmies of the Central African forests. These are only a few of the problems awaiting solution, and I sincerely hope that my friends and pupils, Drs. Landtm an and Karsten, may have an opportunity of enriching science by an "intensive study of a limited area."

A. C. Haddon, Sc.D., F.R.S.
Lecturer in Ethnology in the Universities of Cambridge and London

July 20, 1908
There are also large areas in South America concerning the ethnology of which we know very little. A great deal remains to be done in Brazil. Of more pressing need is the investigation of the inhabitants of the Gran Chaco and neighbouring districts, as this fine healthy country is being rapidly affected by European influence.

The two Finnish ethnographers commended by Haddon were (as Malinowski was shortly to be) students of Edward Westermarck, who was then teaching both at the London School of Economics and the University of Helsingfors and who had himself carried out extended fieldwork in Morocco. Having already won their doctorates for sociological topics at the University of Helsingfors, they had (as Malinowski was shortly to do) come to England for training with the members of "the Cambridge School." Although their departure was delayed for several years, they both did in fact get off to the field—Landtman in 1910 to the Kiwai area on the Gulf of Papua; Karsten in 1911 to the Gran Chaco region mentioned in Haddon's postscript. Each of their expeditions lasted for two years, and each stayed for extended periods with particular groups. Landtman's correspondence with Haddon indicates he was nine months in a single village, and Karsten, who seems to have had an almost Malinowskian linguistic facility, learned two unrelated Indian languages.

Given the general similarity of purpose, and at least superficial similarity of style, we may reasonably ask why the "invention" of modern fieldwork should be associated with Malinowski rather than these two Finns. Part of the answer is no doubt attributable to their national self-affirmation: unlike Malinowski, who forsook his native Poland, both men returned to Helsingfors, where Karsten succeeded Westermarck in the chair of Moral Philosophy and Landtman became the first professor of sociology. Another factor was perhaps delay in publication. Although Karsten was eventually to publish numerous works in South American ethnography, and Landtman published a lengthy monograph on Kiwai before turning to more traditionally sociological problems, none of their major ethnographic writings appeared until several years after Malinowski's Argonauts. Involved in extended researches among the Jivaro of Ecuador between 1916 and 1919, Karsten did not publish a major work in English until 1926. Landtman's Kiwai monograph did not reach print until 1927, after an odyssey which included the loss of his fieldnotes in a shipwreck in the North Sea and their subsequent recovery by a hired diver. When Malinowski reviewed it in 1929, he called Landtman "one of the masters of the modern sociological method of fieldwork"—neglecting to mention that the fieldwork had in fact been completed two years before he himself left his armchair in the British Museum.

By this time, Malinowski's association with the modern fieldwork style had already been established—largely, one suspects, as a result of such factors as his literary gift, his flair for self-dramatization, his loudly trumpeted association with a new theoretical viewpoint, and most importantly, his methodological self-consciousness. Although Karsten did offer a kind of running traveller's account of the circumstances of his fieldwork in South America, there is nothing in either Finn's ethnography to match the opening chapter of Argonauts. If we know now that this is a
somewhat idealized version of Malinowski's actual ethnographic practice, that in a sense is precisely the point. Borrowing elsewhere from Malinowski's writings, we might say that he provided the mythical charter for the social institution of fieldwork—or, in Kuhnian terms, the concrete exemplar of practice around which the new paradigm could be institutionalized. It is in this context that we quite properly associate the modern fieldwork tradition with his name, rather than with Landtman, Karsten, or any of the other young men who answered Haddon's call for "the intensive study of limited areas."

(G.W.S.)

BIBLIOGRAPHICA ARCANA

I. ANTHROPOLOGY AT CHICAGO

For the fiftieth anniversary of the establishment of the Department of Anthropology as a separate unit at the University of Chicago, George Stocking has prepared an exhibition of documents in the Special Collections Department of the Regenstein Library which will run through January 1980. Stocking has authored a 56-page brochure for the exhibit entitled Anthropology at Chicago: Tradition, Discipline, Department, which contains 24 full-page illustrations and a 16,000 word text offering a history of anthropological work at Chicago from 1892 to the present. Although the brochure will not be distributed through normal channels, copies are available for $4.00 plus postage from the Department of Anthropology, 101 Haskell Hall, University of Chicago, Chicago, Illinois 60637. Checks (in U.S. dollars) should be made out to the Department of Anthropology, University of Chicago; prepaid orders will be sent postage free.

II. GRADUATE STUDENT JOURNALS

Past numbers of HAN have included listings of articles in the history of anthropology from several graduate student publications. Joseph Hanc and Bill Sturtevant offer the following additions:

A. Anthropology UCLA (Department of Anthropology, University of California, Los Angeles):


Vol. 8, nos. 1 & 2, 1976. Paths to the Symbolic Self, Essays in Honor of Walter Goldschmidt


Ralph L. Beals, "Anthropology and Government: Unwilling Bridegroom or Reluctant Bride," pp. 159-173 (very useful; con-
tains most of the information in Beals' memo "Anthropology During the War and After" in a more quotable form as well as new information on the Field Division of Education of the National Park Service, Collier's BIA, and the War Relocation Authority--J.H.

B. Journal of the Steward Anthropological Society (Department of Anthropology, University of Illinois, Urbana):


Alexander Goldenweiser, "The Sociological Thought of William Wundt," pp. 175-186 (previously unpublished, this paper appears to have been prepared for the volume of readings Contemporary Social Theory, Harry Elmer Barnes, Howard Becker and Francis Bennet Becker (eds.), Appleton-Century, N.Y.C., 1940).

C. Kroeber Anthropological Society Papers (Department of Anthropology, University of California, Berkeley):


R. Berkeley Miller, "Anthropology and Institutionalization: Frederick Starr at the University of Chicago, 1892-1923," pp. 49-60 (blames lack of institutionalization of anthropology at Chicago in this period on Starr's personal characteristics--W.C.S.).


III. RECENT DOCTORAL DISSERTATIONS

Susan Dwyer-Shick, now at Pennsylvania State University, has completed her dissertation on "The American Folklore Society and Folklore Research in America, 1888-1940" (University of Pennsylvania, Department of Folklore and Folklife, 1979).
IV. SUGGESTED BY OUR READERS


Polese, Richard, introduction. The Malaspina Expedition, "In the Pursuit of Knowledge..." (Santa Fe, 1977). [Illustrated catalogue with useful scholarly articles on late eighteenth century Spanish expedition to California and Northwest coast--R.D.F.]


GLEANINGS FROM ACADEMIC GATHERINGS

American Anthropological Association. 78th Meeting, Cincinnati, November 27-December 1, 1979. The meetings this year included four sessions relating to the history of anthropology, as well as a number of relevant individual papers. The session on "Development of Anthropology: National and Conceptual Viewpoints" included papers by Norris Brock (University of North Carolina, Chapel Hill), "The Concept of the Humanities in Cultural Anthropology"; R. J. Duncan (Inter-American), "The Role of Puerto Rico in the Development of Anthropological Theory"; Elvin Hatch (University of California, Santa Barbara), "Ethical Relativism in American Anthropology"; John Johnsen (Utica), "Towards a Marxist Assessment of Historical Particularism"; Benson Saler (Brandeis), "Eisner's Particularism and Participation"; Stan Wilk (Lycoming), "Clifford Geertz and the History of Anthropology." The session on "Pioneers in Anthropology" included papers by Douglas Caulkins (Grinnell), "Eilert Sundt: Networks in Early Norwegian Ethnology"; Mary Druke (Newberry Library), "Eighteenth Century 'Fieldworkers' in Eastern North America"; Harvey Goldberg (Hebrew University, Jerusalem), "A Proto- Anthropologist in North Africa"; Theresa M. Kelly (University of Texas, San Antonio), "Wordsworth, Geology, Evolution"; Frank Loveland (Gettysburg), "Stephen Peet (1831-1914) and the First American Anthropological Association"; Donald Tumasonis (Norway), "Shirokogoroff's Influence on Ethos Theory." The "Margaret Mead Memorial" included papers by Miriam
Birdwhistell (Virginia), Dorothy Billings (Wichita State), Joan Campbell and Patricia Grinager (Wisconsin-Milwaukee). The two-part session on "Theoretical and Ethnographic Attention on Missionaries" included, among others, Thomas Biedelman (New York University), "Transcendental Romanticism versus Applied Pragmatism: Contradictions in the Self-Image and Behavior of Christian Missionaries in Nineteenth Century East Africa"; Elmer Miller (Temple), "Great Was the Company of the Preachers: the Word of Missionaries and the Word of Anthropologists"; Judith Shapiro (Bryn Mawr), "Ideologies of Catholic Missionary Practice in a Post-Colonial Era." Miscellaneous papers of historical interest included Karl Heider (South Carolina), "Pattern Theory"; Egon Renner (West Germany), "Cognitive Anthropology as an Anthropological Paradigm and the Paradigmatic Development in Cultural Anthropology"; Eugene Ruyle (California State University, Long Beach), "The Potlatch Myth: a Critical Essay on the Ethnographic Record"; Satya Sharma (Saskatchewan) "Cultural Relativism: a Benevolent Scientific Concept or a Status-Quo Maintenance Mechanism and a Potentially Exploitative Ideology?" Information is taken from the Abstracts of the 78th Annual Meeting and further inquiries would be best directed to the program chairman, Thomas Greaves at the University of Texas, San Antonio (G.W.S.).

American Studies Association, Seventh Biennial Convention, Minneapolis, September 27-30, 1979. Two papers of interest in the history of anthropology: "Gregory Bateson, Steps to an Ecology of Mind," by Jay Mechling (University of California, Davis) and "Clifford Geertz, the Interpretation of Cultures," by Karen Lystra (California State University, Fullerton), were given in a session organized by Mechling (R.E.B.).

American Society of Ethnohistory, Twenty-Seventh Meeting, Albany, New York, October 11-13, 1979. Three papers in three different sessions with a history of anthropology focus were those of: Robert E. Bieder (University of Illinois, Chicago Circle), "The Grand Order of the Iroquois: the Ethnographic Investigations of Isaac Hurd and Lewis Henry Morgan"; Jack A. Lucas (Central Connecticut State College), "Science, History, Philosophy: Old Themes Revisited through Austrian Ethnohistory"; and Paul Leser (University of Hartford), "Comments on Some Culture-historical Anthropologists," which dealt with the works of Ankermann, Struck, Baumann and G. A. Schmitz. There was also a session organized by Elisabeth Tooker (Temple University) on "A Half Century of Iroquoian Research" which included papers by William Sturtevant (Smithsonian Institution), Elisabeth Tooker, Wallace Chafe (University of California and Wellesley College), Hazel Hertzberg (Teacher's College, Columbia University), James Wright (National Museum of Man-Canada), and Bruce Trigger (McGill University) (R.E.B.).

XLIII International Congress of Americanists, Vancouver, August 11-17, 1979. Although most of the papers focused primarily upon historical and anthropological issues in the Americas, a few were relevant to the history of anthropology. A session devoted to the "Heritage of Conquest," included a paper by John Hawkins (Brigham Young University) entitled "Redfield's Culture Concept and Mesoamerican Research." In the Ethnohistory/History Section, there were two papers of special interest: one by Leonid A. H. Shur (Hebrew University, Jerusalem) entitled "Russian Unpublished Sources on the History and Ethnology of North-Western America

Fifth International Congress on the Enlightenment, Pisa, Italy, August 27 to September 2, 1979. There was a double session on "Anthropology and Linguistics," including papers by G. Barsanti (Florence), "L'uomo e le classificazioni: Aspetti del dibattito antropologico nelle scienze naturali tra Buffon e Lamarck"; C. Biondi (Parma), "L'immagine del nero nella letteratura francese dell'ultimo Settecento"; F. Crispini (Calabria), "Mostri e mostruosità. Un problema delle 'sciences de la vie' da Diderot a J. Geoffroy Saint-Hilaire"; S. Moravia (Florence), "La nascita della 'science de l'homme' nel secolo XVIII"; L. Sozzi (Turin), "Bougainville et les sauvages"; M. Staum (Calgary), "The Class of Moral and Political Sciences"; and Robert Wokler (Manchester), "The Ape Debates in Enlightenment Anthropology" (R.W.).

Social Science History Association, Cambridge, Massachusetts, November 1-4, 1979, included a session on "The Social Context of Anthropological Theory: Case Studies in Nineteenth and Twentieth Century Britain," with papers by Henrika Kuklick (University of Pennsylvania), "The Savages Within and Without: Political Uses of British Anthropology, 1900-1945," and George Stocking (University of Chicago), "The Ulterior Motives of Victorian Social Evolutionism" (unfortunately, the latter is not available at this time for distribution—G.W.S.).