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ABSTRACT

The purpose of this research project was to identify ethnographers who had used computers in the field to record and analyze qualitative data. To locate such individuals a literature search was conducted, an announcement was placed in the "Anthropology Newsletter," anthropology computer specialists were contacted, a special symposium was conducted, and questionnaires were sent to anthropology department heads. The results were all negative. Excerpts provided from the letters from the anthropology department heads show that computers are used either in conjunction with textbooks or sometimes in field work, but not both. In the field, usage involves statistical analysis, rather than recording of qualitative field notes. The research project also surveyed recent Ph.D.'s in anthropology to determine the methods they use in the field to record and process their field notes. Vignettes from that survey and a paper describing the results are provided in the appendices. Additional items in the appendices are a bibliography of anthropological uses of computers and the field notes survey instrument. (RM)

F I N A L R E P O R T

"Computers in Ethnographic Research"

by

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NIE GRANT G-78-0062

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TABLE OF CONTENTS

BACKGROUND TO THE PROBLEM: GENERAL COMMENTS

The hallmark of ethnography in this century, following the Malinowskian model, has been the description of cultures in all their richness, diversity, and complexity. The primary emphasis in ethnographic research has been placed on holistic and contextual analyses of human beliefs and behavior. Accordingly, insights and understanding are thought to result from a deep and preferably long immersion in the way of life of the group being studied. The ethnographer working in this tradition attempts to gain for himself ---and to convey to others through his ethnographies---a "feel" for the culture and knowledge about what it is like to live as a Trobriander, an Eskimo, or a Nuer, for example. The ethnographer learns by "walking in someone else's shoes," and he (or she) transmits what has been learned by creating "thick descriptions" of the culture that has been experienced. For some anthropologists, such as Ward Goodenough, the anthropologist has done his job well when he is no longer surprised by the behavior of the people he is studying and when he is able to perform appropriately in the culture: he no longer surprises the "natives" by his actions.

The traditional ethnographer did not require much, if any, training in field techniques and methods; indeed, for this type of research, all that was needed to be successful was intensive participation plus sensitivity and perceptiveness on the part of the field worker. Although taking a census and eliciting genealogies may have been included in the tool kit employed for gathering data, on the whole most data were obtained through qualitative methods such as observation and informal interviewing.

But, increasingly anthropologists have been rejecting the Malinowskian method of field work, turning from general, descriptive studies to focused ethnography and the testing of hypotheses. Concurrently, they have been adding to their repertoire a broad array of tests and procedures that yield quantitative data. In the decades following World War II, there occurred a dramatic increase in quantitative articles published in the major anthropological journals. Such articles had been practically nonexistent in 1945. This continuing trend notwithstanding, at the present time publications based on qualitative research methods still outnumber those based on quantitative methods. Indeed, even articles which can be classified as "quantitative" tend to rely heavily on qualitative methods for some types of data and complementary analyses. Moreover, significant segments of the profession, those with a humanistic orientation particularly, still eschew all forms of quantitative research.

Numerous criticisms of qualitative approaches to ethnography can be made. The contemporary Mead-Freeman controversy over the interpretation of the ethnographic reality in Samoa is a good example of the kinds of problems that are generated by the over-reliance on qualitative materials and the lack of solid, replicable quantitative methods. In other words, the results are inherently debatable, ad infinitum. But other drawbacks also exist. Such work tends to be

costly and time-consuming; good ethnographies require usually a minimum of a year of field work and another year or more devoted to analysis and writing. Ethnographies based on qualitative methods generally are atheoretical, focused on the unique aspects of the situation being studied, whether village or classroom, rather than on issues of much broader concern. Often one does not know how the ethnographer "knows" what he or she reports, and suspicion of impressionistic overstatement based on an inadequate amount of evidence is warranted. Furthermore, qualitative data are inherently difficult to handle, and established criteria for analyzing such materials are lacking (the canons of good research in ethnography are incredibly loose and unspecified, to a degree that would be found ludicrous by scholars who are familiar with the rigor involved in historical research). The raw data normally are available only to the ethnographers who recorded them, and they are not available to be subjected to reanalysis by others or to be used in comparative research.

At the same time, many anthropologists would argue that anthropologists excel at the use of qualitative methods and that what distinguishes the discipline from the other social sciences is precisely this reliance on qualitative methods such as observation, participation, and unstructured interviewing, and the basing of one's conclusions on a rich understanding of total context. Additionally, they might argue that the value of qualitative ethnographic work resides especially in its unstructured, open-ended, and exploratory nature. It is an approach which encourages discovery, intuitive understandings, and inductive insights. It is an appropriate approach for a science in the natural history phase. There is a certain validity to such claims.

After a long and distinguished career, the eminent American anthropologist, George Peter Murdock, concluded that the main contribution that anthropologists have made, the lasting monument to their efforts, has been not in the area of theory but rather in the ethnographic corpus that they have generated. Given the emphasis on theory in the discipline, this kind of conclusion is somewhat shocking. But Murdock may be right: theories may come and go, but the descriptions of cultures, many of which have subsequently disappeared or drastically changed, that anthropologists have produced will survive.

In view of the above considerations, it seems reasonable to suggest that what is needed is greater attention to the correction of problems associated with qualitative approaches to ethnography. The question that the discipline must face is, Can ways be found to eliminate the negative aspects of qualitative research? That is, can an improvement in qualitative methods, as a significant complement to quantitative methods lead to the following results:

a) a reduction in the time and costs involved in collecting and analyzing data;

b) an increase in information-sharing with colleagues doing similar work or involved

in comparative analyses;

c) better control over the quality and the depth of detail of data used in writing an ethnography;

d) the creation of alternative means of presenting results;

e) making the data available to a broader audience, especially to groups having a policy interest in the data, and to posterity in the form of a permanent field work record.

It is at this point that the computer enters the picture. It is entirely possible that the use of computers in ethnographic work may allow the discipline to solve some of the above-mentioned problems, but before discussing this matter, a case history of the difficulties involved in attempting to improve ethnographic work through convential methods may help to highlight the problems and to underscore the need to search for appropriate technological solutions to the serious problem of data quality control in ethnographic research.

BACKGROUND TO THE PROBLEM: A CASE HISTORY

When I returned from Peru in 1971 upon completion of ethnographic field research for my dissertation, I discovered that in a two-year period an enormous amount of data had been collected, indeed an almost unmanageable quantity. My wife, Charlene Bolton, and I had typed our field notes on 5x8 cards, and our file consisted of more than 8,000 such cards with single-spaced text (approximately 1,600,000 words). We had also employed three native assistants for most of the field period, and they had produced dozens of notebooks filled with their observations on the village we were studying as a team (particularly valuable for an "emic" perspective on the culture). Additionally, we had carried out a comprehensive census of the community (250 households), obtaining reproductive histories on all women in these same interviews, and we had administered several questionnaires to samples of varying sizes. Finally, we had borrowed thousands of village documents (e.g., wills, minutes of community meetings, papers dealing with disputes and other legal matters, and so forth), and all of these we had typed single-spaced on legal-size paper---for a total of more than 10,000 pages in documentary materials alone. Collecting information was not terribly difficult, but the expense of transporting this material home began to give me some insight into the information "overload" we had developed.

My wife occasionally questioned my omnivorous, Boasian approach to data gathering, but I operated on the principle that any and all kinds of information about the community might eventually prove useful in our attempts to understand the culture and behavior of the people in this village, particularly their involvement in aggressive

action and interpersonal conflict. Franz Boas has been laughed at and castigated over the years by anthropologists because he collected a lot of trivia, so-called. But what is trivia is determined at least in part, perhaps significantly, by theoretical perspective and the questions in vogue at any given time. It is often not possible to know in advance what type of information might contain the key to the solution of an intellectual problem: the serendipity factor comes into play here, and the true merit in qualitative approaches in anthropology may lie precisely in their greater openness and therefore their serendipitous potential.

Perhaps it is possible to have "too much" information, but "too much" is a concept that is meaningful only in relation to the ability of the analyst to process the information; generally, the more information one's theorizing is based on, the better. Certainly, too much information is likely to be better than too little. But, of course, information that is too voluminous to process is worthless, or worse, because of the associated costs in time and money required to collect it--and to the possible paralyzing effect it may have on the researcher's efforts to analyze it. Data do not speak for themselves. They must be analyzed. Given time constraints (degree deadlines, publication pressures, and so forth), the corpus of data gathered by my field team could not be digested thoroughly in writing a dissertation, and only a small chunk was "bitten off" for that purpose; the rest was there for future analyses on a variety of other topics. The serious problem with the masses of data in my possession (estimated at more than 6,000,000 words in textual materials alone) was one of accessibility. It turned out to be extremely difficult to retrive information efficiently from such a large corpus. My own and my wife's field notes had been coded in the field, using the Murdock/HRAF system, with multiple codes entered on each card as appropriate. This coding, however, was not sufficiently detailed to allow retrieval of all the relevant information on a given topic, we found. The field notes of my assistants, the documents that had been copied, and so on, had not been coded at all. Hunting through the bound volumes of materials was like panning for gold; it was tedious and time-consuming, but it occasionally yielded valuable nuggets. In this situation, the unfortunate choice becomes one of analyzing and presenting the results of an analysis of a topic without worrying about having utilized all the pertinent information in one's field materials or of carrying out many fewer studies and basing them on a meticulous search for all of the relevant material--the nuggets and the dust--in one's field corpus. The real solution to this dilemma, it would seem, is to improve accessibility.

An initial attempt to improve accessibility to a larger proportion of the corpus was made when bilingual work-study assistants became available to me. They were employed to read and code more finely my own and my wife's field notes and to code for the first time some of the other materials. Once coded, however, it was obvious that this was not enough. What was needed was an index. It is cumbersome to leaf through 18,000 pages to find information on a topic about which one is writing. The solution to this problem came when I participated in a seminar on the uses of APL that was held for faculty members at Pomona

College. As part of that seminar, a student and I developed an index for the corpus. This index lists the page numbers in the field notes containing information on a specified topic. For example, code #501 deals with boats and code #513 with sleeping. The pages on which there is some information on these topics are indicated below. Since these were not the focus of our research, only a few pages are listed. Code #578, in contrast, ingroup antagonisms, was related to a focus of our work and the index shows 32 lines of page numbers in connection with this code number, in many cases with entries indicating chunks of field note pages (note: this discussion is based on the index for our personal field notes alone, not the notes typed on legal paper nor the field assistants' notebooks).

CODE	PAGES
501 (boats)	52 53 69 75 97 100 292 302 303 307 336 358 364 366 377 383 417 426 437 442 475 486 499 626 869 1010 1025 1121 1122 1129 1131 1235-1241 1246 1248 1287 1629 1946 2380 2387 2543 2560 2601-2603 2608 2919 2980 3001 3008 3113 5104 5105 5198 5275 5399 5652
513 (sleeping)	237 385 470 486 1252 1575 1902 1939 2633 2647 2648 2656 2664 2672 2681 2748 2750 2927 4483 4491 4492 4501 5104-5107 5120 5436 5566

Consequently, it is now possible to look up information by going to the index (24 pages in length), finding the appropriate code number, and then checking the pages indicated by that code. Information that has not been coded properly will be missed, to be sure, but accessibility to coded topics is vastly improved. Such an index could have been created by hand, but using the computer to create the index has significant advantages. For instance, one can go back and re-code a page, adding new code numbers, deleting inappropriate ones, and so on, and then have the computer print out an up-dated, correct version at very little cost. Doing that by hand would be onerous. Moreover, kept on file in the computer, one could custom design searches, e.g., one that looks for the page numbers indicating pages on which both boats and sleeping are mentioned. Then, too, using an index eliminates the practice of some ethnographers of duplicating pages and inserting them in several places in their file. Such a system is feasible if the corpus is small but it becomes highly cumberson when large amounts of data are involved. With the present system, one simply numbers all the pages and then retrieves them by number as needed.

This is a simple use of the computer, but one that helps the ethnographer to manage a large data base which includes an immense

body of qualitative information. This project has given me much greater access to my data that I otherwise would have, and therefore it not only enhances productivity but enables me to base my analyses on all the information in my possession rather than the most accessible segment. Yet, it must be concluded that this system is primitive. Given the technological revolution that has taken place, especially the microcomputer revolution, I would never again engage in large-scale, intensive ethnographic data gathering without putting the information itself in machine-readable form from the outset, i.e., in the field. By "computerizing" the data from the beginning one could markedly increase accessibility, having the computer do the actual retrieving of data from the field note corpus, rather than simply pointing to the printed pages on which the pertinent information is to be found. In most cases, it is likely to be too expensive for anthropologists to convert qualitative field notes gathered in traditional ways into a proper format for computerized retrieval, but creating indices to their field notes, if a large corpus is involved, may help them, nonetheless, to utilize those materials more efficiently.

This case study illustrates the kinds of problems that motivated the current research project discussed in the remainder of this report. Ethnographic field work in the future should look somewhat different from what it looked like in the past. The incorporation of computers in the research process from the outset should have a significant impact on the quality of research done by anthropologists.

Although there has been a considerable increase in methodological discussions in the anthropological literature during the past fifteen years, few authors have discussed methods of recording and analyzing qualitative data. To be sure, "problems" of field work have been dealt with, usually such matters as how to dress, what to eat, how to establish rapport, and so forth. But it is almost impossible to locate indepth discussion of how to record qualitative information and how to process it. Satisfactory information about how ethnographers actually record and process their qualitative data is simply not available. However, it can be presumed that they continue to use pencil-and-notebook techniques for writing down what they learn through observations or interviews. Probably many field workers type up notes from notebooks, i.e., on cards or sheets of paper. In some cases, note cards are coded according to some system such as Murdock's, mentioned earlier. This much, but not more, can be gleaned from methods accounts.

While such methods may have been justifiable, appropriate, and even inevitable when most ethnographic research was done in small, isolated communities, they are hardly satisfactory under changing conditions in which the discipline finds itself today, accused of concentrating on the esoteric and of producing results that cannot be replicated by other investigators. More work is being done now in industrial, urban settings, more work is part of larger research efforts that are interdisciplinary and that involve teams of investigators,

and as anthropologists find it necessary to seek employment outside academia, more research will need to accommodate to demands for greater applied effectiveness and scientific validity. Consequently, it is absolutely essential that anthropologists develop improved methods for handling qualitative data. Advances in computer technology, both in hardware and software, should make it possible to implement the needed improvements.

The fundamental objective of the present project was to seek out information from and about anthropologists who have experimented with new methods of recording and processing qualitative field data insofar as it involved the use of computers, particularly in the field situation. The goal was to find out where things stood at the time this grant was approved--what had already been done. And, a second objective was to obtain some data that might cast light on the possibilities, prospects, and problems likely to be encountered in the future as anthropologists moved in the direction of computerizing field data. Following some comments on the uses of computers in anthropology generally, these issues will be examined given the results of our research activities since the inception of the project.

Anthropologists have been utilizing computers almost from the beginning of their availability to academic scientists. But only a small number of anthropologists did become involved with this tool in the early years. In 1962 a conference sponsored by the Wenner-Gren Foundation was held at Burg Wartenstein, Austria with the theme of "the use of computers in anthropology." The volume that resulted (edited by Dell Hymes) was partially an introduction to the computer for anthropologists and also an introduction to the possible uses that the computer might have in anthropology. It covered a variety of topics including linguistic data processing, statistical processing, content analysis techniques, simulation, numerical classification techniques, among others. At that time the computer age was only about fifteen years old. And one concern of conference participants was the availability and costs associated with computer usage. By the late 1970s such concerns may not have vanished totally, but they clearly had diminished, even before the advent of microcomputers at low prices. Lack of availability is not a valid reason for not using computers in the present age, although in specific cases costs may be prohibitive where individuals do not have free access to computers, but must pay.

An indication of just how available computers are can be gleaned from the pages of the GUIDE TO ANTHROPOLOGY DEPARTMENTS. I examined all entries in the 1978-1979 GUIDE and obtained the following results:

> 168 departments (53%) specifically mention the availability of computing facilities for their students,
>
> 148 departments (47%) fail to mention computers, but such failure does not necessary imply that

computers are not available to students in those programs.

Of the 76 universities reporting the granting of PhDs in 1977-1978, 48 (63%) specifically mentioned the availability of computer facilities, while 27 (37%) failed to mention such facilities. Of these latter, it is clear that they must have such facilities but simply believed that mention of computers was unnecessary (to be assumed); among those not mentioning computers, one had prestigious departments (e.g., Chicago, UCB, UCLA, Harvard) as well as less highly ranked programs. One cannot tell from these mentions of computer resources the ease of access for staff and students, although some institutions did specify that use of computer resources was free, and others indicated the presence of such resources within the department itself. Having remote terminals to mainframes is by no means uncommon within anthropology departments, and computer internships have been created in some departments to aid anthropologists is computerizing their research activities.

Availability and sophisticated use of computers are quite different things, of course. Another way of looking at how widespread computer use is, is to find out how many anthropologists are specialists in the use of this tool. Again, the GUIDE TO ANTHROPOLOGY DEPARTMENTS comes in handy. The GUIDE lists specialties for those faculty members listed. Admittedly these are not comprehensive indications of a scholar's interests or competencies, being rather brief. However, an indication of how extensive computer knowledge is in anthropology can be gleaned from these lists of specialties. In the 1978-1979 GUIDE, 4633 individuals are listed as belonging to departments or museums. A total of 36 individuals mention computer applications or artificial intelligence (which is generally closely linked to computer interests) as one of their specialties, in other words fewer than 1% of the profession. The affiliations and subfield specialties of these individuals are noteworthy: upon inspection some of them turn out to be sociologists or geographers working in anthropology departments or combined departments, and in one or two cases they are computer specialists, not anthropologists. Most of the others are archaeologists and physical anthropologists (whose work with computers is almost exclusively quantitative rather than qualitative). As nearly as I could determine only eight of the 36 are individuals whose primary affiliation is with sociocultural or linguistic anthropology: by name, they are Jon Olson, Eugene Hammel, John Wood, Benjamin Colby, Christine Fry, Oswald Werner, George Collier, and Henry Selby). That is, eight out of approximately 3,000 sociocultural anthropologists is sufficiently involved in computers to list this among their four or five major interests. This number may be increasing, and I suspect that an analysis of the 1983-1984 GUIDE might show somewhat higher figures and some additional names.

That there are few anthropologists specialized in computer developments within the field is clear. Furthermore, inquiries concerning requirements for the PhD in anthropology suggest that com-

petence in computer methods is never required by any department for the granting of the degree; indeed, computer training at least in the late 1970s was not required. At most, we discovered there was a requirement for either statistics or computer science. In some instances, however, the gaining of computer competence was facilitated by allowing a student to substitute computer skills (or quantitative skills and statistics) for a language. Thus, according to the GUIDE, Bryn Mawr, American, Illinois, Massachusetts, Northwestern, and Utah permitted graduate students to learn to use the computer rather than a foreign language. I did uncover one institution or program, the joint PhD program in medical anthropology at the University of California at San Francisco and Berkeley, that required a course in Computer Applications in Anthropology, which was described as "an introduction to data processing methods most commonly used by medical anthropologists...how a computer works, data form design, keypunching, use of SPSS and BMD program packages, and interpretation of computer output."

In other institutions there may be analogous required courses (cases that we missed in our survey of catalogs and the GUIDE), but it is safe to conclude that in general as of the late 1970s when most of the research on this project was conducted, learning how to use the computer was not a well-integrated subject in anthropology advanced degree programs. But graduate students were using the computer in their work, and I shall return to this later when discussing the results of our study of dissertation research.

But the use of computers in anthropology generally and the use of computers in field work by sociocultural anthropologists doing ethnographic research are two separate issues. One of the fundamental goals of the current project was to answer the following question:

> WHAT EXPERIMENTS HAVE BEEN UNDERTAKEN TO USE COMPUTERS IN ETHNOGRAPHIC FIELD WORK? HAVE SUCH EXPERIMENTS BEEN SUCCESSFUL? WHAT PROBLEMS HAVE BEEN ENCOUNTERED IN THIS TYPE OF WORK?

In short, our goal was to find out what ethnographers had done by the late 1970s to try to use computers in their field work.

COMPUTERS IN THE FIELD

To attempt to answer the question(s) noted above, I undertook an extensive search operation to try to identify and contact scholars who had used computers in field work. The procedures used were those specified in our research proposal, and they included the following activities.

A. Literature search. I made a careful survey of the anthro-

pological literature to find any publications that might mention the use of computers in field work. An important byproduct of this work was the creation of a "Bibliography of Anthropological Uses of Computers". That bibliography is attached to this report as Appendix I. This bibliography has been circulated to a number of anthropologists highly involved in computer applications, and several of them have gone over it and suggested additions at various points. The attached version was completed August 23, 1982. Some additional references for 1983 and 1984 are to be added to it before it is submitted for publication during the summer of 1984. Those who have received copies of the bibliography have found it useful according to comments received from them.

Did this literature search, then, produce any information of relevance to the question(s) that this part of the project was to answer. It did, but the evidence was negative, that is to say, an almost complete absence of any written mention of the use of computers in ethnographic field work. The Hymes volume, for instance, did not mention any pertinent work as of 1962, but perhaps that was understandable given the recent initiation of the computer age. A decade later Paul Kay took up this question directly in his introduction to Gilbert's papers in EXPLORATIONS IN MATHEMATICAL ANTHROPOLOGY. He noted that the author had touched on one area that he thought would become of increasing interest - the utilization of computers by anthropologists while still in the field. He added that is is commonplace for those practicing the "new ethnography" to engage in both analysis and data collection while in the field, both proceeding at the same time since the ethnographer doing this kind of work always tried to ask the next question on the basis of full analysis of the questions already asked. Gilbert in his article suggested that computer analysis should be no exception to this rule. But Kay pointed out that the logistics involved are difficult, especially when the anthropologist is working in remote areas of the world.

Of greatest significance is the fact that Kay stated that as far as he was aware in 1971 there had only been one ethnographic project that had attempted to use computer analysis to guide further data collection while still in the field and that was on a project run by Duane Metzger and Williams (the Chiapas drinking project). But even in that case there was no actual use of computers in the field. Rather, data were sent back to the university for analysis and the analyses returned to the field to aid the continuing work. Kay continued, however, noting that with the increasing use of time sharing and scope viewing in contrast to printouts having to be relied upon, there is every reason to believe that effective use of the computer during a field stay was then or very soon would be feasible in areas such as the American Southwest (a prediction on the regional probability of experimentation that proved to be accurate, by the way; see remarks on Oswald Werner's work below).

Yet, experimentation came slowly. And by the time of the inception of the present research there were no other published accounts of and by anthropologists concerning their use of computers while in the field. In August 1980, with the assistance of Donald McIntyre of Pomona College, I conducted a computerized literature search to double check our more

conventional search efforts in the library. Using DIALOG and searching in the Social Science Research Data Base, we located 35 items that were anthropological and that concerned computers in some way. Most of those items involved topics such as computer conferencing, mathematical analyses, sociometric studies, a variety of archaeological and physical anthropological applications, but none that involved anthropological uses of computers in the field. A search of ERIC yielded 40 items, but again, none of these were relevant to the topic of this report.

Michael Burton has on two occasions reviewed computer applications in cultural anthropology, in 1970 and again in 1973. In his 1970 review, Burton noted that much of what was contained in the previous summary (the already-mentioned volume edited by Hymes, THE USE OF COMPUTERS IN ANTHROPOLOGY) was obsolete. He further pointed out that while at the time of the preparation of the earlier volume very few anthropologists had actually experimented with computers, by 1970 this was no longer true, and that anthropologists had begun to employ computers at earlier stages in their research and without even mentioning that fact in print. In his 1970 review, Burton made some passing reference to work by Benjamin Colby on text analysis by computer, by Coult, Randolph, Kronenfeld and Hammel involving simulations and genealogical manipulations, but his review was focused largely on numerical processing. In particular he concentrated on scaling applications which would have been unthinkable, in fact, without the existence of computers to perform the vast numbers of calculations that are necessary for such work. Burton stated: "The training of anthropologists who can understand the relevance of such models to their work may be far in the future since the majority of them are still skeptical of most formal methods and of the computers which make them work." There is no mention in this article of the possible use of computers in field research.

In 1973, Burton's second review dealt with linear programming, linear regression, simulations, and content analysis of texts. At this time he states: "In the past few years recourse to the computer at some point has become common for the practicing cultural anthropologist. Although a large part of that usage takes the form of analyzing data with packaged statistical programs, more and more anthropologists write their own programs for specialized problems which are unique to cultural anthropology. This change in the role of computers in cultural anthropology is a consequence of two trends: first, an increase in the quantification of field data, and second, an increase in the construction of formal models, which often require the computer for their formulation or computation." This review, too, fails to mention any use of computers in the field, and the basic emphasis is on numerical processing, except for the discussion of work on text processing that Colby has carried out over the years.

B. Newsletter Notice. A second strategy for trying to locate individuals who might have tried using computers in the field was to place an announcement in the ANTHROPOLOGY NEWSLETTER, in accor-

dance with the procedures indicated in the research proposal. This publication has a wide circulation since it is sent to all members of the American Anthropological Association. The response was, to say the least, disheartening. I received a total of five letters in reply to this request for contact with scholars who knew of any such efforts, and several of those who wrote did so not because they knew of anything that had been done but only because they, too, were interested in the question of using the computer in connection with qualitative data analysis and field research. There were three respondents whose reports were of some value, however, indeed quite useful and they are discussed later (Oswald Werner, Christoph Wolfart and Willett Kempton). Since the response to the ANTHROPOLOGY NEWSLETTER announcement was so low, follow-up announcements were not placed in more peripheral newsletters as had been originally planned; it was highly unlikely that this strategy for locating field computer users would be productive if it had not worked well when tried with the major anthropology newsletter.

C. Personal Contacts with Anthropology Computer Specialists. I attended the annual meeting of the American Anthropological Association in Los Angeles in 1978 for the purposes of talking to as many people as possible, inquiring about the identities of anyone who had used computers in the field. Again, the results were largely negative. No one knew of anyone who had done this beyond those whose names have been mentioned already. I did have some extremely useful discussions at those meetings, however, with scholars interested in the topic, especially with Oswald Werner and Lee Sailer. Indeed, at this meeting, Werner and Sailer and I agreed to arrange a symposium for the next annual meeting of the AAA that would focus on computer uses in anthropology. This symposium, which was held in Cincinnati in 1978, brought together a small group of knowledgeable people. In my presentation, I again made a plea for anyone present to provide me with names of individuals who had used computers in the field. Aside from Werner, none of the participants had used computers in that way (indeed, most of the presentations were peripheral to the concerns of this project, e.g., simulations, computer conferencing). And, again, the effort to locate the ever-elusive anthropologists who had used computers in the field failed to yield relevant data.

Nonetheless, in other ways these intensive search efforts had valuable payoffs. In connection with the Cincinnati symposium, Werner, acting for Sailer and Bolton as well, applied for a grant from the Wenner-Gren Foundation to support a conference on "the use of computers in fieldwork in the field." A grant was approved, sufficient to pay part of the expenses of fifteen specialists who were to be brought together for a couple days of discussions. Thus, while our efforts did not produce information of much consequence on the topic, they did directly influence discussions of the topic, and were instrumental in bringing together a group of scholars most capable of making progress in the computerization of the discipline and getting them to coordinate their energies in this regard.

For a variety of reasons, the conference planned in 1979 could not be held until March 4-6, 1982. It was then held at the University of Pittsburgh and entitled "The Future of Computer-Assisted Anthropology Conference." The names of participants in that conference are to be found on the following page. Participants did not so much present formal papers as simply report on some of their uses of the computer for dealing with anthropological problems. Discussions then ensued over the directions that might be taken in promoting field use of computers. By this date, 1982, the microcomputer revolution was in full swing, of course, and that added to the enthusiasm and the optimism of the participants. Several of the participants had plans to use a microcomputer in the field in the near future themselves and were encouraging students to do so as well. The relative merits of different types of hardware were discussed, but given the rate of obsolesence the content of those discussions need not be summarized here: the Osborne I was a favorite of some then--it is no longer on the market, the IBM PC was just hitting the market then and was still somewhat unknown.

An outgrowth of the Pittsburgh conference was the establishment of the Committee on Computer-Assisted Anthropology. Initially, a newsletter was planned as a means by which scholars could continue to share information in this area, including the sharing of programs that they had written or at least the descriptions of programs that might be of interest to anthropologists. The idea of a newsletter has since been shelved in favor of an attempt to create in another anthropological publication, such as the ANTHROPOLOGY NEWSLETTER, a regular column or section that would deal with computer-related matters.

Another outcome of the Pittsburgh conference is a report which is due to be published shortly (1984) in PRACTICING ANTHROPOLOGY. It was decided that the best manner in which to present the information generated during the conference was to have the participants write concise vignettes of the computer applications they had discussed; these would then be compiled and edited and published as a document of the CCAA. Since I had developed a list of anthropologists interested in computers in connection with the present project, the conference participants asked me to solicit additional vignettes from people on that list. I sent a letter on behalf of the CCAA in June 1982 to 403 anthropologists asking them to share information on their use of the computer if such use involved something more than standard word processing packages or statistical packages. A copy of the letter follows on page 15 of this report. Approximately 200 recipients of this letter responded because of the indication in the letter that respondents could have their names placed on the CCAA mailing list. However, only about 30 individuals offered any details about their computer use, because, presumably, the others used only packages in common use. Of these 30, only a handful had any relevance to the use of computers in the field and/or the processing of qualitative data. Summaries and abstracts from those replies will be given below.

Other results of the symposium and conference should be mentioned in passing. At the 1983 and 1984 AAA annual meeting,

List of Names and Addresses of
The Future of Computer-Assisted Anthropology Conference
Department of Anthropology, University of Pittsburgh
Pittsburgh, PA 15260
March 4-6, 1982

Mike Agar
Department of Anthropology
University of Maryland
College Park, MD 20742
301/454-4154

Russ Bernard
Department of Anthropology
1350 GPA
University of Florida
Gainesville, FL 32611

Ralph Bolton
Department of Anthropology
Pomona College
Claremont, CA 91711
714/621-8000, x2228
1888 Abilene Way
Claremont, CA 91711
714/621-0895

James Boster
Department of Anthropology
University of Kentucky
Lexington, KY 40506
606/258-2840

Michael J. Evans
Department of Anthropology
University of Florida
Gainesville, FL 32611
904/392-2031

Richard Greene
Graduate School of Public and
International Affairs
University of Pittsburgh
Pittsburgh, PA 15260
Office: 412/624-3616
Home: 412/687-5305

Willett Kempton
Institute for Family & Child Study
College of Human Ecology
Michigan State University
East Lansing, MI 48824
517/353-3717

Margaret M. Kieffer
Cognitive Enterprises
6600 SW 139th Avenue
Miami, FL 33183
305/387-3534

David Kronenfeld
Department of Anthropology
University of California
Riverside, CA 92507
714/787-4340

Sara Beth Nerlove
Measurement Methods and Data
Resources Program
National Science Foundation
Washington, D.C. 20550
202/357-7969

Aaron Podolefsky
Department of Sociology & Anthropology
West Virginia University
Morgantown, WV 26506
304/293-5801

Lee Sailer
Department of Anthropology
University of Pittsburgh
Pittsburgh, PA 15260
412/624-3388

J. Jerome Smith
Department of Anthropology
University of South Florida
Tampa, FL 33620
813/974-2138

Oswald Werner
Department of Anthropology
Northwestern University
Evanston, IL 60201
Office: 312/492-7463
Home: 312/328-4012

COMMITTEE ON COMPUTER-ASSISTED ANTHROPOLOGY

June 25, 1982

Dear Colleague:

In March 1982, those whose names appear on the left met for a conference at the University of Pittsburgh to discuss computer applications in anthropology. As a result of that conference, supported by the Wenner-Gren Foundation, the Committee on Computer-Assisted Anthropology was formed. Its purposes are to continue the exploration of the uses of computers in our discipline and to disseminate information on this topic to members of the profession. Given recent developments in the field of microcomputers, it is probable that an increasing number of anthropologists will be turning to computers for assistance in analyzing data. The Committee is preparing a document describing current uses of computers by anthropologists. It also expects to initiate a newsletter to serve as the medium of continuing communication among those working with computers. Lee Sailer will coordinate the activities of the Committee.

In a survey of anthropology department chairpersons in 1980, conducted by Ralph Bolton under a grant from the National Institute of Education, your name was provided by a respondent as someone with an interest in computer applications and/or as someone who has done extensive work with computers. If you wish to have your name placed on the CCAA mailing list to receive the newsletter, please return the enclosed sheet giving your name and address. Extra copies of this announcement are enclosed, and we would appreciate if you would pass them along to colleagues or graduate students who might also be interested.

If you have used a computer for more than standard word processing or statistical manipulations involving canned programs such as SPSS or SAS, we would like to hear from you. Specifically, we would be most grateful if you would send us a brief description (perhaps several paragraphs) of each type of computer use, including the purpose of the research, the kinds of data utilized, the methods involved in the analysis, an assessment of the experience, and a list of publications related to the project. These vignettes should be sent to: CCAA, c/o Ralph Bolton, Department of Anthropology, Pomona College, Claremont, California 91711. If you have any questions, please contact any member of the Committee.

at the meeting of the Society for Applied Anthropology in Lexington in 1982 and at the 1983 meeting of the ICAES in Vancouver, workshops and displays of microcomputer equipment and applications have been held in order to disseminate information. Jerome Smith and Lee Sailer have been particularly active in conducting these activities which were suggested at the CCAA conference in Pittsburgh. Those sessions were well received.

Independent developments should also be noted here. Summer workshops of five days' duration have sprung up to train anthropologists in computer uses. In 1982, for example, one such workshop, named "Computers & Statistical Methods in Anthropology," was held at Texas A & M University. The program of that workshop was devoted, as might be expected, to quantitative methods, and did not include training in the handling of qualitative field data.

Another symposium on computers was held at the 1983 annual meeting of the American Anthropological Association. This one was organized by James Dow and Rodney Kirk and was entitled "Computer Software Applications in Anthropology."

Appendix II contains vignettes from the document being edited by Lee Sailer on behalf of the CCAA. A few of these describe ways of handling textual materials; others discuss other innovative uses of computers in anthropological research. Included is Oswald Werner's description of the projects in which he has been involved using computers in field work in the Southwest.

D. Questionnaire to Anthropology Department Heads. The research proposal included as one of the procedures for ferreting out individuals who might have used computers in the field a questionnaire that was to be sent to all the heads of anthropology departments listed in the GUIDE TO DEPARTMENTS. A letter and a questionnaire (see pp. 17-19 of this report) was sent in July 1980 to one person in each department listed in the 1979-1980 GUIDE. It asked them to provide the names of department members who might have used computers to analyze qualitative data, or who employed computers while in the field. We received questionnaire back from 175 of the 320 academic departments to which we had sent the questionnaire. From these returns I compiled the list of 403 anthropologists knowledgeable about computer uses which was used in the CCAA mailing described above.

In the next section I shall provide excerpts from the letters received in response to our solicitation. In some of them there are brief descriptions of the use of the computer with texts and in others of computers used in the field, but not both together. If used in the field, the usage involved statistical analysis, not the recording of qualitative field notes.

I believe that it is fair to state that as part of this project we began a dredging operation in 1978 and we continued that for some five years, utilizing in that endeavor various methods from questionnaires to personal contacts to literature searches. That we managed to

POMONA COLLEGE
CLAREMONT, CALIFORNIA 91711
(714) 621-8000

DEPARTMENT OF SOCIOLOGY AND ANTHROPOLOGY

July 15, 1980

Dear Colleague:

As part of an investigation of the uses of computers in anthropology, I am attempting to locate anthropologists whose work has involved certain kinds of computer usage. In particular, I wish to contact anthropologists who have utilized computers or computer-related equipment while engaged in field work (e.g., to record data or to analyze data). And, also, I am interested in contacting anthropologists who have used computers in handling qualitative kinds of data (e.g., texts, field notes), whether in the field or back at their home bases.

I would be most grateful to you if you would take a few minutes to provide the names and addresses of individuals in your department (faculty members and past or present students) who have used computers in any of these ways.

This letter is being sent to one person in each anthropology department listed in the 1979-1980 GUIDE TO DEPARTMENTS OF ANTHROPOLOGY, in most cases to the chairperson. If there is someone in your department with a more substantial interest or knowledge about this topic than your own, perhaps you would prefer to pass this request on to that person. Even if you know of no one who has done computer work of the sort described above, I would appreciate having the form returned. A stamped, addressed envelope is provided for that purpose.

Thank you for your assistance.

Sincerely,

Ralph Bolton
Associate Professor
of Anthropology

COMPUTERS IN ANTHROPOLOGICAL FIELD RESEARCH

Name of respondent: _____ Date: _____

Respondent's affiliation: _____

I. Names and addresses of anthropologists who have used computers or computer-related equipment in the field situation or who have prepared their data while still in the field for computerized analysis:

1. _____
_____
_____
_____
City State Zip

2. _____
_____
_____
_____
City State Zip

3. _____
_____
_____
_____
City State Zip

4. _____
_____
_____
_____
City State Zip

5. _____
_____
_____
_____
City State Zip

6. _____
_____
_____
_____
City State Zip

II. Name of the person in your department who is most involved and most knowledgeable about the uses of computers in anthropology:

_____
_____
_____

III. Names and addresses of anthropologists who have used computers to aid in the storage, retrieval, and/or analysis of qualitative data (texts, field notes, and so forth):

1. _____
_____
_____
_____
City State Zip

2. _____
_____
_____
_____
City State Zip

3. _____
_____
_____
_____
City State Zip

4. _____
_____
_____
_____
City State Zip

5. _____
_____
_____
_____
City State Zip,

_____
_____
_____
_____
City State Zip

Thank you!

Please return in the stamped and addressed envelope provided.

naires to personal contacts to literature searches. All of these took considerable time. That we managed to turn up so little was not due to a lack of investment of the necessary energy and effort, but rather the negative results reflect the reality of the situation. Put bluntly, at the time of our investigation no one had done what we were looking for; persisting in searching was frustrating and problematic in that it caused us to fail to conclude the project--ever seeking the elusive anthropologist who had used a computer in the field to record and analyze qualitative data.

Yet, we can consider the search a success because it stimulated discussions of the topic among anthropologists. It may have helped the process of getting computers into the field along somewhat, although most of the credit for that will have to be given to technological developments, the microcomputer revolution. While it was not possible to write an elaborate report on anthropological uses of computers in the field in 1979, nor even in 1984, it will be possible to do so within the next five years since by then we will have the experiences of quite a few anthropologists who are now planning to experiment with computers in the field, using them to record all of their field data, both quantitative and qualitative.

EXCERPTS FROM RESPONDENTS' REPORTS ON COMPUTER USES

1) Robert K. McKnight. Some years ago, under my supervision, an advanced undergraduate major in anthropology typed into a computer most of the origin/innovation myths occurring in Radcliffe-Brown's ANDAMAN ISLANDS. Using a program named "Wilber," the student sought to retrieve recurrent themes and words in the myths. Actually, before the computer confirmed it, the student successfully identified the word (or idea) 'anger' as having a high frequency and we were then able to use Wilber to provide us with context printouts which were useful. The results contributed to an explanation of Andeaman initiation rites that is at variance with that provided by Radcliffe-Brown, as well as to a more general theoretical formulation having to do with innovation in that and other societies. The results are (briefly) summarized in my article "Past and Future Culture Change: A Quest for Variant Explanations" in M. Maruyama and A. M. Harkins (eds.) CULTURES OF THE FUTURE (1978) Mouton Publishers, The Hague."

2) Charles Super. When Sara (Harkness) and I went to the field, to Western Kenya, our goal was to stay there for a substantial length of time and collect interrelated data on a number of topics. We were fortunate in our second year to secure funding for a small desktop programmable computer (Hewlett-Packard 9830). In order to convince the funding agency to allow us this expenditure, I wrote a long letter outlining my experiences with the University of Nairobi computer (in trying to get some of John Whiting's data through it I discovered it would not run a frequency program provided by the manufacturer), and the alternative of sending the information back to Harvard for punching and analysis and sending results by return mail. Since Sara and I lived a long day's drive from Nairobi, the net result was that data analysis

at Harvard, with adequate support by a research assistant there, was more efficient than struggling at the University of Nairobi. Even that procedure would have expected turnaround time of five or six months. Since our main purpose was analysis of preliminary questionnaires, assessment of reliability, and other preparatory topics, that seemed less than satisfactory.

There were a number of interesting adventures, as you might imagine, importing expensive electronic equipment into Kenya, but we eventually succeeded, and even avoided the possible 100% import duty. The computer was powered by a gasoline powered generator attached to a voltage regulator. This had the interesting consequence of limiting analysis time to the duration of gasoline in the generator's tank: about two hours. At one point when Sara was in the middle of an analysis she needed for her thesis, the generator broke down. We took a trip to the Kericho Club, a legacy from the British in Kerico, the main town in the tea-growing area of the Western Highlands where we went regularly for supplies. We set up operations in the Club library and Sara got her work done on time.

In addition to preliminary analyses we used the machine for keeping census data which greatly facilitated selection of subjects of certain ages, etc. for testing.

Despite all the hassles, having the computer was well worth the effort for us and made a real difference in the quantity and quality of our field work. The technology has changed a great deal since then, however, and there might be better ways of facilitating this kind of work these days. Even some of the small hand-held machines have (limited) data storage possibilities, which was an important feature for us.

3) James Dow. (Sent as an abstract of a paper). The combined use of computers and audio tape recorders in storing, retrieving, and manipulating qualitative ethnographic data in one field research project are described. The field work was an updating of the ethnography of a Mexican municipio and a detailed ethnographic study of one shaman informant. General verbal data and specific life history data in verbal form was gathered.

All of the data was originally recorded in audio form on a stereo cassette tape recorder. The second track of the recorder was used when necessary to add comments by the other informants. The general ethnographic data was not transcribed from the cassettes to written form, instead a detailed written index of the tapes was prepared and entered as records into a data-base management system programmed for a Honeywell 6800 multics computer. Programs were written in the TED language to trim the words in the index to significant ones and then to sort the index by these words so that all passages referring to any topic or any combination of topics can be conveniently located. A document containing the sorted index was prepared for using the tapes while writing ethnographic descriptions.

The life history data from the shaman was translated from the tapes

and entered directly into a computer file. Each record in this transcript file consists of a translated paragraph from the interviews, the location of the original words on the tape, a paragraph number, and several manuscript numbers indicating where the paragraph is to appear in future manuscripts. The translations can be sorted so that they form subfiles that can be called by word-processing programs. A master word-processing program contains an ethnographic description and calls the subfiles as they are needed to make the text. Thus manuscripts containing the shaman's words can be produced at any stage in the project.

These procedures have a number of advantages. The large volume of recorded general ethnographic data does not have to be transcribed. The interviewing is not slowed down by note-taking, and the full record of the interviews is retained on tape. The content of the interviews can be quickly assessed by the many themes that they contain. Translations or transcripts do not have to be retyped or re-edited each time they are used. A preliminary manuscript can be made available for submission to granting agencies, and transcripts can be incorporated into any number of documents without affecting the way that they are used by other documents. In general a wide range of future uses of the field data is opened by having it available in electronic form.

A field computer was added to the project in 1982. It was a portable Osborne-1. This computer is a 64 K Z-80 type computer with two mini-floppy disk drives operating under CP/M. The major program packages were Word-Star, CBasic, MBasic, and SuperCalc. It functioned well under field conditions in a rural Mexican town. It enabled the tape index to be prepared and accessed in the field. It also allowed the translations to be completed in the field. These were later transferred to the larger Honeywell computer when the period of field work was over.

After the field computer was put to use a number of unexpected advantages to anthropological research emerged. Probably the most significant unexpected one was that the word processing capability of the computer allowed work on the final book manuscript about the shaman while the field work was underway. Instead of writing notes the researcher could work on writing and modifying a book chapter on the subject. Another advantage was that quantitative and other sorts of coded survey data could be recorded in an electronic form. The data could be processed in the field immediately within the capacity of the small field computer, or could be read out later into the larger university computer. A program was written for the convenient storage of data gathered by a local government project in random access disk files.

Cooperation with rural government agencies was good, but it would have been improved by having a small portable field printer to go with the computer, which had only a CRT output. A printer allows the circulation of output from the field computer to others who are cooperating with the investigation. The need for a field printer was not anticipated at the beginning of the project. The major problem with the field computer was in obtaining special programs for processing, storing,

and communicating data. It turned out to be easier to write most of these programs than to purchase them. It is suggested that people using field computers get ones that have good programming languages and large memory capacities so that they can write the specialized programs needed for anthropological applications.

4) Michael Livesay. My position at the Institute (for Research in Social Science, University of North Carolina at Chapel Hill) is one of liason to anthropologists in the area and elsewhere. The IRSS is trying to organize its services (many of which involve computer applications) to be more useful to our discipline. As part of that effort, we're looking at the ways anthropologists actually use and could use computers in their work. We have been examining various uses and possibilities, including the utilization of microcomputers for word processing and data entry in the field or as terminals interfaced with larger capacity machines.

We are beginning to work with a Verbal Data Retrieval System which is being developed by Dorothy Holland. That system is still in the process of development, but is sufficiently far along that it already has been used to a limited extent in project analysis and data cataloging. The VDRS is based on the Bibliographic Processing System, which was developed from a Library of Congress application for use at the Carolina Population Center. It has some limitations that may not be encountered in systems based on other programs (such as SPIRES), but it is compatible with the computation center here and appears quite useable.

The VDR System is as yet poorly documented and ultimately "belongs" to the BPS developers at the Population Center, but in the near future I may be able to pass along some further information (dated 1/24/83).

All other projects about which information was received involved uses by non-sociocultural anthropologists (especially archaeologists) or if by sociocultural anthropologists involved the analysis of numerical data rather than qualitative data or field notes per se.

To conclude this section, I would like to point out some references to materials which should be consulted by anyone interested in using computers in the field. These items are quite recently published; nothing in this genre was available at the time this research was begun for this project, nor even at the time when the original date for the culmination of the project arrived. But these items can be recommended.

Podolefsky, Aaron, and Christopher McCarty

1983 "Topical Sorting: A Technique for Computer Assisted Qualitative Data Analysis," AMERICAN ANTHROPOLOGIST 85:886-890.

Agar, Michael

n.d. "Microcomputers as Field Tools," COMPUTERS IN THE HUMANITIES (in press).

Werner, Oswald

1982 "Microcomputers in Cultural Anthropology," BYTE 7:250-280.

Sproull, L. S., and R. F. Sproull

1982 "Managing and Analyzing Behavioral Records: Explorations in Nonnumeric Data Analysis," HUMAN ORGANIZATION 41:283-290.

Kirk, R. C.

1981 "Microcomputers in Anthropological Research," SOCIOLOGICAL METHODS AND RESEARCH 9:473-492.

Bernard, H. Russell, and Michael J. Evans

1983 "New Microcomputer Techniques for Anthropologists," HUMAN ORGANIZATION 42:182-185.

These items as well as the papers from the recent symposia and the Pittsburgh conference complement the materials discussed above.

NON-COMPUTER METHODS IN RECORDING AND PROCESSING QUALITATIVE FIELD DATA

Although the emphasis in this project was computer applications in the field, an additional focus involved the examination of the methods currently employed by anthropologists in the field to record and process their field notes. This goal was accomplished using two approaches:

1) a review of the anthropological literature for as many references as could be located to discussions of how anthropologists have been handling field notes;

2) a survey of recent PhDs in social and cultural anthropology (1977-78), as determined by names listed in the GUIDE TO ANTHROPOLOGY DEPARTMENTS.

UNIVERSITY OF TRONDHEIM
Department of Social Anthropology

N-7055 Dragvoll,
Norway.
Phone (075) 96 500

May 25, 1979

Dear Colleague:

Although field notes are basic to anthropological research, little explicit information has been published on the techniques commonly (or even advisedly) used to record observations. This lack is particularly striking when contrasted to the great concern among anthropologists at present over the quality of research results and over professional goals, since it is clear that the recording methods used by fieldworkers have an important effect on the quality of the final product.

In an effort to clarify current field recording practices and to provide a basis for assessing the potential applicability of modern information storage and retrieval technology to field work, we are sending the enclosed questionnaire to a sample of recent recipients of the Ph.D. in anthropology. We will greatly appreciate your cooperation in answering the questionnaire fully and candidly and returning it to us as soon as possible. An addressed envelope is included for your convenience.

We are planning to present the results of this survey at the AAA meetings this November in Cincinnati, Ohio. In doing so, the anonymity of all respondents will be strictly protected. In particular, in the analysis and writing up of the survey material, care will be taken to ensure that there will be no way to identify an individual through any mention of his or her research topic, methods, or geographical location.

In addition to returning the questionnaire, we would very much appreciate your sending us if possible a copy of your dissertation abstract and a copy of the section of your dissertation that deals with field methods (if there was one). We shall be happy to repay you for the copying costs and mailing of these items if you so request.

Correspondence concerning this project should be sent to us at the address given below, rather than to our temporary Norwegian address. Thank you in advance for your time and assistance.

Sincerely,

Anne Chambers
Research Associate

Ralph Bolton
Associate Professor

Ralph Bolton and Anne Chambers
Field Note Survey
Department of Anthropology
Pomona College
Claremont, California 91711

POMONA COLLEGE
CLAREMONT, CALIFORNIA 91711
(714) 626-8511

DEPARTMENT OF SOCIOLOGY AND ANTHROPOLOGY

February 10, 1980

Dear Colleague:

Several months ago Anne Chambers (Univ of Auckland) and I mailed a questionnaire on field methods to a sample of anthropologists who had received the Ph.D. in 1976 or 1977. The completed questionnaires that we received back were full of fascinating and useful information on the field experience. We believe that the findings from this survey will be of interest to many fellow anthropologists, and that they will be particularly helpful to younger scholars preparing to go to the field for the first time.

We are writing to you as one of those to whom a questionnaire was mailed. If you returned the questionnaire, we wish to express our appreciation for your assistance, for your willingness to serve as an "informant-at-a-distance" on this research. If you did not return the questionnaire, may we urge you to do so? We need your help to make the report as comprehensive as possible, one that accurately reflects how anthropologists do field work.

If you have misplaced the questionnaire, we would be happy to send you another copy. Or, if you did not receive the questionnaire, we will gladly send you one now. Since the questionnaire was sent out originally from Europe, we suspect that some got lost en route.

We would appreciate if you would take a moment to read and check off the appropriate items on the enclosed sheet, returning it to us. An envelope is provided for your convenience. Thank you.

Enclosures

Sincerely,

Ralph Bolton

Ralph Bolton
Associate Professor,
Anthropology

FIELD NOTES PROJECT

Name _____

Address _____

_____

_____

City State Zip

Please read the following statements and place an "x" on the line in front of each statement that applies to your situation. Thank you.

_____ 1. I received the questionnaire and returned it.

_____ 2. I received the questionnaire but have not returned it because

_____

_____

_____

_____ 3. Although I have not yet returned the questionnaire, I will do so by the following date: _____

_____ 4. I did not receive the questionnaire on field methods.

_____ 5. If sent a copy of the questionnaire I will fill it out and return it.

Date: _____

Work on this portion of the project was done in collaboration with Anne Chambers, then of the University of Trondheim and now of the University of Auckland. We designed a questionnaire concerning field conditions and data handling techniques (see Appendix III). This was sent out in May 1979. We encountered severe difficulties in locating current addresses for these recent PhDs since many of them had not gotten positions in academia and were not listed in the GUIDE with addresses. It took many months of correspondence with department secretaries to elicit correct addresses. During that time we also conducted the literature search which produced many very brief discussions of the topic, but little of great substance.

Additional complications in carrying out this part of the research arose because of having to send the questionnaires out from a base in Norway (as a result of the grant having been approved after the period of time when this work was to have been done prior to the principal investigator's departure for a sabbatical in Norway. Returns came back slowly. Moreover, the response rate was low. Consequently, it was necessary to send a reminder to these potential informants. That was done in February 1980, and it resulted in boosting the number of usable questionnaires to a number that justified analysis, although many of these additional responses did not come in until late in 1980. The total N equals 61.

These data do allow us to summarize information on techniques currently being used by ethnographers to record and process their field data. And, in fact, it is unnecessary to summarize those results in this context since a separate report has been written on that subject by Chambers and Bolton, and that report is attached as Appendix IV. That paper, which is a draft manuscript for an article, does pull together the basic information about how anthropologists handle field notes. Drafts of the paper circulated to the 1979 symposium participants and others who have expressed interest have been well received, and in paper form the manuscript has been cited repeatedly (e.g. Podolefsky and McCarty). A revision of this manuscript will be completed during the summer of 1984 (when the co-authors will be together in California during Chambers' sabbatical) and submitted for publication by September 1, 1984. We expect to expand the manuscript to include more information from the PhD survey.

The proposal for this project indicated that the Final Report would contain tables summarizing data obtained on the structural portion of the PhD questionnaire on field methods. Responses to questions have been tabulated and are presented in full in Appendix V (coded responses) and Appendix VI (open-ended responses).

In closing this Final Report, I merely wish to highlight a few of the findings from the PhD survey that are most directly relevant to the future prospects for the use of computers in the field to record field notes.

FIELD CONDITIONS, PROFESSIONAL ATTITUDES AND THE PROSPECTS FOR FUTURE USES OF COMPUTERS IN THE FIELD

The majority of anthropologists going into the field in the mid-1970s went to places where electricity was available to them (over 55%), even in their own residences. Thus, the image of the anthropologist isolated in some remote locale without access to modern conveniences needs to be revised. Most microcomputers are powered by electricity, of course, and for most to use computers in the field electricity will be necessary to have available. In the future, and to some extent at present, battery-powered microcomputers will exist that can be used even where there is no access or limited access to electrical power. But lack of electricity poses no stumbling block for a majority even now.

Anthropologists are accustomed to taking some equipment into the field. Over 90% of our respondents indicated that they took a typewriter with them or had one available for use in the field situation. There is not much difference between typing field notes with a typewriter and typing them into a computer. In addition to typewriters, tape recorders are taken into the field by most anthropologists (over 82%). With each passing month, the size of microcomputers gets smaller. Briefcase-sized micros have been available for at least two years, but their cost tended to be high. Recently, however, similar products have entered the market at a much-reduced price. Therefore, while the bulkiness of some microcomputers might have served as a deterrent in the past, it should no longer be a serious factor. Even quite serviceable printers are available in small size already.

Qualitative data are obtained during fieldwork by almost all field workers. Over 96% of the respondents indicated that they employed participant observation techniques in their work, and 70% noted that they take life histories as part of their research; both of these techniques tend to yield qualitative data.

Approximately one-third of the respondents indicated that they had employed computers at some stage in their dissertation research. Most often such use involved statistical analyses using package programs, especially SPSS. While 12 did not respond when asked if they felt they could have made better use of a computer in their research, 14 did respond that they felt they could not have made better use without adding why, 11 did respond negatively giving reasons such as the adequacy of use that was made of the computer, and 20 (almost 30% of the total) indicated that they could indeed have made better use of the computer in their work. Thus, resistance to computer use in the field may exist in some quarters, but it is clear that a significant portion of these young professionals is ready to intensify their computer use.

Among the benefits to be derived from computerization of field materials is the broader sharing of basic ethnographic data among

scholars. There exists some sharing already, of course, and approximately one-fourth of our respondents indicated that they had given another scholar some access to their field notes or unpublished data. In some instances, moreover, where the respondent had not shared field data with others, it was because "no one asked." When asked whether they would be willing to support a data bank concept for ethnographic field notes, the answers ranged from highly positive to extremely negative. But I believe that it is quite encouraging that half of the respondents did agree in principle to such a development, one-fourth were unsure, and the rest were against the idea (for a variety of reasons ranging from a phenomenological theoretical stance to concerns over rights to privacy).

Thus, when the microcomputer revolution hits the profession, as it surely will during the 1980s, there can be little doubt that it will lead to significant changes in the ways in which ethnographers do their work. Moreover it should result in all of the benefits that were pointed out in the project proposal:

a) a reduction in the time and costs involved in collecting and analyzing field data;

b) an increase in the information-sharing with colleagues doing similar work or involved in comparative analyses;

c) better control over the quality and the depth of detail of data used in writing an ethnography;

d) the creation of alternative means of presenting results of research;

e) making the data available to a broader audience, especially to groups having a policy interest in the data.

That the kinds of developments we investigated had not taken place prior to 1980 made this project difficult to bring to a "successful" conclusion; that those developments are now taking place give us something to look forward to.

APPENDIX I

BIBLIOGRAPHY OF ANTHROPOLOGICAL USES OF COMPUTERS

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APPENDIX II

VIGNETTES

MICROCOMPUTERS IN ETHNOSCIENCE ETHNOGRAPHIES

by Oswald Werner

For lexicographic work, or in ethnoscience ethnographies, it is useful to know the location of the words in a text. Most editing programs allow the user to find key words, but there is usually no provision to assess a text for the occurrence for all key words that are of interest to the ethnographer. Indexing techniques that allow for locating all words in a text alphabetically are useful for managing field notes as well.

In our work on the Navajo Ethno-Medical Encyclopedia Project (NEME) we are dealing with Navajo texts. These were collected from knowledgeable Navajos by Martha A. Austinn-Garrison, who was the Director of the Project in Kayenta, Arizona. On the way home from an interview Martha listened to the new tape on her car tape deck. She would then classify each tape from excellent (1.) to poor (5.). Babette H. Daniels transcribed the best tapes at the home office in Kayenta. Occasionally, with time on her hands, Babette transcribed a tape rated 2 or lower, but that did not happen often.

Unfortunately, in early 1979 there were few inexpensive microcomputers that could support multiple terminals. Babette's transcriptions on a typewriter were typed into the computer by Martha, who was fast (much faster than transcription from tape) and then

used the computer for analysis. One work station used for data entry and the other for analysis would have been ideal.

There are a number of ways for locating key words for analysis. At first we used a yellow felt marking pen and highlighted words of interest. Any list of these words had to be compiled by hand. In addition, it was very easy to overlook an occurrence.

Another method we have used successfully was computer-made concordances, or Keyword-in-Context (KWIC) indexes. In these every word appears in a context--of, for example, 10 words preceding it and 10 words following--the occurrences in context are then alphabetized.

Several years ago we transformed interview tanscripts into punched paper tape. The tapes were mailed to Northwestern University where the concordances were made by mainframe computer. These were then mailed back to the field. The procedure was useful but cumbersome, heavily dependent on the mails.

Keywork in Context Indexes are not practical on a microcomputer. They take too long to print. For example, in a text of 5,000 words and a context of 20 words the final printing comes to 20 times 5,000 or 100,000 words. On the slow printers which are available with most microcomputers it takes many hours to print out a document of that length. We needed a better solution.

My answer to the problem was the WORDINDEX program. For a text of 5,000 it only slightly more than doubles the printing task. The

idea is very simple. Each word is assigned an integer from 1 to N, where N is the number of words in the entire text. The first printing is the text with index numbers. Then the computer alphabetizes the list of all words of the text. This is printed as the alphabetized WORDINDEX.

With this WORDINDEX we can now assess the distribution and the frequencies of all words. This makes the selection of key words for further analysis considerably easier. We know what words are in a text and how many of them there are. We can now make intelligent judgments about the contribution of particular key words to the Ethno-Medical Encyclopedia.

The basic theoretical point of our procedures is that the use of every key word in a text contributes to its meaning. Therefore each use has to be identified and all uses of the key word have to be collected in a central file, the location of the encyclopaedic entry for that key word.

Today we do this semi-manually but we hope to further automate the process in the future. Almost every editing program has a search function. It finds particular words, phrases, and key words. We decide which key word to investigate on the basis of (1) the WORDINDEX and of (2) the problem (for example, anything having to do with conception, pregnancy and birth) we are studying. The WORDINDEX program allows us to identify all words relevant to the problem. It also helps to find key words that are mispelled and more importantly, words that may be grammatical variants of the original key word.

With the search command Martha locates the first key word. She inserts several blank lines on the screen and then procedes with the analysis. This consists of replacing all pronouns with the nouns or

noun phrases they stand for. In Navajo there are also proverbs that must be replaced by the verb or verb phrase that they stand for. For example, the sentence "He puts up a post for her inside of it," is completed to "The pregnant woman's husband puts up a post for the pregnant woman inside of the hogan." This sentence is then dispersed to at least three encyclopaedic entries: (1) to the husband (of the pregnant woman) file, (2) to the pregnant woman file and to (3) the hogan file. In a more fine grained analysis an entry for post (birthing post) may also be called for.

A quick check of the WORDINDEX outputs of a number of texts tells the analyst which diskettes with which files contain material for the husband, pregnant woman, hogan, and possibly the post encyclopaedic entries. Each diskette and each file are subjected to the same analysis. The encyclopaedic entry files grow proportionately. With editing the entries are ready for incorporation into the encyclopaedia.

If I had to do it again I would do many things differently. Although I have not seen one in operation, I understand that there are automatic indexing programs with some text editing programs (e.g., WORDSTAR). I prefer a commerical product over home brew, primarily because it took me a couple of months, part time, to write the first WORDINDEX program. The only way I can justify the time spent programming is that in the process I learned to do it well. Commerical programs are often much faster than programs written by novices. Some of the programs that are available to correct spelling also give word frequencies. These can do in minutes what my program does in a couple of hours. Of course, processing time is not a problem. After the initial purchase of the microcomputer there are only repair costs.

There is no equivalent to connect time, cpu time, or storage costs that that I was used to in using computing centers. The faster programs simply free the computer for other tasks. That is why we run our relatively slow WORDINDEX program at night.

Extraordinary time investment for programming was not our only problem. On the first day of operating our computer in Kayenta, Arizona, the screen faded out completely. We discovered voltage fluctuations of as much as 30 percent. A relatively inexpensive voltage regulator solved that problem.

During the summer months lightning was another problem. Even the slightest interruption of power wipes out the computer's memory. An Uninterrupted Power Source, however, was beyond our budget. As a result work in the afternoons during July and August, when lightning activity is at its maximum, was impossible most of the time.

Repairs had a disrupting effect too. We did try repairs by telephone (repairman telling the computer operator step by step what to do) but that was a disaster. We burned out the computer's power supply. The local repairman in Albuquerque (400 miles away) could not handle it. It took several weeks in Texas to fix the damage.

Later in spite of assertions that the Central Processing chip, the Z80, "never fails" we experienced a very troublesome transitory condition that would sometimes wipe out files or scramble them beyond repair. It took six months and several round trips by air express to Chicago to diagnose and repair the problem.

The future of the text or field note maintenance and analysis by microcomputer is bright. Hardware prices are coming down and the available selection of software is growing. I am now experimenting with a multi-window editing program that allows viewing the WORDINDEX

and the original text simultaneously. It also makes moving information (partial texts) from one file to another file very easy. I hope to have the file for the encyclopaedic entries and the file for lexical/semantic fields (e.g., folk taxonomies, part/whole diagrams, plans, decisions, etc.) all available in separate windows, with the possibility of updating each file at the push of a few buttons.

This process will cut down on analysis time because it cuts out the time it takes to work on each file in linear sequence. Each file is available for updating virtually simultaneously. Only at the end of the session is each document in each window stored sequentially on diskette.

COMPUTER-ASSISTED TOPICAL SORTING

by Aaron Podolefsky

My purpose in the next few paragraphs is to describe a method by which an anthropologist with virtually no computer experience can use a computer to assist in the analysis of field notes. The method, which I call computer-assisted topical sorting (CATS), was developed for, and has been used on, interview data recorded without anticipation of computer assistance. Thus, the method requires no alteration in field research or analysis strategies. Essentially, it merely does more efficiently, systematically, and completely things that anthropologists already do.

Our present ways for handling field notes become awkward as datasets become larger, making anlysis difficult or impossible. This became quite clear to me after several years of working with 10,000 pages of typed field notes produced by 29 researchers in ten sites. CATS provides means of allowing the richness and unique perspective of qualitative data to enter into the world of large-scale research.

One way of approaching a set of field notes is by asking "What information is in the dataset on topic X, Y, or Z?" In other words, some notes are relevant to one topic while adjacent notes are relevant to another. This means that the notes, which were recorded in chronological order, must be categorized from a topical perspective. It would be nice if a computer, as an information storage and retrieval device, could just "spit out" relevant data on command. Well, that's what CATS does. First, CATS requires a computer. My experience

is with a large university computer, though a small computer in the $3000-$5000 price range would serve just as well. Second, CATS requires some preparation, though not much, often less than would ordinarily be required in recording and indexing qualitative data. Once ready for analysis, data management involves literally only the push of a few buttons.

I applied the concept of computer-assisted topical sorting using a text--editor (called WYLBUR) on a mainframe computer. The key is the ability to search for strings of characters and to print lines which contain those strings. These strings may be words or numerals. Some, but not all, text-editors have this ability.

In entering the data, the standard 120 column computer line is split into two portions; one for the data and the other for numeric codes representing topics of interest. I type the text into the first 80 columns of the row and reserve the last 40 columns for codes. Once the data have been entered, a "hard" copy with each line sequentially numbered can be printed.

As usual, each topic of interest is identified and given a code number. This might be done after the data are on computer or it might be done before the fieldwork begins and revised later (each strategy has its uses). Categories specific to the research can be generated, HRAF codes can be used, and so on. Coding categories might include systems of relationships (mother's brother/sister's son), events of particular types, the type of data (observation or interview), the credibility of the community, political system, exogamy, etc. I use a two-digit numeral to identify each category.

The coding process involves reading through the notes and marking codes next to each paragraph that contains relevant informations. These

codes are then entered into columns 81-120 using a modification procedure which allows any number of lines to be chnaged at one time. It is worth noting that entering the data, coding (once categories are defined), and entering the codes can all be done by research assistants.

Data management is now relatively simple. For example, I can obtain a copy of all lines which contain code 04. The output would include the line number text, and code numbers from lines 5-9 as well as any other lines (therefore paragraphs or question-answer sequences) in the entire data set coded 04. Also, once "computerized," key word searches can be used to locate unindexed information.

Analysis of qualitative data involves an ongoing process of concept formation, development of categories, analysis, reformation of concepts and categories and so on. Using CATS, new or refined coding categories can be added at any stage of the research process. In short, I can now deal efficiently with 10,000 pages of interview notes in a way that would be impossible without the aid of the computer.

AUTOMATED DATA COLLECTION

by H. R. Bernard, P. D. Killworth, and Lee Sailer

A problem that all field workers have is recording all the behavior that is going on around them, let alone the parts that they are interested in. For purposes of a study reported elsewhere we needed a complete record of all communications between any two people in a social group. We were interested in the relationship between the reports of their communication that people give and the actual communication that occurred. We conducted some experiments in which we observed people (in an office, for example) as they talked to others, and then later asked them whom they talked to. There are problems with this data collection method. It is very expensive, there are problems of reactivity (Were the informants acting differently because we were observing them?), and sometimes observers get tired, make mistakes, see what they want to see, etc.

We needed to find a group of people whose natural communications (i.e., not a laboratory setting) could be monitored automatically by a machine. Our choice was to study a group of 52 scientists who communicate regularly via a computer conference network called EIES. We obtained their permission to monitor all of their communications for 2 months. We recorded everything about the communications except the content (in order to protect informants). The data included dates, time, length, and frequency of the messages, as well as individual characteristics of informants such as age, education, amount of ex-

perience on the system, and so on.

This produced nearly 5000 pages of data, already coded, "punched", and formatted, ready for analysis. These data are free of the types of errors that would have been present (but unmeasurable) if we used human observers. The cost of collecting 2 months of a complete behavioral record on this system was approximately $10,000 for programming and computer time. We estimate that the cost to replicate this data collection with human observers and coders would be one million dollars.

EVALUATION: We were primarily interested in general questions about informant recall, so the "esoteric" nature of users of electronic computer conferencing sytems is not germaine to out research. Of course, there are many things of interest to anthropologists that cannot be monitored automatically. However, we expect that in the future there will be many creative uses of computers to collect "field" data.

MICROCOMPUTERS HELPING TO PRESERVE LOCAL CULTURES

by H. R. Bernard

Since 1962 I have been working with Jesus Salinas, an Otomi from the state of Hidalgo in Mexico. In 1972 we began to develop an orthography of Otomi, in the Otomi language, and I have been translating the ethnography into English. Two volumes of a planned seven-volume work have been completed, and the third volume gets underway this year. Publishing these volumes might have been impossible, if not for the fact that both the Otomi and the English translation were entered and produced on a word processor by a professional typist, and then corrected on a screen by Salinas and me. We used word processing software on a big computer, because, when we started in 1976, small computers cost far too much. The point is publishers found it economically feasible to publish the ethnography only because we were able to provide them with camera-ready copy of an exotic text. We found two major new benefits in writing the Otomi ehtnography on a computer-based word processor. First, Otomi has never before been a truly written language. That is, to my knowledge, there have been no Otomies who have been genuinely facile and prolific writers in Otomi. Now, every sentence ever written by Salinas in Otomi is on a retrievable computer file.. We can study the text for linguistic patterns, and we can test to see if there are patterned changes over time. We have recorded a single instance of an "experiment" that goes on millions of times a year, and yet is never recorded: people learning to be literate. To

understand this process, it is useful to have data that enable us to measure cognitive and stylistic changes that occur as a result of this common experiment.

Second, we are finding that inexpensive computers have the potential for preservation of Otomi and many other local cultures around the world. We are now considering placing a $5000 computer with word-processing and data base management software, in Salinas's village. This will allow him and his fellow Otomi to write down folktales, local medical knowledge, and Otomi lore that he and his colleagues want to preserve. Much of this, of course, could be done with a typewriter. The advantage of a computer, though, is that people can modify the data base, interrogate it to see if something has already been entered, and so on.

Today, it seems, there is an increasing demand among Native American groups (Aleuts, Samoans, Paiutes, and others) to develop cultural data bases for future generations, to preserve their languages in written form and to support their identity with locally produced newspapers about their communities. All of these functions can be well served by providing people with the computer systems and simple training that the Otomi project has used.

A CONCORDANCE OF ANDEAN FOLKSONGS

by Ralph Bolton

In a study of the relative salience of color terms in various cultural domains, I found myself counting color terms in folksong texts, going through the texts of approximately 1,000 songs line by line. Such work is both tedious and far from error-free. Since I intended to carry out more intensive analyses of the texts of those folksongs, its occurred to me that it would be helpful to automate the process of searching for lexical items in these texts. I decided that my future work on the folksongs would be facilitated by generating a concordance. Concordances have been found to be useful to scholars in various disciplines in the humanities (classics, literature, and so forth), and they can be produced rather easily with the assistance of a computer.

The texts of 1,082 waynos, which I had collected over a period of years in Peru, were entered onto the computer. A program was then written that provided the following output:

(1) TEXTS. The texts of all the songs were printed out. Should it prove feasible to publish the concordance, it will be simple to make corrections and then to produce photo-ready copy of the texts;

(2) WORD COUNTS: a) the computer generates an alphabetical list of all the words in these songs and gives the frequency of occurrence of each item; b) the computer generates another list of words according to descending frequency of occurrence in this body of folksongs. Thus one can look up specific words and find out how often they occur,

or one can examine the frequency list to explore the kinds of themes that are prominent in this musical genre;

(3) CONCORDANCE: The computer then produces each lexical item and prints out the identification number and title of the songs in which that word appears and it also prints the line containing that word. This makes it possible to examine quickly all of the occurrences of a given word in context.

The uses for such a concordance are multiple. Having concordances of this type available for many cultures would greatly facilitate comparative work. Even if available only for one culture, though, it can permit more fine-grained analyses of cultural concepts by vastly reducing the amount of time needed to hunt and retrieve the relevant information from a large corpus. It should enhance accurracy and comprehensiveness in the examination of data. Publication of massive concordances may not be feasible, but once produced they could be copied and made available to interested scholars at a reasonable cost. The concordance we produced is contained on approximately 2,500 pages (five volumes). Cultural anthropologists dealing with textual materials should find concordances quite helpful.

COMPUTER ANALYSIS OF SKEWED KIN TERMINOLOGY

by David Kronenfeld

A. K. Romney, in 1965, claimed that he had come up with a reasonably automatic form of the kind of extension analysis of kin terminological systems developed by F. Lounsbury (1964-1965). Romney suggested that, since the process was automatic, even a dumb machine should be able to convey at it.

In Lounsbury's analysis, relatively extended members of a kinterm category (such as egya in Fanti) are sytematically reduced to relatively close ones (e.g., Fa Si Da So --1/2 Fa Si So, Fa Si So --1/2 Fa Br, Fa Br --1/2 Fa). These reductions are accomplished through a small set of rules, (such as Fa Si ...= Fa Mo ...) which are constant within any given system, but which vary some across systems. Romney introduced a notational scheme that more closely approximates a genealogical chart (Fa = a+m, MS; = mof) and added much detail to the specification of the problem.

In attempting to program a computer to do the analysis, I tried to copy exactly each stage of Romney's method. As the process went on several kinds of problems developed. Some of his procedures were not well enough specified to be implemented directly, and I had to provide the details. Some of his procedures seemed not to work in the form he specified here I had to find a version that would work. Sometimes problems arose that he had not foreseen--problems negating the precise form of the data representation.

The program gradually came to work by trying out all possible reductions, making, the ones that worked, and storing the one's that did not do the job.

One regenerates the original data in order to make sure that it had outlined the correct rules. Problems such as those described above appeared as an inability of the program to make correct reductions or as its making of correct ones but with many rules (as seen in the re-expansions).

The final program (described in Kronenfeld 1976) was run on a variety of terminological categories including Romney's Omaha and Aberle's Valmuk. Its procedures are quite general in that its procedures apply to many systems (Crow, Omaha, Dividium, Hawaiian, "Kalmik", "Trobriand", etc.). It is exhaustive if not perfectly so, in the sense that its small set of basic procedures does almost all the work before final moe and hoe procedures are needed. It is moderately complete in the sense that not all terms are reduced to two kernel kintypes or less (as opposed to always one).

The usefulness of the program is not to do the work of actually analysing terminological systems. After the pioneering work of Lounsbury and Romney it is faster to do that analysis by hand than even to type the data into a machine! The benefits come in the assessment of the power and accuracy of Romney's analytic algorithm, in the improvements which the explicitness of the machine forced, and in the new analytic insights about kinship that come to be embodied in the program.

COLLECTING QUANTITATIVE BEHAVIORAL DATA

by Willett Kempton

Our research seeks to understand the cognitive and behavioral determinants of energy consumption in U.S. residences. It is a curious fact that identical residences use widely varying amounts of energy. Correlational studies have been able to explain only part of the differences by standard socioeconomic factors such as income, education, age, and so on. Studies of change in energy use through time have found no factors clearly identifying those households which reduce their energy use. Not even an individual's belief in the energy crisis or his attitude toward energy conservation explain energy consumption data. Therefore, we decided to collect more direct measures of behavior to explain patterns of energy use. Previous studies have attempted to infer behavior from survey research questions, such as "What do you set your thermostat at?" or "Do you set back your thermostat at night?". We first tried to improve on these questions by using open-ended ethnographic interviews. Unfortunately, both the survey data and the ethnographic data are unreliable. People are not always sure what their own behavior is, and they are likely to over generalize to give an idealized version. Furthermore, many behaviors other than thermostat setting determine energy consumption, some of which, like opening of windows and odors or use of hot water, are not readily remembered or categorized as energy use.

The problems with self-reporting of behavior led us to attempt

automatic recording of energy-related behavior. We now can easily wire a house with a microcomputer which measures temperature, thermostat setting, window and door openings, refrigerator and appliance use, and hot water consumption. This microcomputer does not look like those advertised as personal computers--it has no key-board or screen. Rather, it has a calculator-like keypad and display, for simple programming, and twenty wire connectors. The connectors are wired to sensors throughout the house. Every ten seconds the microcomputer checks each of the sensors; when one has changed value and time of day on an attached cassette tape recorder. One cassette will hold two or three days of data; cassettes are changed either by a researcher or by the informant. The data are stored on the cassette in a form that is usable by the main coputer on campus, thus eliminating the usual labor of coding and keypunching.

This data-recording microcomputer system gives us an unambiguous record of selected activities in the household. However, it does not provide us with the ethnographic context needed to interpret this behavioral record. We plan to combine tha automated recording with three other types of data. First, we are conducting intensive ethnographic interviews with the people whose houses are being monitored. We ask them how they regulate heat and human thermal comfort in their home, when they use appliances and hot water, and how they use them.

Second, we will ask informants about their "folk theories": how they think their thermostats work, how they think their houses lose heat, what they think are major home energy uses, and so on. We plan to do this because our previous data reveal many beliefs about heat

and energy which, although widely held, are incompatible with scientific theories, since they may be functional for household energy management. For example, some people seem to regard the thermostat as a valve; when they want the house to heat up quickly, they turn it all the way up.

The third type of data we will add is the observation of daily activities by an ethnographer. This is needed to interpret the microcomputer records. If the thermostat is changed at different times on weekdays, we want to know who is doing it on what days, and what other things they are doing which affects the variation in times. We can infer some house activities from the automatic record, using door openings and hot water use, for example. Obviously, more can be ascertained by an ethnographer, and the ethnographer also has the opportunity to ask informants why they are doing what they are doing (questions must come near the end of the observation time, because they are likely to change behavior). While the human observer has some clear advantages over the blind machine, the machine has three advantages over the human observer. First, we have clear evidence that it is less intrusive and less likely to change behavior: An earlier project found that some informants were setting the thermostat down just before the researcher came to collect the cassette data tape. Second, machine recordings are not skewed by the observer's cultural biases. We don't have to worry about the record not including a door being opened because "it was only to let the dog out." Third, the automatic recorder can record 24 hours per day, for years if necessary. This will allow us to record rare events which might never occur over the short time periods possible with

a human observer.

In summary, we combine three types of data to understand energy consuming behavior in residences: Long-term automatic recording of energy-related behavior, open-ended ethnographic interviews, and short-term observation by researchers in the informant's home. By using these three in conjunction, we hope to deduce both the regular patterns of energy-related behavior which cause energy consumption to vary widely among houses and the folk theories and other cognitive factors which guide that behavior.

MEASURING VERBATIM RECALL

by Oswald Werner

Recently I have become interested in the problem of recall. How much can be expected to be recalled by an ethnographer if he is not permitted to record or take notes during an interview? I think it is important that we test not only for recalling the gist of an interview but obtain a measure of verbatim recall as well.

I used the members of a seminar to conduct an experiment. I read to the students simple stories from the READER'S DIGEST and asked them to recall these under a variety of recall conditions.

The exact details of the experiment are not crucial here. The main point is that in this experiment I wanted to emulate a tape recorder, which captures, for example, voice tone, ethnic peculiarities of speech, accents, clever phrasing, etc. While focusing on such features, verbatim recall becomes an important measure of accuracy.

I tried out different methods, for example, word counts, counts of ideas or thematic units, and other measures. None proved satisfactory. While some of these approaches measured the recall of individual words, none were sensitive to the exact recall of longer stretches of connected speech.

At this point I had to review my APL (A Programming Language) users manual for another programming task. The matrix "outer product" struck my imagination. It functions like a multiplication table. For example,

(.x is the sympol for the "outer product"):

(1 2 3) .x (1 2 3)

results in:

1 2 3
2 4 6
3 6 9

The manual states that if the x is replaced, for example, by the = sign the "outer product" produces a 1 where there is a match between columns and rows and a 0 of there is no match.

Now, if the first vector is the original text, and the second vector the recalled text, then whenever a word in the recalled text matches a word in the original there will be a 1 and if there is no match, there will be a zero. If there are longer sequences of connected speech that are matched, all 1 will be on a diagonal from left top to right bottom. For example, if the original text is (a b c d e f g h) and the recalled text is (a b d f g h), the resulting matrix is as follows

	a	b	c	d	e	f	g	h
a	1	0	0	0	0	0	0	0
b	0	1	0	0	0	0	0	0
d	0	0	0	1	0	0	0	0
f	0	0	0	0	0	1	0	0
g	0	0	0	0	0	0	1	0
h	0	0	0	0	0	0	0	1

and it is clear that there is one word recalled in isolation (d), there

is a recalled sequence of two words (a,b) and a sequence of three words (f,g,h,). It is easy to write a small program, that tests occurrences in the diagonals.

There is but one small problem left: Words are character strings, not single characters or numbers that can be easily compared. However, from another application I have a program that converts character strings to unique integers. In combination with that program my evaluation of verbatim recall works very well. For very large tests (e.g., my largest is 1,700 words) the original matrix may be split into submatrices. That way the matrix cannot exceed the capacity of the microcomputer's memory.

Matrix manipulations and conversions are particularly easy to program in APL, which is now available for CP/M operating systems and the Z80 microprocessor. Both are found in a large number of microcomputers, including APPLE II.

COMPUTER SIMULATION IN ETHNOGRAPHIC RESEARCH

by Stuart Plattner

As part of a study of the economic decision making of merchants at Soulard Farmers Market in St. Louis, Missouri (Plattner 1982), I wrote an interactive computer game (called SOULARD) which allows the player to simulate the decisions of a market merchant. I wrote the program for two purposes: to explicitly test my own understanding of the vendors' decision making, in a manner similar to my previous computer program PEDLAR which simulated long-distance itinerant peddling (Plattner 1975); and as an elicitation device, to have vendors interact with the program which simulates their own busineses. By imitating their own economic behavior in a controlled, imaginary, yet ethnographically realistic setting, I was able to get vendors to talk about decision criteria that otherwise would have required extensive interviewing to bring out.

I observed and interviewed vendors at the wholesale market (where they bought their produce), at the market (where they sold it), and also in their homes. When I brought my portable terminal to their homes we first discussed the general issues of decision making in their business. I tried to point out strategies and constraints more than I did in previous interviews. They then played the game while I took notes on their reactions. Their actions, the decisions they made while playing, were recorded on the paper printout from the terminal. I questioned them intensively on why they made each decision. Sometimes a ques-

tion would lead into a long discussion; the noise, excitement and "real-time" pressure that prevents intensive interviewing during market days makes this kind of interviewing impractical, but with SOULARD, the terminal waits quietly while we resolve some difficult point.

"Simulation-interviews" such as I describe here are most useful in dealing with relatively abstract issues (e.g., the relationship between the number of items sold per stall and gross sales) in a concrete way (e.g., "why don't you also buy the apples offered in the program this week for your stalls?"). The concreteness and realism of the game-choices allows the ethnographer to lead informants easily into discussion of very abstract issues. Having the referent of an abstract concept, the context in which it came up in the discussion, and the informant's behavior on paper (the computer printout) facilitates the jump from specific to abstract.

My informants (who did not, as a rule, have more than a high school education) accepted the computer terminal's behavior. They did not mistrust it, although they misunderstood it. They thought that it was smarter than they, since it was a computer, and wanted it to tell them how to solve their decision problems. Discussing what sort of information would be needed for the computer to be smarter than informants was a good way to justify my need for information.

The limitations and disadvantages of using computers in the field are serious. I used a remote terminal and communicated with the main computer through the telephone system. The problem of telephone linkage and access to an overloaded system make this a bad alternative. A portable microcomputer would solve these problems. Computer programs take enormous amounts of time to create and modify, especially for people like me whose computer expertise is spotty and ad hoc. The

demand of the computer for specialized knowledge seem endless, and it is easy to lose sight of the fact that the main goal is analysis of the real world. Computer simulation programs can help one learn about a natural system one is studying, but I would not use them if they were not fun. And they are useful for investigating strategies that are complex and occur under pressure. The Soulard market is a good example: Vendors cannot be bothered to sit still for intensive interviews during the main market day when the bulk of their business is done. They are busy, tired after having worked half the night in preparing their produce, too anxious if sales are going poorly or too excited if sales are going well to indulge the fieldworker's reflective questions. Interviews away from the market were highly productive and produced the information I used to construct the model at first. Once my hypotheses were formed into a formal model, I found the simlulation game approach productive of new understanding and corrective of old misunderstandings.

For example, the computer program lists a selection of produce available for purchase at the wholesale market each week, to simulate "shopping" the wholesale market. At first, all I thought merchants needed to know about the produce to decide whether to buy it or not were the type of produce, its wholesale cost and the existence of special deals (where it is sold below cost). Yet informants had trouble deciding on that basis. They needed to know what retail price the produce had sold for the previous week. The comparison of last week's product this week, given its cost, allowed them to infer the state of demand and supply.

Of course a good fieldworker and a good informant will produce these insights sooner or later. Yet the benefit of simulation-interviews is that they allow the interviewer (to speed up the process).

MATERIAL ENTAILMENT ANALYSIS

by Douglas R. White

PURPOSE: To describe tendencies towards subset-superset relations, mutually exclusive sets, and co-exhaustive sets in binary data on set membership. Chains of subset/superset relations between variables form cumulative Guttman scales or implication hierarchies. Multiple and cross-cutting hierarchies provide a multidimensional generalization of Guttman scaling. The results may be expressed in venn diagrams, entailment digraphs, or first order predicate logic of if-then relations.

DATA NEEDED: A rectangular matrix with case as rows and variables as columns, coded 0 for item absences, 1 for presences, and 9 for missing data (row item i is/is-not a member of column set j).

METHOD: All 2 by 2 tables are examined for (1) direction and strength of correlation, and (2) percentage exceptions to entailments consistent with the correlation. Statistically relevant entailments are determined by a signal detection procedure. Relevant entailments are admitted to the final entailment structure in order of least exceptions and strongest correlation only if they pass a partial correlation test for transitivity.

ANALYSIS: Results are printed in three formats: (1) an ordered list of relevant entailments, showing which pass and which fail the transitivity test; (2) a matrix showing all entailments up to the max-

imal percentage of exceptions; (3) sets of entailment diagrams, [redacted] for the maximal level of exception, and several for lower levels.

EVALUATION: The analysis represents improvement over the implication analysis used by D'Andrade (1979) in that (1) signal detection is used to test the null hypothesis for eadh entailment; (2) the maximal level of exceptions is non-arbitrary--i.e., determined by signal detection; and (3) all entailment chains have passed a stringent test of transitivity. These are also the advantages over the ordering theory methods of Airasian and Bart (1973) which are similar to those of D'Andrade.

Extensive testing of the program against various datasets shows that the results are highly satisfying for rules of implication in limited and well-defined domains.

SIMULATION OF COMPLEX SYSTEMS

by David Kronenfeld

I once got worried about whether or not skill made any difference in professional sports leagues. That is, since the nature of baseball guarantees that there will be winners and losers, I wondered whether some teams won more consistently (within a season or across seasons) than they would have been expected to by chance. This problem can be solved mathematically, but for me it was simpler to write a computer program that "played" several seasons worth of baseball.

The program asks the user for a list of teams, for the number of games each team plays with each other, and for the number of seasons to emulate. It then procedes to play each game by drawing a random number between 0 and 1 and giving the first team the win if the number is less than .5 and the second team a win otherwise. It prints out the standings and the wins and losses for each season. This much could have been calculated directly without mimicking actual games, but this way allows one to experiment with some further complications. The user could also modify "weights" determining each team's odds of winning a game according to "the attractiveness of the city," "the wealth of the owner," and "how they did the season before". The first two weights were constant for all seasons and last was re-calculated for each season.

When every team had an equal chance of winning, many of the seasonal team statistics seemed very realistic. But some teams were win-

ning the pennant with too few wins (80-90 vs. 100+ that was typical for the National league in the 50's). The weights increased the number of wins for a simulated pennant race from 80-90 to 90-95.

The main conclusion was that skill was worth about 5-10 games a season to the winner, the difference between 100 and 90-95. Consider this a problem in "baseball anthropology." It seems surprising that skill should make so little difference; the simulation has produced a counter-intuitive result.

This kind of simulation can be used for a variety of anthropological purposes. While not perfectly accurate, it can help the simulator clarify those places where thinking is fuzziest, and sometimes lead to very instructional results. They are easy to do and can be done on very small computers.

DEALING WITH TRIADS DATA

by David Kronenfeld

A triads test can sometimes provide useful data to the anthropologist studying meanings or symbolic behavior. In a triads test, each subject is presented with many sets of three words (or pictures, photos, objects, etc.) such as

House Car Garage

and is asked to choose the word representing the concept most different from the other two. These data can be used to compare subjects to one another, construct componential analyses, and so on. Unfortunately, they are a pain to construct, a pain to randomize, a pain to score, and it is a pain to enter the data into a computer.

But now, we have computer programs that relieve the drudgery. The words wanted in the test are typed into the computer by the person administering the test. The program constructs all the traids, randomizes their order of presentation, and the order of items within each triad. It prints out each form and later solicits the answers from the anthropologist. (If the subject can use a terminal, the computer records the answer and the reaction time.) It calculates and prints out the individual data matrices. If several subjects take the same test, it re-randomizes each time and constructs aggregate similarity matrices. All data are saved by the computer for future use. Old tests can be called up for new informants, and so forth. In short, the machine does all the boring work. Other versions include extra features: One corrects for a tendency of lazy subjects

to select the third choice in each triad; another implements the balanced block design of Burton and Nerlove (1976), a technique which permits a much larger number of concepts to be presented by giving each subject a subset of the total set of all triads.

The program makes the design, administration and scoring of triads tests quite easy. It also makes taking the test very easy, and maybe even fun. We have used the tests on a variety of subjects ranging from adults to six-year-old children including illiterates, and "natives", and few have had any trouble with it.

Any kind of questionnaire could receive the same kind of machine organization, administration, and instant pre-analysis. The program fits easily in a small computer, one that could be used in field sites where power is available (solar power is available everywhere).

APPENDIX III

FIELD NOTES SURVEY

I. Biographical Data

1. Degree Institution _____ 2. Date of degree ___
3. Year of Birth _____ 4. Respondent's sex _____
5. Is your dissertation based on your field research? ___Yes __No
6. Current position _____
7. Anthropological specialities (please list subfields in order of importance):

a. _____ b. _____
c. _____ d. _____
e. _____ f. _____

II. Background Information on Field Research

1. Dissertation fieldwork location (country) _____
2. Name of tribe or society _____
3. Type of community studied (urban, village, tribal, etc.)

_____

4. Size of population actually studied _____
5. Total time spent in the field _____months.
6. Language(s) used by you in the field _____

_____

7. Where did you live in relation to the study population? ___

_____

_____

8. Please describe your housing situation: _____

_____

_____

_____

++

If additional space is needed to answer any of these questions, please continue on the back of the questionnaire pages.

9. Was there electricity in your residence in the field? ___Yes __No

10. If desired, could you have obtained a work place with electricity in this field research project? ___Yes ___No

11. Please describe the means of transportation to your fieldsite:

III. Dissertation Topic

1. Summary of dissertation (attach an abstract, if possible):

2. Please list the subfields in the discipline to which your dissertation corresponds:

IV. Equipment

Please indicate the kinds of equipment you used in the field:

1. ____Yes ___No Typewriter ..... Manual _____ Electric _____
2. ____Yes ___No Calculator ..... Type _____
3. ____Yes ___No Video Equipment .. Color _____ B&W _____
   Portable ___ Nonport, _____
4. ____Yes ___No Still Camera ..... Type(s) _____
   Extra lenses _____
5. ____Yes ___No Movie Camera ..... 8 mm _____ 16 mm _____
   Syncsound ___Yes ___No
6. ____Yes ___No Tape Recorder .... Cassette _____ Reel _____
   Brand(s) _____
7. ____Yes ___No Computer Equipment. Specify: _____
   _____
8. ____Yes ___No Other _____
   _____
   _____

9. Please describe any problems associated with equipment use (or non-use) in your field research:

V. Data Collection Methods

We are interested in the methods, techniques and instruments used in the field. Would you please indicate which of the following you employed in your dissertation research and give a rough estimate of the percent of research time you devoted to each:

	Yes	No	% of Time	Method	Mainly for Which Topics?
1.	___	___	___	Participant Observation	
2.	___	___	___	Key Informant Interviews	
3.	___	___	___	Group Interviews	
4.	___	___	___	Life Histories	
5.	___	___	___	Genealogies	
6.	___	___	___	Projective Tests	
7.	___	___	___	Formal Elicitation Procedures	
8.	___	___	___	Census	
9.	___	___	___	Household Inventory	
10.	___	___	___	Standardized Questionnaires	
11.	___	___	___	Intensive Economic Survey	
12.	___	___	___	Intensive Demographic Survey	

	Yes	No	% of Time	Method	Mainly for which topics
13.	___	___	___	Coded Behavioral Observations	
14.	___	___	___	Locally Available Written Records	
15.	___	✓	___	Archives, Government Records	
16.	___	___	___	Tape Recordings	
17.	___	1	___	Extensive Photography	
18.	___	___	___	Other (specify)	

19. To what extent would you characterize your research as "quantitative" or "qualitative"? (Circle correct percent.)

Quantitative										Quantitative
0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
100%	90%	80%	70%	60%	50%	40%	30%	20%	10%	0%
Qualitative										Qualitative

20. Did you use field assistants? ___Yes ___No

21. If yes, how many? _____

22. If yes, please specify their level of education/training and how you used them:

23. Please discuss the main problems you encountered in doing your fieldwork:

24. If you were to do the research again, what changes would you make in how you collected your data?

VI. Note Taking and Information Recording in the Field

1. Please describe your method of recording field data (note taking etc.):

2. Estimate how much time on an average day in the field you spent writing notes (original and final forms):

3. Type of notebook, paper, etc. commonly used to record original notes. Please describe in detail.

4. Were you able usually to take notes publically? ___Yes ___No

5. Please describe any situations or topics when this was usually not possible:

6. To what extent were your original notes written in public (while observing, interviewing etc.) or in private (later on, in the evening, etc.)?

7. Did you use covert note taking techniques for sensitive topics? ___Yes ___No

8. If yes, please describe.

9. What memory techniques did you find useful in supplementing original notes that were incomplete?

10. In which language were the notes recorded? If more than one, indicate the approximate percent of notes recorded in that language.

11. Did you work on a dictionary of the native language? ___Yes ___No

VII. Organization of the Final Version of Notes

1. Did the final form of your notes differ from the original notes? ___Yes ___No

2. If yes, please describe the extent of difference.

3. When and how did you prepare the final version of your field notes?

_____

_____

_____

_____

4. Were they: _____ typed? _____handwritten?
Other? (specify): _____

5. Did you obtain advice and comments on your field notes from an advisor? ___Yes ___No
Explain: _____

_____

_____

6. Would such a process have been helpful to you? ___Yes __No
Explain: _____

_____

_____

7. Language(s) used in the final form of notes. If more than one, specify the percent of notes in that language.

_____ __ _____ ___ _____ __

8. Type and size of paper, cards, or other materials on which the final notes were recofded:

_____

_____

_____

9. Please estimate the total amount of material you accumulated during your field research.

	Approx. No. Pp.	Approx. Words/Page
Qualitative observations		
Census		
Questionnaires/surveys		
Texts (tales, songs, law cases, etc.)		
Archival records		
Genealogies		
Other		

10. Number of copies made of the final version of notes. 1

11. What did you do with the additional copies made?

12. Specifically, did you send a copy of your notes back to a dissertation advisor or other professor while you were in the field? ___Yes ___No

13. Did you use a system for coding information while in the field? ___Yes ___No

14. If yes, please describe it.

15. Specifically, did you use Murdock et. al.'s Outline of Cultural Materials to order your data? ___Yes ___No

16. Please describe any problems you may have had in coding, classifying or ordering qualitative field data.

17. Did you further code your material after leaving the field?
___Yes ___No

18. If yes, please describe:

19. What filing system did you use for your notes while in the field?

20. What filing system did you use thereafter?

21. To what extent did you use your notes while you were in the field?

22. What precautions did you take against environmental damage to your notes while you were in the field?

23. What precautions did you take to safeguard the confidentiality of your materials while you were in the field?

24. How have you stored your materials for safekeeping subsequently?

VII. Use of Notes During Analysis Phase

1. Did you use pseudonyms in your notes? ___Yes ___No

2. Do you use or do you plan to use pseudonyms in your publications? ___Yes ___No

3. What problems did you encounter in using your notes during the analysis and writing phase of your dissertation research?

4. Are your fieldnotes in a form that they could be used by other scholars? ___Yes ___No

5. Have you allowed any other scholar(s) access to your field data in unpublished form? ___Yes ___No
Please elaborate:

6. What is your opinion on the sharing of data trough the development of a central ethnographic data bank for anthropology?

7. In addition to your dissertation, have you written other documents based on your dissertation field work? ___Yes ___No

8. If yes, please specify the number of each:

_____ published monograph or book

_____ published articles or chapters in edited books

_____ article manuscripts, written but not yet published

_____ monograph manuscripts, written but not yet published

IX. Computer Usage

1. Was a computer used in any stage of your dissertation research?
___Yes ___No

2. If yes, please describe:

3. Did you do your own programming? ___Yes ___No

4. Which programming languages do you know?

5. Do you think you could have made use of the computer, or better use of it, in your research? ___Yes ___No

6. Please explain:

X. Previous Training in Notetaking Methods

1. Was training in note taking or data recording part of your graduate education? ___Yes ___No

2. If yes, please describe it:

3. Were specific methods recommended? ___Yes ___No

4. If yes, please describe them:

5. What is the source of the note taking or data recording methods you used in the field?

6. Did your field situation affect your methods of note taking?
___Yes ___No

7. If yes, please describe:

8. In retrospect, could your research have been improved by some (or more) training in data recording techniques or other field methods? ___Yes ___No

9. If yes, please explain:

XI. Recommendations

1. Please discuss any recommendations or ideas you might have that could contribute to an improvement in field techniques in anthropology.

A Reminder: we would greatly appreciate receiving an abstract of your thesis and a copy of any discussion of field methods you included in your thesis. Thank you!

APPENDIX IV

WE ALL DO IT, BUT HOW?

A SURVEY OF CONTEMPORARY FIELD NOTE PROCEDURE

Field notes, in all their variety, are a vital part of all anthropological research. Their production consumes a significant portion of a field worker's time and energy. They are also essential for preserving information for future reference and, as time passes, tend to be regarded as a form of basic data, encapsulating all that remains of the immediacy of the field experience. Fieldnotes also play an important role in converting raw information and observations into a coherent, at least somewhat organized, form that later can be usefully re-read and further analyzed by the fieldworker, and perhaps others as well.

This paper presents a compilation of information on field note procedure, drawn both from published sources (1) (ethnographies, reports of fieldwork experiences, handbooks of method) and from a questionnaire (2) sent to recent recipients of the Ph.D. in socio-cultural anthropology. Though the emphasis of this paper is on "qualitative notes," those that record the detailed, infinitely variable and often impressionistic facets of a culture, it is necessary to put these in perspective by first outlining the full range of types of data records used by fieldwirkers. We will then briefly summarize the "shoulds" of qualitative note taking, the procedures and standards advocated om the literature, before turning to my central concern: the forms of field notes employed by fieldworkers. Against this background we will finally discuss some preliminary findings from our questionnaire survey.

RANGE OF DATA RECORD TYPES

It is the rare anthropologist who uses only a single format to record all field notes. Most researchers selectively make use of a variety of data record types, in part because it is convenient to keep different types of information seperate and also because some types of information are best suited to specific types of recording systems. But in spite of this diversity, all types of data records are not used with equal frequency by field researchers. Judging from published accounts of fieldwork, as well as method handbooks, field note types can be divided roughly into three groups: those that seem to be used by most anthropologists, those in fairly common use, and the more specialized forms which are mentioned only occasionally.

Data record types used by most anthropologists include:

-detailed, qualitative field notes (3)

-on-the-spot jottings made in pocket notebooks (4)

-personal journal or fieldwork diary (5)

-preliminary analyses, stocktakings, field reports (6)

-photographs (7)

-notes or copies of official records, archival material, publications (8)

Individual researchers many combine some of these forms, writing theoretical analyses as part of a fieldwork diary, of keeping a single journal in which both "objective" fieldnotes and "subjective" material are recorded.

A second category of data record types, those in fairly common use, includes:

-a community census of standard background information (9)

-a separate file of information on individuals or households (10)

-extensive genealogies (11)

-separate records of major public events, lengthy rituals, court cases or formal interviews (12)

-questionnaires or schedules on a specific topic (13)

-texts or essays written by informants (14)

Typically these data records segregate the specific topics of information collected by the researcher. They are often made in a standard format and/or on a special form.

A third group of specialized data records, reported occasionally, includes:

-tape recordings of music or oral literature (15)

-copies of the fieldworker's correspondence (16).

-informants' drawings (17).

-material culture collections (18).

-separate index of the content and context of photographs (19)

-movies and videotapes (20)

-a "noise record" of typical sounds (21)

-plant and/or animal collections (22)

-meteorological and climate reports (23)

-tape recorded life histories(24)

-tape recorded information on conceptual categories (25)

-linguistic notebooks, word cards (26)

-record of the abbreviations used in notes (27)

This list probably underestimates the great variety of types of anthropological field notes. It is important to remember that each type has techniques that are specifically appropriate to it. Our primary interest here is in the detailed, qualitative notes generally used to record information collected through observation, casual conversation and interview. Serving both as a catch-all for and the standard repository of detailed information, these are probably the type of data record most commonly meant by the general term "field note." Often, though not invariably, these notes also constitute the bulk of recorded field material.

THE "SHOULDS" OF QUALITATIVE NOTE TAKING

While fieldwork handbooks rarely describe note taking and associated data retrieval tecniques in any detail (28), virtually all advise

researchers fo follow some general, methodological guidelines. As might be expected, the scope and emphases of these recommendations vary from author to author. The following list of fifteen "shoulds" is a composite drawn from diverse sources of published advice. Writers are in agreement, except where noted otherwise, that field notes should:

(1) Be extensive and detailed. A generally approved principle is to record more details than the researcher originally thinks are absolutely necessary, because it is impossible to know from the outset what will be relevant information. Notes should systematically contain verbatim quotes, local expressions and categories of thought, close paraphrases as well as detailed descriptions of events and activities. (29)

(2) Be written up as soon as possible. Doing this ensures that the "immediate realities" of the research situation will be preserved and that the data recorded will be as accurate as possible. (30)

(3) Include the source of the information and the context in which it was obtained. A standard heading containing this information is often recommended. It is also considered important to distinguish between direct observations, answers to questions and volunteered information, either through a system of abbreviations or in a narrative form. (31)

(4) include (or be supplemented separately by) a personal record of the field research. This should include information on such topics as the researcher's subjective reactions, initial and subsequent impressions, daily activities, problems, fears, confusions, pleasures, mistakes and ideas. It should be written daily. (32)

(5) Be well preserved and safegaurded. Fieldnotes must be protected against accidental loss, theft, environmental damage and natural catastrophe. Using high quality writing materials is recommended, as is making more than one copy. These precautions should be employed not only for qualitative notes but also for all the information recorded in separate formats. (33)

(6) Be written in language that is as concrete and descriptive as possible. Observations should be recorded at a low level of abstraction and should preserve the "sense evidence" on which any descriptive gene-

ralizations are based. (34)

(7) Record data in a usable manner. The original researcher must be able to find information that has been previously recorded. To this end, an appropriate note format should be selected and a system of data retrieval should usually be designed into the system. Some, though not all, anthropologists also believe that fieldnotes should potentially be usable by other researchers for comparative purposes. (35)

To achieve these goals, a researcher should:

(8) Be sensitive to the effect of note taking on informants. Usually this means that until rapport is securely established and/or until informants agree to note taking, information must be recorded later and in private. (16)

(9) Pause to make on-the-spot jottings, writing down key words, common idioms, quantitative information, and lists of important events or subfield notes. If these jottings cannot be made publicly, fieldworkers must make use of covert methods (cf. Sturtevant 1959) or of any available privace (i.e., frequent trips to the toilet, returing briefly to one's car or room etc.). (37)

(10) Be aware of own biases and weaknesses in note taking and try to compensate for them. It is important to try to overcome observational biases, to direct attention to features that tend to be neglected, to learn the limits of one's own attention span and to develop methods for increasing recall ability. (38)

(11) Review field notes periodically. Doing so improves field note quality by allowing gaps in information to be filled before leaving the field, by allowing contradictions and disparaties to be pursued and clarified, and by helping the researcher to recapture the fresh viewpoint with which he or she began fieldwork and again to become aware of significant phenomena that had come to be ignored or taken for granted. This review can also be extended by sending copies of field notes to a colleague (or advisor) for comments. (39)

(12) Digest field material by making written analyses of it. This can be done by "brainstorming" as part of a fieldwork diary, writing "posi-

tion papers" that summarise the current state of the researcher's knowledge, by preparing "short essays" on specific hunches and patterns, as well as by taking short breaks to present papers in seminars and conferences. (40)

(13) Always keep data recording materials ready and available for use. They should be carried with the researcher or kept with a flashlight by the bed at night. (41)

(14) Keep a record of abbreviations, symbols or initials used in notes. With the passage of time, the meanings of even the seemingly most obvious abbreviations may no longer be clear. (42)

(15) Not become a victim of one's own note taking system. The time and effort spent on paper work should be rewarded by greater recording efficiency and more comprehensive data. If it is not, recording and retrieval systems should be modified. The costs and benefits inherent in every system of note taking should be understood and weighed in choosing the form that is used. (43)

Given that researchers attempt to follow many, if not all, of these principles, we can now turn our attention to the variety of forms that can be used to record qualitative field notes.

FORMS OF QUALITATIVE FIELD NOTES AND ASSOCIATED DATA RETRIEVAL SYSTEMS

Potentially there are two related functions to be fulfilled by field notes. The most basic function, performed with some degree of competency by all systems entails recording accurate and comprehensive information for future use. Additionally, some systems also feature a means of retrieval, commonly by indexing or coding, that allows information, once recorded, to be found again as needed for cross-checking, tabulation or review. Most anthropologists probably would agree with Perlman (1970:311) that an adequate system of note taking should be "sufficiently systematic so that it is possible to know and to find relatively easily the kinds of data already collected."

There would be much less agreement, however, about the extent to

which a note taking system should be structured by considerations of effective data retrieval. John Gulick, who advocates using a note taking system oriented primarily to recording information, advises leaving any indexing and coding of notes to a post-field stage of analysis on the grounds that they cannot "catch the immediate realities of life" if they are written with preconceived categories in mind (Gulick 1977:100). An opposite point of view, well presented in Boissevain, emphasizes the necessity of an in-field analysis of field notes, holding that "unless what is observed is digested thoroughly while in the field, it loses meaning and therefore value once you leave" (Boissevain 1970:83). Note taking systems that include a systematic means of data retrieval are presumably an essential aid to such "digestion," even though they probably require more fieldwork time.

Clearly there are many dimesions to the dilemma of which note taking system to choose. A researcher must consider his or her own needs, skills and temperament as well as the field conditions in making this decision. It is necessary to remember that the advantages of any system do much to create its disadvantages.

We would now like to summarize the main forms of field notes used by anthropologists (and other social scientists who do qualitatively-oriented, participant observation fieldwork). Sources for this summary consist of some fifty references to note taking method, usually giving brief and/or fragmentary information, which we ferreted out of the ethnographies, journals and method handbooks that provide any information at all on this topic. The six forms we will describe below can best be viewed as a continuum, beginning with those structured mainly by a data recording function and moving through to those whose format is increasingly determined by data retrieval considerations.

FORM I: PLAIN JOURNAL OR DIARY

Field notes are recorded in a chronological, journal style with entries usually separated only by dates. There is no system of data retrieval built in for field use, though indexes and topic categories may

be added later during post-field analysis. Some researchers write a single set of notes, combining "objective" information with their more personal or subjective material. There is no exact information available on the typical quantity of notes produced using this system though one user, Richard Nelson (1973:11), noted that after eleven months of research, he had 450 pages of notes recorded in "bound notebooks" of an unspecified size. (44)

FORM II: SEPARATE NOTEBOOKS FOR DIFFERENT TOPICS

In the only published account of this method that we have located (Hostetler and Huntington 1970), the researcher used a set of seven 5" by 8" spiral notebooks, each allocated to a general topic, such as "men's work," "movement to and from the settlement." In this case, notes recorded in this form were supplemented by tape recorded observations sent away for transcription since the researcher's field privacy was extremely limited. (45)

FORM III: INDEXED JOURNAL SYSTEM

The notes themselves are recorded in a chronological, journal-type format, either on ordinary-sized paper or in bound notebooks. In either case, a large margin is left on the left hand side and the pages are numbered. The appropriate code words (or numbers) for significant topics are written in the margin of each page of notes, and these are then cross-referenced with the date and page number of the notes. The index thus created is usually placed at the end of the notes indexed. Indexing systems vary in form and complexity (cf, descriptions by Boissevain 1970 and Whyte (1955, 1960 and referred to in McCall and Simmons 1969), though most use separate indexes for individuals and for topical categories.

Those who use this form of field notes often keep other supplementary types of data records such as a personal diary, census records, files on individuals, topical file folders for photographs, clippings and printed matter, and so on.

Boissevain (1970) provides the only published information we have located on the quantity of information recorded by a user of this sys-

tem. After fifteen months of field research, he had filled 1500 pages in his journal notebooks, an estimated total of about 360,000 words. (46)

FORM IV: JOURNAL WITH PAGE COPIES ALSO FILED TOPICALLY

Notes are written or typed in a narrative format, with provision for making several copies of each page, either by using carbon paper or by typing directly on duplicating stencils. The notes are categorized or coded in the margins, the pages are reproduced, and a copy of the relevant information is filed under the topics that have been chosen to organize the field research. The original pages are usually retained in chronological order to serve as a fieldwork journal.

Predictably there is great variety in the categorizing and filing procedures used (cf. Gulick 1977 for the "Rimrock" approach, McCall and Simmons 1969, Perlman 1970, and Sasaki 1960 for the system used by the Cornell University Southwest Project). Some researchers have developed their own topical categories. Others have used Murdoch et al.'s Outline of Cultural Materials or other comprehensive outlines. Some users, whose ability to produce large numbers of duplicate copies is limited, cut the pages of notes into separate pieces by topic and file these. More commonly, though, intact pages are filed under each heading.

As the testimony of Perlman (1970) indicates, this system may involve more paperwork than an individual researcher can easily perform. It may be most workable in a large scale project where several researchers must share their fieldnotes and where secretarial support is available. We located no published information on the quantities of field notes produced in a typical fieldwork period using this method. (47)

FORM V: TOPICAL FIELD NOTES

Here the researcher categorizes his or her data as it is recorded so that each chunk of information is written, at least ideally, on a separate sheet and can be filed under the relevant topic heading. Particularly complex or lengthy accounts may require additional cross-referencing. In categorizing this material, some researchers have used prepared cultural outlines while others have relied on a system of their own devising. In any case, the category system can be amended and re-

fined as the research progresses.

Characterisically, topical field notes are recorded on smaller sized papers or cards. "Note sets," a sheaf of paper interspersed with carbons, are prepared to produce at least one copy and sometimes as many as six. Depending on the number of copies available, the notes may be filed under a variety of topics, chronologically and/or by individual informant, and a copy may also be sent out of the field for safekeeping. Topical fieldnotes may be either typed or handwritten. They may be the original data record produced or may be prepared in privacy later as an expanded form. One essential feature, however, is that a system must be developed for inscribing each note with essential background information, such as the name of the informant (and possibly other information about him), the context in which the data were collected, the researcher's initials, the main subject matter and the date.

In this system a separate personal diary is usually kept to provide context and background. Researchers may also record specialized information, such as genealogies, long descriptions of meetings and ceremonies, household censuses and standardized interviews or surveys, in more convenient formats.

Published descriptions of this form of field notes are fairly numerous, (48) but only two provide information on the quantity of data records produced. Williams (1967) estimates that his handwritten "note sets" averaged about six sentences per page, with daily production ranging between 40 and 60 pages (not including the six copies he made of each page). If a daily average of 50 pages of notes were made under this system, a year of fieldwork would result in some 18,000 pages of notes (originals only). Norbeck (1970:256) estimates that the 5" by 8" file of notes he collected in about a year's research was "equal to perhaps 2000 manuscript pages." E. H. Spicer and John Honigmann are credited by various users for originating or disseminating this note taking system.

FORM VI: DOUBLE INDEXED FIELD NOTES

In this system topical field notes are first prepared as described above, either on papers or on sort cards. The researcher then recodes or

more finely categorizes the information on each sheet, usually by noting a topic or number in the left hand margin. Some method of cross-indexing or retrieving these additional topics is then used.

Four varieties of data retrieval for this form have been described above, either on papers or on sort cards. The researcher then recodes or more finely categorizes the information on each sheet, usually by noting a topic or number in the left hand margin. Some method of cross-indexing or retrieving these additional topics is then used.

Four varieties of data retrieval for this form have been described in some detail in the literature. (49) Yengoyan's (1977) method is perhaps the most elegant, making use of 5" by 8" Burroughs Unisort cards (Form Y-9) which have 91 punch categories around their border. These margin holes are assigned to include the cultural categories, topics and individuals relevant to the study. Fine indexing of information on each card is done by punching the margin hole(s) corresponding to the topic(s) noted in the card's margin. This allows the cards with information on a specific subject to be separated out with a sorting needle. Yengoyan found that three to ten cross-references were usually sufficient.

Boissevain (1970) wrote topical notes on regular cards, recategorized the entries and then transcribed the topics into a master index. Wolff's (1960) system was basically similar but involved putting topical field note clippings in subject envelopes, and then appending two lists of cross indexes to each envelope.

Honigmann's (1970) system of double indexing makes use of an 8" by 12" looseleaf notebook in which certain pages are reserved for the major topics of interest in his study. The content of each page is then re-indexed and these topics are cross-referenced by being entered in a separate index volume.

A POSSIBLE SEVENTH FORM???

A seventh form of field notes is apparently now possible, involving the computer storage of qualitative field data. The development of this method is very recent and constitutes the subject of another chapter in this report. At the time of the writing of the present chapter there

existed no published accounts of this form, although articles by Denham (1975, 1977) contained information concerning his development of an exploratory computer recording system in the field. The invention of this new form is occurring in the late 1970's and undoubtedly will be advanced significantly in the 1980's as a result of the so-called microcomputer revolution now underway.

SURVEY OF CONTEMPORARY PRACTICE

We would like to present some preliminary findings from a questionnaire on field note methods that we sentout in 1979 to approximately 200 recent Ph.D.'s in socio-cultural anthropology. Our sample comprised all the individuals whose socio-cultural dissertations are listed 1977-1978 GUIDE TO DEPARTMENTS published by the American Anthropological Associations.

We had a number of goals in sending this questionnaire. First, we hoped to obtain information that was more detailed, more representative and more recent than that available in print. We were also interested in obtaining a variety of information rarely discussed in published sources. For example, we are interested in how satisfied the researchers were with the note taking methods they had used, in the major difficulties they had encountered and the modifications they would make in their techniques for subsequent research, in the ways and the extent to which field situations determined or limited their note taking practices, in

the extent of training they had received in note taking techniques and other field methods and whether they felt it had been adequate, and in the source of the recording methods they had used. We also hoped to elicit any novel or unique data recording systems that these researchers may have developed.

Thirdly, we were interested in ascertaining whether any general conclusion could be drawn about the use of specific data recording systems. We had in mind such questions as: Is there a significant correlation between the use of a given form of note taking and the number and/or type of subsequent publications? How much standardization is there between the topic researched and the note taking methods used to record data? Is there any correlation between the note taking form and the volume of field notes produced? Or between the choice of a note taking system and the length of stay in the field?

This should give a general picture of the scope of the questionaire and the direction of our research interests. Unfortunately, I am not yet able to present our findings in any detail. We received response from only 44 individuals (a low 20% response rate) and of these, only 34 were complete and unambiquous. We plan to try to persuade more of our busy (or perhaps reluctant) informants to cooperate but our small data base restricts us at present to presenting only general answers to four relatively staight-forward questions.

First: what were the forms of data recording most commonly used?

Two forms, the plain journal (Form I) and topical field notes (Form V), were most frequently reported. Each was used by 30% of the respondents. The indexed journal system (Form III) was used by 20% of researchers. No one reported using the seperate note notebook system. The remaining forms were used by only a few researchers.

Second: What were the sources of the recording method used?

The majority of respondents (53%) reported that they themselves devised the system of note taking they had used. Most also indicated that they had not received training in note taking and had not been given specific recommendations by an advisor. In this light it is particularly

interesting that the plain journal and the topical field note forms were used with equal frequency by these respondents.

A further 29% credited an advisor or graduate training program as the source of their system. It is noteworthy that of those who learned their note taking method in this manner, a majority used either a topical or double-indexed form, both of which emphaseze systematic data retrieval. The remaining researchers (18%) credited fellow graduate students, published sources, oral-tradition or miscellaneous others as the sources of their methods.

Third: How much field time is usually spent in preparing notes?

Predictably, this varied greatly between individuals: the lowest reported was 1 1/2 hours per day and the highest was 7 1/2 hours per day. When responses were averaged on the basis of the type of data recording system used, users of the journal-with-filed-page-copies form averaged the least amount of time: 2 1/2 hours per day. Plain journal and topical fieldnote form users reported spending about three hours daily on notes, while averages for both the double-indexed and indexed-journal forms were slightly over 4 hours. Our data base must be increased before such figures can be taken as fully representative, but it is interesting that the majority of respondents spent less than the third of fieldwork time recommended as a minimum by Boissevain (1970:83) and Junker (1960:12). (50)

Fourth: What was the average volume of the notes produced?

Here also there is much idiosyncratic variation. Some researchers reported returning from the field with only several hundred pages of qualitative field notes, while others brought back several thousand. An Average figure, for what it is worth, is about 1100 pages. There was also a wide range in the number of words reported as recorded in qualitative notes, varying from a high average of 468,000 by users of the journal-with-filed-page-copies form, to a low average of 197,500 by users of the double-indexed form of field notes.

In addition to qualitative field notes, most researchers also recorded additional notes in other formats. On general average, these add

slightly less than 200,000 words to the volume of field notes. I might also add, insofar as such figures may be of interest to those developing computer recording systems, that the most words any researcher reported recording in field notes was 1,990,000 (during 18 months in the field), though several other researchers estimated that they had recorded overa million words.

Conclusion

To return to the note on which I began this paper, as we know, we all do it. It is hoped that now we know perhaps a little bit more about how we do it. The next question is, of course, how can we do it better? I leave it to other members of this symposium to propose some possible answers.

NOTES

1. It is hard to exaggerdate the lack of published information on the specific note taking techniques used by field researchers. It would seem that most anthropologists share the attitude expressed by Langness (1965:46):

> "It probably makes little difference just how one actually records his information, provided he is able to work with it later."

My bibliography lists the sources (53) that I was able to discover on this subject after combing through hundreds of publications. While additional sources are likely to exist, I feel my efforts represent a thourough search of the literature. I would be grafeful for a citation to any additional sources known to readers of this paper.

2. The idea for a questionnaire on this topic originated with Ralph Bolton, though we compiled it together. In it we seek answers to over 100 questions grouped under 11 general headings: respondent's biographical data, background information on field research, the subject of the dissertation produced, the equipment used, data collection methods, note

taking and information recording in the field, organization of the field notes, use of field notes during the analysis phase, computer usage, previous training in note taking methods, and respondents' recommendations for improvements in anthropological field techniques.

3. See: Barnett 1970:4ff, Beattie 1965:41-2, Boissevain 1970:79, Diamond 1970:140, Gonzalez 1977:137, Hilger 1960:v-vi, Hitchcock 1970:76, Honigmann 1970:40, Kiefer 1972:7, Maxwell 1970:477, Mead 1940 and 1956, Nelson 1973:11, Norbeck 1970:256, Oswalt 1963:168, Pelto 1970:265-6, Perlman 1970:311, Powdermaker 1966:94-5, Robertson 1978:24, Smalley 1960, Vanstone 1962:6, Whiting and Whiting 1973:282-315, Whitten 1970:351, Williams 1967:38, Yengoyan 1977:230.

4. See: Boissevain 1970:79, Diamond 1970:40, Freilich 1977:159, Gonzalez 1977:137, Gulick 1977:99-100, Maxwell 1970:477-8, Nelson 1973:10, Norbeck 1970:255-6, Pelto 1970:265, Perlman 1970:311-2, Robertson 1978:22, Vanstone 1962:6, Yengoyan 1977:230.

5. See: Barnett 1970:4ff, Beattie 1965:42, Boissevain 1970:79-80, Diamond 1970:140, Gulick 1977:99, Honigmann 1970:40, Kiefer 1972:7, Mead 1956:499, Perlman 1970:311, Rose 1965:10, Whitten 1970:351, Williams 1967:38.

6. See: Boissevain 1970:80-1,83; Dentan 1970:96, Diamond 1970:140, Hilger 1960:vi, Hitchcock 1970:176, McCall and Simmons 1969:76, Pelto 1970:266, Perlman 1970:311-2, Spradley 1979:76, Whitten 1970:351, Yengoyan 1977:231-2.

7. See: Beattie 1965:42-3, Hilger 1960:vi, Honigmann 1970:40, Mead 1956:495-6, Nelson 1973:10, Norbeck 1970:256, Pelto 1970:266, Perlman 1970:312, Williams 1967:36-7

8. See: Beattie 1965:41, Boissevain 1970:79-80, Gonzalez 1977:137, Honigmann 1970:66, Maxwell 1970:477, Oswalt 1963:168, Whitten 1970:383.

9. See: Beattie 1965:39-41, Boissevain 1970:78, Gulick 1977:97-8, kiefer 1972;7, Mead 1956:482, Perlman 1970:311.

10. See: Boissevain 1970:80, Honigmann 1970:66, Mitchell 1967:39, Perlman 1970:311-2, Rose 1965:10.

11. See: Boissevain 1970:77-8, Chagnon 1974:88-124, Mead 1956:482, Whit-

ten 1970:351, Yengoyan 1977:231.

12. See: Honigmann 1970:66, Mead 1940:326, Perlman 1970:312, Robertson 1978:24,131; Yengoyan 1977:230.

13. See: Dentan 1970:96, Hitchcock 1970:176, Maxwell 1970:477, Sasaki 1960:x.

14. See: Beattie 1965:30-4, Boissevain 1970:80, Perlman 1970:312, Robertson 1978:25,131-2.

15. See: Gonzalez 1977:137, Norbeck 1970:256, Whitten 1970:383.

16. See: Honigmann 1970:40, Robertson 1978:1-2.

17. See: Honigmann 1970:66, Mead 1956.

18. See Gonzalez 1977:137.

19. See Williams 1967:37.

20. See Hitchcock 1970: 76-7.

21. See Williams 1967:38.

22. See Maxwell 1970:477.

23. See Williams 1967:40

24. See Keiser 1970:230.

25. See Whitten 1970:383.

26. See Boissevain 1970:78.

27. See Williams 1967:39.

28. A noteworthy exception is Williams (1967). Articles by Smalley (1960) and Wolff (1960) also provide fairly detailed descriptions of one note taking form.

29. See: Boissevain 1970:83, Crane and Angrosino 1974:11-2, Junker 1960:14, Mead 1956:482-495, McCall and Simmons 1969:73, Nelson 1973:10, Royal Anthropological Association 1951:46, Whyte 1960:365-6.

30. Beals 1970:50, Freilich 1977:159, Gulick 1977:100, Griaule 1957:73, Hilger 1960:vi, Langness 1965:46, McCall and Simmons 1969:74, Norbeck 1970:256, Paul 1953:449, Pelto 1970:266, Royal Anthropological Association 1951:4[illegible], Radley 1979:75.

31. Beattie 1965:42, Beals 1970:50, Crane and Angrosino 1974:11-12, Gulick 1977:99, Griaule 1957:74-6, Hilger 1960:ix, McCall and Simmons 1969:74, Norbeck 1970:256, Paul 1953:449, Pelto and Pelto 1978:71, Royal

Anthropological Association 1951:45, Salamone 1977, Smalley 1960:49, Spradley 1979:75.

32. Beals 1970:50, Beattie 1965:42, Boissevain 1970:79-80, Crane and Angrosino 1974:12, Gullick 1977:99, Langness 1965:46, McCall and Simmons 1969:74-5, Paul 1953:449, Royal Anthropological Association 1951:46, Spradley 1979:76, Whitten 1970:351.

33. Beals 1970:50, Beattie 1965:41, Boissevain 1970:79, Chagnon 1974: 103-4, Crane and Angrosino 1974:11, Dentan 1970:96, Diamond 1970:140, Griaule 1957:73, Gulick 1970:101, Langness 1965:46, Maxwell 1970:477, Norbeck 1970:257, Paul 1953:449, Smalley 1960:148, Whitten 1970:351, Williams 1967:39, Yengoyan 1977:230.

34. Honigmann 1954:91, Pelto and Pelto 1978:70-1.

35. Barth 1966:xi, Chagnon 1974:107, Gulick 1977:101, Junker 1960:16-7, 20-1; Perlman 1970:316, Smalley 1960:147-9.

36. Freilich 1977:159, Hilger 1960:vii, Keiser 1970:230, Smalley 1960: 151.

37. Freilich 1977:167, Griaule 1957:74, Gulick 1977:100, Langness 1965: 46, McCall and Simmons 1969:74, Norbeck 1970:255, Pelto 1970:265-6, Royal Anthropological Association 1951:46, Spradley 1979:75, Sturtevant 1959, Williams 1967:39, Whyte 1960:368.

38. Gulick 1977:100, McCall and Simmons 1969:74, Pelto and Pelto 1978: 70, Spradley 1979:75, Whyte 1960:366-8.

39. Barth 1966:x-xi, Boissevain 1970:80-1, Dentan 1970:96, Diamond 1970: 140, Gonzalez 1977:137, Gulick 1977:100-1, Hilger 1960:vi,ix-x; Hitchcock 1970:176, Honigmann 1954:93, Larson 1964:144, McCall and Simmons 1969:74, Perlman 1970:311-2, Smalley 1960:151, Whitten 1970:351, Yengoyan 1977:232.

40. Boissevain 1970:81-3, Junker 1960:12, McCall and Simmons 1969:76, Pelto 1970:266, Spradley 1979:76, Yengoyan 1977:231-2.

41. Williams 1967:40.

42. Williams 1967:39.

43. Boissevain 1970:79, McCall and Simmons 1969:73, Williams 1967:39, Yengoyan 1977:231.

44. Sources that give some information on this form of note taking include Gulick 1977, Nelson 1973, Robertson 1978, Rose 1965.

45. A source describing this form is Hostetler and Huntington 1970.

46. Sources describing this form are Boissevain 1970, Whyte 1955, 1960, (and in McCall and Simmons 1969:75-6).

47. Sources describing this form include Gulick 1977, McCall and Simmons 1969, Perlman 1970, Sasaki 1960.

48. These include Beattie 1965, Dentan 1970, Hilger 1960, Honigmann 1954, 1970; Mead 1940, 1956; Norbeck 1970, Powdermaker 1966, Smalley 1960, Whitten 1970, Williams 1967.

49. In addition to the four authors cited in this section, very brief accounts by Diamond (1970) and Vanstone (1962) indicate they also used a system of this type.

50. Using a conservative figure of fifteen hours for the average field-work day, this would entail that about 5 hours be spent daily in recording (and in associated activities).

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