TOWARD A METHOD OF INDUSTRIAL ETHNOLOGY

by Frederick C. Gamst

Ethnologists are increasingly turning their attention away from their traditional interests in tribal and agrarian societies (see Gamst 1974:1-10) to industrial urban societies (see Angrosino 1976a; Gamst 1977). However, only a few of our number have turned their inquiry to the industrial (work, occupational) sectors of the latter. The purpose of this article is to provide some guidelines for the methodology, techniques, rationales, and strategy of an ethnological study of industrial organization. In providing these guidelines, I also discuss the nature of ethnological inquiry as a basis for the two concluding sections on the place of the field in the modern industrial world. As working definitions, *ethnology* is considered to be the study of all aspects of existing cultural (and hence social) systems, *technique* to be the procedure for gathering or analyzing ethnographic (descriptive) data, and *methodology* to be the relating of techniques to theory for attaining basic and applied goals of research.

Methodology, including techniques of gathering and analysis of data, has never been a major concern in ethnology, whose students have traditionally learned of these in a haphazard way. As Raoul Naroll and Ronald Cohen explain in the introduction to their monumental work, *A Handbook of Method in Cultural Anthropology* (1973:3):

> Methodology within anthropology has always been something of an underdeveloped area. Somehow, by reading, by association, intuition, and being the "right" kind of person to begin with, a student was traditionally supposed to pick up enough understanding of method to carry out field work or comparative analysis. Whereas in other sciences . . . a great deal of effort both in research reports and in graduate training programs has dealt with methodological matters, very little of this type of activity has ever interested or bothered most anthropologists.

Lack of concern with methodology leads to waste of time and energy in

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the training of ethnologists, a luxury our field, and society, can no longer afford.

THE NATURE OF THE ETHNOLOGICAL STUDY OF INDUSTRY

The subject matter of ethnological study of industry includes much of what may be considered the ethnology of work, of all occupations—industrial and nonindustrial, Western and non-Western. In order to bring sharper focus to the concerns of this article, consideration of industry is limited to the United States and Canada. (This geographic and culture area has the greatest concentration of ethnological studies of industry.) However, the concerns have wider comparative ethnological applicability. In accordance with definitions found in dictionaries and with those used by federal bureaus, such as the Bureau of Labor Statistics, an *industry* is a distinct branch of productive work and habitual employment on a large scale, including its capital and labor. As a concept, an industry represents a group of firms such as factories, stores, government agencies, and transportation companies producing and/or selling goods or services (Schneider 1969:1-9; Wolfbein 1971:29-30). Industries in the United States are customarily classified into a number of *major industry divisions.* The goods-producing industrial divisions are mining, contract construction, manufacturing, and agriculture. The service-producing industrial divisions are transportation and public utilities; trade (buying and selling); finance, insurance, and real estate; other services (including recreation, law, private medicine, private education, and repairs); and government on all levels (providing many of the services found in the public sector and including police, military, administrative, legislative, and judicial personnel).

Another, narrower, definition of industry comprehends only the capital-intensive mechanical and manufacturing branches of productive activity. At times, certain capital-intensive service industries such as transportation and communication are included. Formerly, industrial sociologists and psychologists studied only industry of these "heavy" capital-intensive kinds, but today they investigate industry of all kinds, although certain of these scholars still restrict the word *industry* to its narrower meaning.

Industrial ethnology is the study not only of occupational social organizations and their institutions, but also of relations among these organizations and of their relations to the wider social order. Such endeavor has theoretical and practical ends because it increases our understanding of occupations and their settings and of the effective use of human labor. Industrial ethnology investigates subcultures of industrial work,

including ideal, believed, and actual patterns; physical settings, including places of work and their material arrangements; social organizations of work, including statuses and roles, division of labor, and paths of communication; symbols important to workers; and behavior and attitudes of these people. Industrial ethnology is conducted within our customary holistic frame of reference, in which the entire occupational subculture is investigated, including probing beyond the confines of the location of work, to the home, the community, and the state.

As with surveys of definition of boundaries of industrial/occupational sociology (Roth, Ruzek, and Daniels 1973), no hard and fast boundaries can be fixed for industrial ethnology. Definitions of the two related subfields in both disciplines necessarily issue from the concerns of the practitioner-writers and their case studies and articles, which are seen as having common denominators. What about a study of organizational theory including information on industry, work, or occupation? In such an instance, an arbitrary decision must be made according to the degree to which the concerns of the piece are central enough to those of industrial ethnology. Some publications will be regarded as being industrial ethnology and some as not, according to the orientations of the readers.

The study of industry in other disciplines is distinguishable from industrial study in ethnology in a number of ways. Foremost among these is the use of "a set of assumptions about 'culture' as a master concept in terms of which human behavior is broadly explainable" (Pelto 1970:18). The concept of holistic culture provides an extraorganic setting, a broad meta-theory common to almost all ethnologists (Beals 1978) and most other anthropologists in the biological, archeological, and linguistic fields of our discipline. This meta-theory is in part an implicit frame of reference of order in the sociocultural realm (Pelto 1970:17-20, 64; Gamst and Norbeck 1976:3-6, 18-19). The frame of reference includes "meta-methodologies" (Gamst 1977:3) used in addition to explicit methods.

Ethnologists come close to attaining what is usually considered "a study of *a* culture and its society" only in the case of a culturally homogeneous "tribal" people whose population is relatively small. Here our customary microcosmic community study yields an approximation of the macrocosmic entirety of the ethnic group being investigated. With larger tribal societies, and those on the agrarian and industrial urban levels of development, our microcosmic community study frequently fails to come close to yielding an approximation of the cultural macrocosm. Even with small tribal societies we often find only a study, by a male ethnologist, of aspects of the subculture of males in a specific culture. The female subculture is usually not investigated, in part because of meta-methodological assumptions of the male fieldworker. Holism cannot refer to

the analytic encompassing of all of a specific, entire culture (such as that of the Navajos, Zulus, or Germans) within one study. What it does refer to is investigation of a whole cultural system in its natural setting, be it a hunting band, a horticultural village, or a factory in a city. Holism also refers to a reflecting of this wholeness in the research report. Of course, as Paul Diesing notes, "there are varying conceptions of how whole the system has to be" (1971:141).

Related to our holistic cultural meta-theory is a meta-methodology, our cross-cultural perspective, even when studying our own native cultures. This perspective is in accord with that expressed succinctly by Goethe, *"Wer fremde Sprachen nicht kennt, weiss nichts von seiner eigenen."* The meta-methodological perspective conditions what ethnologists say about our own occupational subcultures. It also limits what they generalize about the nature of work and occupation.

Ethnology's central methodology is formed by joining the cultural meta-theory with our basic technique of research—participant observation. This technique gives us a first-hand contact with our subject matter and subjects, which is not the norm in other social sciences. Various additional explicit techniques and theories are, and should be, added as needed by ethnologists. Ethnology, with its broad theoretical and methodological bases, cannot have a single conceptual focus. Perhaps this is an inevitable and natural state in any *social* science (see Landsberger 1967, but cf. Whyte 1965). In any event, grounded in the eclectic evolution of ethnological method and theory, the industrial subfield is developing a variety of orientations useful for different kinds of problems to be investigated in appropriate ways. The present paper is not an entry into the long-standing contentious debate over whether the participant-observational hammer is a "better" carpenter's tool than the survey-research saw, or the projective-test drill. Instead, the view just presented is of the basic, broad, heuristically valuable utility of the central (but not by any means sole) ethnological methodology. The answer to the argument over which tool is best is answered well by a question: What is the task to be accomplished? As we shall see in the two concluding sections of this article, the hammer of ethnology is sometimes valuable for certain non-academic investigations in industry, but it is rightly viewed by managers as a costly tool.

Ethnological study is ordinarily quite flexible in its applications to problems and to research that is not problem oriented. Its largely qualitative methodological approach produces a vast amount of new insights and descriptive data not usually found in the results of research of more quantitatively directed disciplines. One difficulty with ethnological data has been the "flexibility" of its interpretations by those who attempt to use the information. Social scientists outside of anthropology are

troubled about aspects of our meta-methodology related to our standards for representativeness, reliability, and validity of reported data (see Pelto 1970:19; Pelto and Pelto 1978:23-34). Ethnological methodology is sometimes obtuse and baroque and is often developed in exotic nonreplicable field settings (Gamst 1975a:271, 289).

Ethnologists have long had the scholastically unthreatening distinction among social scientists of usually working among nonindustrial peoples. These are preliterate tribalists and illiterate agrarians (see Gamst 1974:14), who, consequently, cannot criticize or otherwise review ethnological results of research for which they served as subjects. Moreover, criticism of our studies by our own number is often lacking or misdirected. The problem of review exists because many of us have as our subjects of research a little studied or even previously unstudied people. To a large extent, traditional ethnology has developed over the years unhindered by the twin checks of validation by informed scrutiny and of possible replication of the study.

Industrial ethnology *is* so "hindered." Our subjects of research can read our reports and write critiques of them. Our fellow social scientists have long worked the territory, have expert knowledge about aspects of the matters upon which we write, and have ready access to the settings from which we gathered our data. Certainly some, or at times, much, quantification and even statistical analysis is required in industrial ethnology. Precise organization and delimiting of the scope of research is needed. Prejudices and plausible conjectures of the researcher must not merely be illustrated from books in his own personal library (Ember 1978:439), from the dissertations that he is required to read, and from the presentations of on-campus colleagues and to-campus visitors that he chooses to attend. We get by with far less in our traditional kinship (and other) analyses among our own kind than we do among our exotics. Industrial ethnology is forced to measure up in many ways not required of ethnology as a whole.

Ethnological studies of work, industry, and occupational organizations in the United States and Canada have always been required to measure up, but this has not had a discernible effect upon ethnology because of the small number of such studies. These studies go back to the 1930s, but have not become significant in numbers until the 1970s. What makes the recent studies different from those of earlier decades is the contemporary acceptability of the investigations. American ethnology before 1970 was a field where one customarily did research among preindustrial non-Europeans. This is not so today. One can now win his or her ethnological spurs at home with study of nonexotic peoples and problems. In the large and growing subfield of urban ethnology, as practiced in the United States, however, the researchers have tended to

shy away from main-stream middle-America, including working America, and to study what has been called "a bewildering array of interesting tidbits in cities" (Meyer 1974:6; Gamst 1977:1). Of late, the production of main-stream industrial studies of our own society has increased to a modest outpouring, adding to the pioneering classic studies of the 1930s through the 1950s.

Edward Norbeck made a noteworthy contribution to industrial ethnology at the end of the classic phase, but gathering of data began two decades earlier. His *Pineapple Town—Hawaii* (1959) is a study of both a community and an agricultural industry. The research on a Hawaiian pineapple plantation and its interrelated company town affords a vivid emic view of an otherwise unstudied industrial setting, providing cogent analysis of data as well. Data were gathered during five years as a plantation administrator, beginning in 1938, and in subsequent fieldwork in the late 1950s. Incidentally, I am often asked by managers of industry with whom I come in contact whether the average professor would be able to earn a comparable living in the "real" (read "business") world. I never answer the question directly, but I often mention Ed Norbeck, who I know has done very well in both worlds according to the canons of each. The managers are always favorably impressed with my account of this scholar-teacher-administrator.

<center>TECHNIQUES AND RATIONALES
OF INDUSTRIAL ETHNOLOGICAL RESEARCH</center>

Procedures and techniques of collection and analysis of data in fieldwork are customarily learned by the neophyte ethnologist through a process similar to osmosis, with little or no preparation given in these matters. Only recently has our discipline been concerned with formal presentations of and reflection upon research techniques. To such concerns I now turn in the discussion of industrial ethnology.

Participant observation

Ethnologists use a number of methods to collect data. Foremost among these and essential to what can be formulated in ethnological theory is participant observation, which "is central to effective fieldwork" (Pelto 1970:91) and "has been emulated far and wide, even by rivals such as sociologists" (Freilich 1970:viii). Participant observation ranges along a continuum from total identification and participation with the subjects of research ("going native"), through lesser degrees of involvement with subjects, to pure (nonparticipant) observation. Ethnologists rarely come close to the latter pole of the continuum and their ideal is to be as near the former pole as possible, without going native.

Morris Freilich circumscribes the ethnological part of this continuum as ranging from going native to "privileged stranger," with the usual roles of research falling somewhere between the two (1970:2).

Fully participating observation. Within the primary technique of participant observation, I regard *full* participation as its potentially most productive variety. This variety exists on the continuum just short of going native. Fully participating observation is rare, however, in the largely nonethnological social research conducted inside the United States and Canada. It is not even very common in ethnological research outside of industrial societies, where Euro-Americans must ordinarily use less than full participation because of underlying racial and gross cultural differences between them and the people they study. In industrial ethnological study in Anglo-America, however, fully participating observation can be realized in most instances, for example, that of Portland longshoreman William Pilcher (1972), railroad switchman Luis Kemnitzer (1973, 1977), and tuna seinerman Michael Orbach (1977).

A fully or near fully participating observation is necessarily continuous as opposed to periodic (or "nine until five"). Also it is lengthy in duration, normally for a period of a year or more, in order to allow time for effective enculturation of the ethnologist into the culture under study. With the ethnologist's customary immersing of himself into the lives of those he studies, he thereby internalizes large parts of the culture under investigation, be it that of Zulu cultivators or of Yankee lumberjacks. It follows that much of the information gathered by participant observation is not consciously collected, let alone recorded by mechanical means, but is stored on a subliminal level of consciousness of the ethnologist. This information is used along with liminal data in interpreting cultural patterns.

Participation is always a matter of degree of involvement with one's role (Sarbin 1954), even for native carriers of a culture or subculture. For example, an industrial native who is characterizable as a "loner" and is not fully integrated into his occupational setting may be less enculturated into his work role than an ethnologist who has long been an enthusiastic full participant in the occupation. To be an ethnologist of an occupation is to be a long-term, well-enculturated participant in the work role and its industry.

According to the circumstances and to the personality and background of the researcher in American or Canadian industrial ethnology, a somewhat less than full participation may be the maximum attainable. For example, such would be the case if the ethnologist were a member of an African or Asian society. However, the value of the insights derived from a meta-theory provided by a non-Western cognitive orientation would in many cases offset the lesser rapport achieved by an alien inves-

tigating an American industry. Certainly, a need exists for aliens to study American industry and other aspects of our society. It is customary for us to encourage our non-European Ph.D. candidates to study their own people and not to study us. This practice is one more form of the prejudice in American anthropology examined by Francis Hsu (1973).

The role of the worker-researcher. During the investigation of American occupational organizations, an industrial ethnologist becomes a part of the group being studied. He can "hire out" in a particular industry for a year or so and join the appropriate union, various common interest associations, and cliques related to the job, as did carpenter Charles Clinton (1977). He must attain, through on-the-job training, reading, and technical courses of study, a *thorough technical competence* in the occupations being investigated. In order to conduct effective fieldwork, the researcher must exceed the competence normal for a neophyte with equal time in the occupation. By hiring out on a job, one can entirely finance one's field research, an important consideration in these times. No mystique exists in the conducting of this kind of fieldwork; one simply establishes compatible relations with co-workers and relaxes in the industrial setting.

Sociologists invariably note that participant observation may be disguised or open (see Becker and Geer 1957:28), a distinction which has not been a concern of customary ethnology. In the ethnologist's tribal or peasant village no one (including the dogs) really believes that he is one of them, no matter what polite words the local natives may utter to this effect. Thus, entirely disguised research is impossible, even though natives probably do not fully understand the rarefied analytic framework used to convey views of their lifeways to the outside world. Industrial ethnology normally uses open techniques of research, where the researcher's true identity and general purpose are known to the subjects. Of course, openness is a matter of degree, because of imperfections in communication between researcher and subjects. *Fully informed consent* usually cannot be obtained from subjects of research by an industrial ethnologist, because the subjects ordinarily cannot or do not wish to understand all of the proposal of research and the consequences of the investigation. This state of affairs poses one more problem for the ethnological ethics in which we only rarely train our students (see Rynkiewich 1976:58-59).

Size of research unit. In discussions of ethnological research, the question often arises whether a research team rather than an individual fieldworker would be more productive. Reflecting his experience in the study of contemporary American society, Charles Valentine tells us: "Outstanding work in urban slums has been done by a single investi-

gator working alone" (1968:180). In the kind of fieldwork envisioned by Valentine, the ethnologist "should be able to devote as much of his intellectual and emotional energy as possible to his relationship with the people under study" (1968:181). In my own experience, industrial ethnology requires an intense involvement with and commitment to the subjects of study in order to achieve the rapport needed to give a full return on the ethnological investment of time and resources. One researcher, or at times a duo or trio, is optimal. In my experience, be it among Yankee railroaders or Qemant peasants or Wayto hunters, larger teams necessarily bring lesser rapport within and depth of penetration into the system under study.

Pertti and Gretel Pelto correctly assert that in socially diverse settings the advantages of a larger research team *can* outweigh the losses, if the research is properly conducted (Pelto 1970:270; Pelto and Pelto 1978:223-224). However, I would contend that over a period of six months or more a fully participating lone ethnologist frequently has more chance of collecting such data than does a less fully participating team. The problem is again one of which carpenter's tool is most appropriate for the task.

Many areas of information are absolutely closed to outsiders. A good example is provided by William Foote Whyte in the methodological appendix to his *Street Corner Society*. During a visit to a gambling joint, Whyte asked the gambler (1955:303):

> "I suppose the cops were all paid off?"
>
> The gambler's jaw dropped. He glared at me. Then he denied vehemently that any policemen had been paid off and immediately switched the conversation to another subject. . . .
>
> The next day Doc [Whyte's gang-leader friend] explained the lesson of the previous evening. "Go easy on that 'who,' 'what,' 'why,' 'when,' 'where' stuff, Bill. You ask those questions, and people will clam up on you. If people accept you, you can just hang around, and you'll learn the answers in the long run without even having to ask the questions."
>
> I found that this was true. As I sat and listened, I learned the answers to questions that I would not even have had the sense to ask if I had been getting my information solely on an interviewing basis.

Either in the long or short term, the lone ethnologist ordinarily has a far better chance of becoming an accepted insider and confidant than does any member of a group of ethnologists.

With longer investments of time, the lone ethnologist can obtain results having the breadth of data collected by a larger team using a shorter time, and his results will be of greater depth (see Valentine 1968:182). Furthermore, if each of a number of individual industrial ethnologists studies the same industry at only one of its different locations (for example, steel plants at Gary, Pueblo, Provo, Birmingham, and Pittsburgh), they will

achieve multiple perspectives of the same industry while retaining the customary ethnological depth of penetration or keenness of insight. Finally, cross-cultural studies of steel plants in areas such as Japan, Taiwan, India, Germany, and Britain would add still more dimensions (see Goodman and Moore 1972) to comparative ethnological perspectives and analysis of work. Surely, the "best" size of the research team depends upon the nature of the problem to be investigated. Especially in the still-formative years of industrial ethnology, however, pathfinding lone ethnologists are needed to delimit the characteristics of the subculture under study.

Keenness of insight of fully participating observation has been mentioned frequently in this article. This is not an empty phrase but a genuine strength of the technique. "Impression management" and protection of "back region behavior" (Goffman 1959:106-140, 208-237) are barriers to participant observation by an outsider and a serious, perhaps insurmountable, obstacle for the survey researcher armed with interview schedules and questionnaires not necessarily related to the social reality of the group under study. The fully participating industrial ethnologist should be able to abstract patterns of culture from the back region.

Full, or near full, participation adds to social science the penetrating focus of first-hand involvement in the dynamics of interpersonal relations of subjects, and adds greater comprehension and appreciation of their attitudes and behavior. We can assess the validity and completeness of data collected by other techniques (see Becker and Geer 1957:28). An ethnologist can get beyond ideal patterns of culture, the "shoulds" and "oughts" of a particular situation, to the believed patterns, verbalized by the subjects as "what actually happens," and to the behavioral (or so-called real) patterns, which he or she observes and participates in as modal behavior. With this deep probing, the ethnologist is able to determine the degree of congruence between these three kinds of patterning to achieve an integrated depiction of the patterns of a particular subcultural system. An achievement of this kind is beyond the reach of less deeply probing survey research, interviews and questionnaires, collecting ideal and believed traits and patterns and sometimes not collecting certain information. As Beatrice and John Whiting remind us (1973:284-286), the ethnologist often must rely upon participant observation and not upon quicker methods of gathering data in which the subject of research essentially reports his own behavior or that of others. This is because the subject may not be able to describe behavior, because: (1) he and the ethnologist have no common language suitable for the description; (2) no clear, regularized behavior exists; (3) the subject is unaware of certain regularities of behavior; (4) the subject is unable to report behavioral matters of importance with regard to something his immediate subgroup does not do; (5) the subject may be defensive about a

behavior and distort its frequency or context; or (6) the subject draws upon past experience not accurately remembered.

Full participant observation allows emic feedback to and correction of the plan of a research project, thereby yielding results more attuned to sociocultural reality. After first checking the frequency and distribution of phenomena used in describing the organization under study, the industrial ethnologist begins analysis and classification of data during fieldwork. This preliminary assessment allows the fieldworker to redirect research into more significant areas of inquiry. Such redirection may be serendipitously brought about by participation without strict commitment to an etic frame of reference. (The more structured the plan of research, the less likely the finding of data not previously known to the researcher.) The ethnologist then must fill in any holes in the study with empirical data, or, if supporting data are not to be had, must reject a tentative idea. Finally she constructs a model of a social system for the organization being investigated, which accounts for the investigated phenomena.

The use of key informants

Before going on to the subject of survey research, we note that participant observation is supplemented by seeking guidance from and by intensive informal interviewing of sagaciously selected key informants, with whom a close relationship has been established during the course of fieldwork. Informants of this kind are often those workers who are involving themselves in salient activities, encountering new work experiences, and negotiating with management (or, at times, even with unions). Information supplied by the key informant, who is usually more sophisticated and "aware" than the average worker, is not always entirely reliable. The informant may color the responses to questions in order to serve the needs of management, the union, himself, his fellow workers—apart from any union or company involvement—or a certain faction among one of these. He may even try to give his ethnologist friend whatever kind or frequency of information the ethnologist "telegraphs" in conversation as being vital to the success of the research project. The industrial ethnologist must cross check data from his all-important key informants with other informants, through documentary analysis, and, most essential, through participant observation.

Survey research

Regarding the numerous kinds of combinations of participant observation, survey research, and other research techniques used in contemporary social science, Paul Diesing (1971:5) remarks: "The variety of combinations is so great that survey research and participant observation can now be seen as two ends of a continuum rather than as two distinct kinds of

methods'' (see also Becker and Geer 1957:28-29). Quantified results of survey research add materially to the customary qualitative method of ethnology. An ethnologist's quantified results aid others in replication of research, in future study of the group investigated, and in study related to the investigation of a particular occupation. In other words, quantification and use of statistical controls make the research more representative.

The wide (although shallow) coverage of structured interviews or of questionnaires to be completed by hundreds of the industrial ethnologist's co-worker subjects provides a demographic base to the research customarily done in the ethnologist's census, genealogical, and mapping surveys during the beginning of his fieldwork among nonindustrial peoples (see Committee of the R.A.I. of G.B. and I. 1951:47-62). Furthermore, survey research of these kinds yields numerous data on the range of variations of certain aspects of workers' opinions and attitudes. Such data may at times cause the ethnologist to modify preliminary conclusions generated by participant observation. Of course, spurious associations between survey variables may arise if they are not tested against the native cognitive orientation possessed in some measure by the ethnologist.

Survey instruments are used by the industrial ethnologist after he has learned something about the industrial subculture being studied. He attempts to survey and to measure that which will contribute to understanding instead of that which is customarily or easily surveyed. Survey instruments designed and pretested by the ethnologist may be interviews or questionnaires. With interviews, the ethnologist or his assistants personally record the responses of subjects either to a number of informed unstructured questions or to a structured interview schedule. This schedule is a form with questions and spaces for recording subjects' answers and coding these. At times, the schedule is not completed by the fieldworker but is instead sent to the subjects, and it thereby becomes a questionnaire. In this case the subjects fill in the required answers by themselves on the form.

A discussion of specific procedures and strategies of survey research is outside of the scope of this article. A useful brief introduction to the subject with references to relevant literature has been written for ethnologists by the Peltos (1978:77-82, 217-219) and a longer introduction by John Bennett and Gustav Thaiss (1973). Once the ethnologist has some background in design, administration, and analysis of instruments of survey research, Delbert Miller's *Handbook of Research Design and Social Measurement* (1970) should prove helpful for quick reference to guides to research design and sampling, statistical analysis, selection of scales and indexes, and budgeting and reporting of research, all as customarily used by sociologists and others in the study of the contemporary United States.

Ethnologists generally agree that limitations exist in survey research, but not all of them are familiar with specific limits of this technique. As I mentioned previously, the technique usually cannot get beyond the ideal patterns to the believed, and rarely beyond these to the behavioral. Furthermore, it is not at all certain that the technique can always gather data on second-hand events previously experienced by the subject (see Becker and Geer 1957). Survey research is based upon the often dubious assumption that all of its subjects fully comprehend its questions and can adequately express a response to them. A more usual assumption is that the survey sample is representative. Assumptions met still more often in surveys in the industrial West, but not elsewhere, as in village India (Rudolph and Rudolph 1958), are that the individual is a valid unit of opinion and that for the purpose of evaluating the survey all opinions are equal.

Cultural traits and patterned interrelations of these are understandable only in their natural (cultural and physical) contexts. Customary survey research is often so narrowly focused that it is conducted without consideration of the natural settings of the data being gathered. Furthermore, correlations and other statistical manipulations of two or more traits taken out of context may be devoid of meaning. Consequently, the articulation of data derived from survey techniques with extant sociocultural reality in the field is often difficult to find and provides a poor foundation upon which to build either enduring theory or ethnography. This is especially so where flights into statistics are used to avoid dealing with the realities of the natural setting (Bass and Barrett 1972:11-12). What is needed for a more complete interpretation of survey data is, of course, participant observation (Vidich and Shapiro 1955; Becker and Geer 1957).

Survey research without extensive prior participant observation can result in unintentional analytic merging of different phenomena masked by the same labels. Here etic classification results in a social category with an unrecognized number of different kinds of content. Statistical analyses of survey data may yield only a reflected glimmer of an underlying complex of sociocultural reality, which at times consists of a broad range of underlying lifeways. Commenting upon the kinds of significance possible, in this instance when census data show that some of the households surveyed are not headed by observable resident males, Charles Valentine remarks (1968:6):

> it has now long been clear that census figures alone tell us nothing directly about structure or process in a cultural system. To the extent that census data are valid, they give us a statistical shape of a demographic reality. This statistical pattern is a surface phenomenon that may have a wide variety of cultural designs for living underlying it.

Documentary analysis

Analysis of published and unpublished documentary data on the particular occupation being studied is absolutely essential to the success of industrial ethnological research. The specialized knowledge concerning an industry is very complex and is not carried solely in men's heads, as is knowledge in primitive societies. It is in large part contained in libraries and archives, and it involves a long period of specialized training for understanding. Documentary analysis relating to the group under study is nothing new in ethnology, although in industrial research usually far more exists than we are accustomed to in the way of necessary reading.

Documentary (written and other graphic) sources used in industrial ethnology are of two broad kinds. The first kind is published accounts. These may be for general public consumption or intended for more restricted reading, by management, organized labor, employees of a particular group of occupations, or users of goods and services of an industry. Publications of interest to an ethnologist studying a particular industry include agreements between management and labor, company operating rules and policies, government codes of regulation, and reports of management to the stockholders or public. The second kind is archives. Archival sources include correspondence, memoranda, newsletters, reports, transcripts of hearings and proceedings, briefs containing the facts relevant to a particular presentation, business records, and instruments of business transactions such as bills, invoices, receipts, and contracts.

Compared to the "primary" data of participant observation, all publications and some archival materials are secondary sources. Research based solely upon documentary analysis is necessarily biased. The existence of documents of certain kinds or for certain purposes, but not of other kinds or for other purposes, influences and shapes research interests and results. An analysis of sociocultural reality through documents alone is usually not attainable and may result in depiction of ideal and believed cultural patterns. Although incomplete as accounts, documents—because they are often written by natives of an industry—are usually far more emic in nature than are the results of survey research. At times, when allowed access to certain kinds of documents, the ethnologist will come across publicly undisclosed information on the behavioral level.

Documents, then, are incomplete representations of sociocultural reality when used by themselves. But in industrial ethnology we need these representations to round out the accounts derived from our participant observation. As John Madge concludes in his presentation on documentary analysis (1965 [1953]:118-119):

> The rich human material that so many documents contain is a fertile source of
> ideas. Spontaneous personal documents, newspaper reports, committee min-

utes, business or official files—all these provide an invaluable preliminary to direct observation and to participation in social process. They also supplement observation and participation in retrospect, by broadening the base of experience. But by themselves they tell an incomplete story, and it is clearly unwise to stretch their adoption into contexts in which they can offer neither economy nor satisfaction.

Other techniques of data collection

Life histories are colorful and colored (by the subject of research) and are thus not always reliable. In my study of railroaders I find data derived from this technique the least usable. Information concerning the 1910s and 1920s from "old timers" is often factually incorrect or internally contradictory in details. During one particularly vexing interview with an "old rail," I could not help wondering if the old Indians who furnished the data for the "memory culture" ethnologies of aboriginal America had the same qualities of recollection. Life histories have some value as guides to patterns and symbols of behavior and attitudes thought by the subject to be important as ideal, public information, rather than as objective and balanced accounts of past sociocultural reality. L.L. Langness discusses this point and other matters in his introduction to use and limitations of life histories in ethnology (1965). The subject who supplies a life history as well as the subject used as a fieldworker are subtypes of the key informant, and what we have said about this kind of informant applies to the two subtypes.

Subjects as fieldworkers. Certain particularly qualified and motivated subjects may collect and supply valuable information. Such endeavor may take a number of forms: letters containing current events of a time when the ethnologist has left the field, collection of certain kinds of data taken from participation in an event of interest to the ethnologist, collection of documentary sources (or even taking photographs) for the ethnologist.

Case methods have long been used to aid ethnologists in their studies of legal proceedings (Epstein 1967) and other particular aspects of culture (Gluckman 1967) about which a collection of the circumstances of a large number of events is recorded. This collection of occurrences of related cases is used as a basis for generalizing on the behavior, attitudes, and relationships occurring in a matter, such as legal proceedings. The industrial ethnologist can profitably use the case method in the study of hearings and investigations by management or government concerning accidents and alleged breaches of operating or regulatory rules, and in the study of union-management negotiations on contracts, grievances, and other matters.

Projective tests of personality, in which subjects are asked to react in some way—usually verbal—to something presented by the researcher, were first developed by clinical psychologists but have long been used by ethnologists during the course of their fieldwork, usually in limited and rather

simplified forms. Regarding the use in modern industry of two such tests, which are the most commonly used in ethnology (Pelto 1970:111), Bass and Barrett say: "Unfortunately, results from the ink blot test when applied to the industrial situation have been uniformly negative," and: "this is not too surprising since the *Rorschach* was developed as a clinical instrument and was not designed to differentiate among essentially normal adults" (1972:290). This fact has not always been appreciated by ethnologists. Regarding the *Thematic Apperception Test (TAT)* they say, "The *TAT* has been an unpromising technique for industrial selection with two exceptions" (1972:289). Lack of success in industrial settings with the two favored ethnological tests of personality poses no great problem, however. Psychologists have developed large numbers of sophisticated tests of personality, interests, aptitude, proficiency, and simulated on-the-job "situations" related to occupation and work in industrial society (Bass and Barrett 1972:265-279, 287-291, 306-309, 567-572). These tests give the industrial ethnologist who is so inclined a wide range of instruments for the study of personality in forms much more sophisticated than he is accustomed to in his non-Western fieldwork. Again, the context of and guidance for the proper use of psychological tests in industry are derived from participant observation.

Mapping and diagramming have always been used in ethnology to depict physical settings and social relations, by means of community maps, kinship diagrams, and so forth. Maps, flow charts, and plans and elevations of work settings are important mediums for recording and presenting social data in industrial ethnology. These are also important in assessing the manner in which the physical setting of work limits and conditions human behavior (see Melbin 1960: 255). Mapping and diagramming may be useful in depicting the "fit" between physical and social space, as in Norman Green's study (1956) of the urban settlement patterns in an American city, where the wealthy ostentatiously use space and the poor are overcrowded. The two closely related techniques may be used to show changes in physical plant or in social units and their interrelations over a period of time.

Rankings of statuses of individuals in an industry can be conducted in structured interviews or as part of a questionnaire, after pretesting and after sufficient background is acquired by the ethnologist through participant observation. The ethnologist can devise his own framework for rankings or can build upon research of this kind commonly used as a standard of reference in the social science of industrial society (for examples, see Miller 1970:169-199).

Visual ethnology is the recording of ethnographic data, with cameras, in the form of photographs, slides, motion pictures, and video tapes. John Collier's *Visual Anthropology: Photography as a Research Method* (1967) is a noteworthy introduction to the subject. Processes,

procedures, and organization of work in many industries are so complex that they must be captured on film to be fully and enduringly recorded in the field notes of the ethnologist. During analysis of data, a recall of the field experience through photography can be repeatedly conjured up with the ethnologist's projection equipment. For example, I have found that viewing of slides on procedures and the highly varied physical settings of railroad work aids immeasurably in the analysis of written and tape recorded field notes.

Natural and field experiments are conducted in man's natural settings rather than in laboratories, where events and their settings are created by the researcher (Katz 1953:78-170; Freilich 1963; Pelto and Pelto 1978:272-274). The industrial ethnologist will ordinarily have little opportunity to use a field experiment in which the researcher manipulates an independent variable in a setting outside of the laboratory in order to test a hypothesis (French 1953). Field experiments in industry were the basis of the classic studies at Hawthorne, but are rare in ethnology (Lewis 1953), having been used in only a handful of studies in rural non-Western settings (Holmberg 1958). The ethnologist may have more opportunity to use a natural experiment, and many may wish to do so. A natural experiment is an important change brought about by policy makers and technicians and not by social scientists. The change is experimental from the viewpoint of the social scientist because it provides opportunities for measuring the effect of the change. This form of experiment is based "on the assumption that the change is so clear and drastic in nature that there is no question of identifying it as the independent variable" (Katz 1953:78).

Requital to the subjects of a study relates to what social scientists frequently mention as "feedback" from the role of researcher into the social system being studied. One kind of feedback which should be a part of all ethnology is a "pay back" or requital to the subjects of research. While and after "mining" his subject-co-workers for data and ideas, the industrial ethnologist must reciprocate their time, effort, interest, and friendship. Reciprocity cannot, of course, be in the form of overtly biased reporting of results of research. It can be said, however, that one's co-workers will receive some form of return from a balanced report, which will counteract any "folk tales" and untruths about their occupation and generally educate the public on the reality of their work and its setting. At least one report, readable by the average man and preferably in the format of an inexpensive paperback monograph, should result from each major research project in industrial ethnology. Reciprocity can also be in the form of time and effort spent in correspondence and chatting with the informants, many of whom derive some benefit from using their social scientist-co-worker as a "sounding

board" for their thoughts and problems and for release of tensions.

APPLIED INDUSTRIAL ETHNOLOGY

Increasingly, the cry is raised regarding the pressing need and necessarily high priority of ethnologists "involving" themselves with the study of contemporary problems of communities and groups in the United States and Canada. Some ethnologists resist pressures towards involvement of this kind because of their ideal attitudinal patterns concerning the role of academic anthropologists as "neutral, objective, and detached" from political process and social concern. Despite a general sentiment (expressed, for example, in the "Statement on Problems of Anthropological Research and Ethics," adopted by the Fellows of the American Anthropological Association in March 1967) that anthropology should be of benefit to mankind, little is attempted by most to put this ideal sentiment into action. As Ralph Beals and Harry Hoijer comment (1971:669):

> Most anthropologists have felt their contribution was best made through basic research to develop understanding of human behavior in the context of culture and society. A good many anthropologists felt uncomfortable about the beginnings of applied anthropology or any direct connection between anthropology and public policy.

It is curious that cross-culturally oriented students of holistic culture could believe that their discipline and their efforts exist apart from the politics of the greater system of which they are a part. As industrial ethnology grows, it will increasingly enter into public and private policy because of its very existence. The desires of anthropologists have little to do with this matter. An applied, advocacy ethnology has even more problems than the customary applied ethnology (see Peterson 1974).

If we realize that applied and basic ethnology are inseparable (see Foster 1969: 39-71), what do we do about overt attempts by funding agencies to turn a part of industrial ethnology toward applied goals? Ethnologists frequently contend that they should have a "freedom of investigation" to study whatever they choose in whatever way they choose. If too much of industrial ethnology is tied up with applied policies and funding, what happens to our discipline? One answer to this question is another question: Where would medical and public health research be today if these disciplines were not in large part oriented to overtly applied research and to basic research with eventual application in mind?

Can we in ethnology claim that our functional and our structural concerns with the details of our Crow and Omaha kinship constructs are more likely to produce enduring contributions to social scientific theory than are

research concerns with avowedly applied bents (see Pelto 1970:321-322)? Because "the difference between pure and applied anthropology lies neither in research nor in the researchers" (Foster 1969:45), overtly applied ethnology, whose end is in aid of the solution of a problem of contemporary society, is not necessarily devoid of pure or basic results.

The industry of higher education, and its social scientific components such as ethnology, exist in a greater social system and not in a vacuum. Some university presidents have admonished against involvement of the academy in the social problems of the outside community. But, just as physical and organic scientists have concerned themselves with application of their work to the greater system (for example, the development of nuclear weapons, the "race" into outer space, the defeat of polio, and the attack upon cancer), so too must social scientists significantly concern themselves with applications of their work. Admittedly this application would be to more complex problems than those facing physical and biological scientists. (Some of these problems, such as eradication of economic poverty, social discrimination, legal injustice, and even much of biological pathology, may well be unachievable short of a partial re-ordering of our social system. If this is true, the implications of this thought for applied social science are, to say the least, overwhelming.) In any event, a task of industrial ethnology must be contributions to the solutions of these social problems. Kenneth Clark expands upon this thought (1965:xxi):

> I believe that to be taken seriously, to be viable, and to be relevant social science must dare to study the real problems of men and society, must use the real community, the market place, the arena of politics and power as its laboratories and must confront and seek to understand the dynamics of social action and social change.

APPLIED INDUSTRIAL ETHNOLOGY AND "QUICK, DIRTY" FIELDWORK

Application of ethnology in the industrial setting is fraught with difficulties involving time and money, both so important in our capitalistic economic system. (Incidentally, as has been shown elsewhere [Gamst 1975b:37-39; Angrosino 1976a], so-called "applied anthropologists" ordinarily apply only ethnology and not any of the other three fields which together with ethnology constitute American anthropology. Anthropology *per se* is rarely applied.) One difficulty of time and money, as Michael Angrosino explains, is that the "new" applied ethnology is financially supported by short-term contract and often involves evaluation of public and private programs (1976b:1,7). Few public or private agencies can afford the time or the money for an ethnologist to do the cus-

tomary year of participant observation before beginning to reduce the field data for a solution to the immediate problem. Accordingly, Angrosino informs us, "the new applied anthropology, in the agency setting, must perforce be 'quick-and-dirty' survey work, the sort of thing anthropologists have sneered at sociologists and social psychologists for indulging in" (1976b:7).

It is undoubtedly true in most instances that, from the agency viewpoint, by the time the ethnologist gets into high gear, the policy problem of the agency could be of more historic than immediate interest. The truth is that doing ethnology, as reviewed in this article, is not usually what an agency will want to employ us to do. This truth raises a question concerning who we are and where we are going as ethnologists. With the loss of long-term, intensive participant observation, to what extent does the "new" applied ethnology remain ethnology?

Regarding present trends in applied ethnology, Walter Goldschmidt correctly admonishes us that if ethnology is to be useful it must retain its essential characteristic, developed in our traditional fieldwork, including first-hand intimate contact with subjects of research and holistic examination of their social life (1977:294-295, 301-302). According to Goldschmidt: "The anthropologist who loses touch with the subjects of his investigation ceases to be an anthropologist" (1977:302). He says that as we currently leave tribal and agrarian research and enter industrial urban research, we lose our "crucial holistic perspective" and we "ape" the "methodologies of other disciplines" (1977:302). Ethnology could become "lost in a cloud of particulars" and its utility could be only that of a weak copy of highly specialized disciplines, he notes. Burleigh Gardner also warns against trading the generalist strength of ethnology for increasing "tunnel vision" specializations (1977:172).

Related to the discussions of Goldschmidt and Gardner, Robert Wulff has considered a similar question: Must we abandon ethnology in order to function outside of our traditional university scholarship? (1976:34). Generally, Wulff values the strengths of traditional ethnological fieldwork, those reviewed in this article (cf. Wulff 1976:36, 44). But he suggests some realistic, practical changes within the canons of ethnology. These changes include giving up orientations in our field of extreme cultural relativism and of the ideal of a value-free social science, adding techniques and methods as appropriate to our extant ethnological toolbox, and devising of new advice formats—specifically attuned technical reports rather than journal articles and monographs (1976:40-42). However, Wulff also argues that "we must streamline, augment, and rethink our traditional ethnographic methods" (1976:42). He says he does not advocate "sloppy" design of research or the generation of inaccurate data. What he calls for is a "more realistic standard of quality for

applied urban research" (1976:43). Even a "six-month fieldwork project consumes far too much time and money" (1976:42), he affirms.

Clearly, we have a paradox. How do we remain ethnologists and yet conduct quick investigations which are not too dirty, that is, which reasonably accord with the canons of ethnology? Agreement on need for holism, first-hand contact, and non-sloppy design in our study of industrial urban natives does not shed much light on *the* fundamental methodological problem: duration of fieldwork. How little participant observation can we get by with and still remain bona fide ethnologists? Goldschmidt and Wulff do not specify. Gardner, with his considerable experience in industrial study, believes we *can* remain broadly focused ethnologists and "produce results in a month or two that in academic research would span a year or more" (1977:173). Perhaps such a short duration would satisfy Wulff and would be agreeable to Angrosino, who advocates fast "survey work." Drastic cutting of the duration of our fieldwork is something which I think cannot be done if ethnology is to remain the ethnology of the past eight decades, in accordance with the outlines presented in the previous sections of this article.

Certainly, nothing is sacrosanct about our normative full year of initial fieldwork among tribalists and agrarians. Normally, twelve months gives us sufficient time to understand our alien subjects and, quite importantly, time to see the effects of the seasonal geographic cycle upon a society with a low-energy technology not able to shield its members from the seasonal vicissitudes, as does our own high-energy technology. Seven or eight months of continuous industrial ethnological fieldwork should do as well, especially for our own society where we understand not only the basic grammar of language, but of human relations also. What about four months? Probably it is sufficient in the United States and Anglo Canada, for an Anglo. Generally, the only justification for an industrial ethnologist conducting fieldwork for a period shorter than three to four months of highly intensive study would be that the research is in a particular industry previously investigated by the researcher. Previous study would include the intensive pre-professional experience of W.F. Cottrell in railroading (1940). With her previous year's study of a china dish factory, Patricia Tway (1977a) might be justified in doing shorter ethnological research in another such factory across the continent.

Holism and first-hand contact with subjects of research leading to real as well as ideal cultural patterns, and providing back-state views of social reality, are gained only with a large investment of time and energy. Without a moderately long-term and continuous participant observation used as a basis for our employment of supplementary informal surveys, questionnaires, projective tests, and other techniques, we have no ethnology, but, instead, merely a shadow sociology, political science, or

social psychology. In such an instance, holism is not achieved, and first-hand contact with subjects of research is more token than effective. To the extent that ethnologists do "quick and dirty" research in industry, it is proportionally not ethnology.

INDUSTRIAL ETHNOLOGY IN THE CONTEMPORARY WORLD

Writing in 1947, John Sirjamaki noted that the theory and techniques of ethnological research had developed to the point where the methods used in the study of primitive societies could be used to study modern American culture. He also remarked, "The American way of life . . . has remained, curiously enough, beyond the direct scrutiny of most social scientists, and the ironical fact is that more is actually understood about the culture of the Trobriand Islanders, for example, than of Americans" (1947:253). This situation has not changed appreciably during the past three decades. In contrast, industrial ethnology will necessarily always be in large part a study of the American and Canadian ways of life. Middle American social relations, values, and problems, then, constitute not only a legitimate focus of study, but a long-neglected one upon which to test the keenness of our methodological tools.

Many studies in industrial ethnology will be those of some part of the vast "Middle America," to be added to those of the customarily studied America of the more powerless, lower class, "minority" groups, and hyphenated Americans, who are the focus of research in much of urban anthropology conducted in the United States. Because in actuality most Negroes are in the so-called "working class," we could profitably conduct an industrial ethnology of blacks in their occupational roles and settings. Such study would facilitate understanding of needs and aspirations of the more typical Yankee of this racial type than would the exotics, "deviants," and welfare cases usually sought out by middle-class white social scientists in their research (see Liazos 1972).

As shown by the controversial studies of Goldschmidt (1947) and Miner (1949) and as with other ethnology of the West—for example, James Spradley's investigation (1971) of *Seattle* drunks both in and out of jail—the political influence of industrial ethnology does not lie as much in its contributions to sociocultural theory as in its penetrating probing of social reality beyond the ideal patterns and in its reporting from the "native" viewpoint. The potential for the shaping of public policy derived from industrial, and other, ethnology of contemporary United States and Canada is great, even when such research is nominally value-free, neutral, or without bias. Industrial ethnology will be increasingly relied upon to furnish what Gerald Britan has explained as the con-

textual model of evaluation research for public programs (1978). Contextual evaluation can provide large amounts of ethnographic data on program activities and initiatives (and the lack of these) and can develop conclusions about organizational and environmental conditions influencing programmatic action and development (Britan 1978:12; see also Goldschmidt 1977:302; Foster 1969:57-58). Although not without difficulty in its application, industrial ethnology has a considerable potential in public policy and public and private evaluation research (see Chambers 1977; Britan 1978; Whitehead 1978). The potential is not for shadow sociology, but for solid ethnology. Any program head who wants sociology will hire a real (professional) sociologist. Those few agency heads who are not entirely satisfied with sociological and economic analysis (remember the uses of the saw and the hammer) might hire a real ethnologist, at a cost in time and money.

Since I am frequently asked about it, a word or two should be said about non-academic employment of ethnologists of industry in contemporary United States and Canada. Some giants of our subfield have been so employed. Eliot Chapple, Burleigh Gardner, and Lloyd Warner come immediately to mind. Of late, a few handfuls of the two thousand or more ethnologists in the two countries have found non-academic/museum employment. The realities of non-academic employment for ethnologists ill-trained for such an endeavor have been discussed (Angrosino 1976b:1; Wulff 1976:41; and, importantly, Tway 1977b). Although non-academic industrial ethnologists must be computer-literate, able to understand and conduct survey research, and at ease with statistical analysis, their skills in these areas should not be used to make their research "quick and dirty." Industrial and government agencies have market-generated demands, largely for "quick and dirty" studies that will deflect the ethnologist from the ethnology advocated by Pelto, Goldschmidt, myself, and many of us in the field. Because of the demands for this kind of quick study, it could well be that *most* applied ethnologists working for industry or government will not be so employed full time. Rather, they will continue to consult during summers, a day or so per week during the semester, and when on consultation leave of absence, but usually from the secure base of their academic positions. To the extent of time that industrial ethnologists are non-academic, it could be many will cease to be ethnologists but instead become just evaluative administrators.

In her cogent review of the problems of off-campus employment for ethnologists, Tway spotlights an important difficulty when she notes that: "The ability to make important decisions rapidly and often is a necessary requirement for non-academic employment, yet it is never (to our knowledge) taught to graduate students, nor required of university

professors" (1977b:158). I generally agree with this statement. Graduate schools fail to train future teacher-scholar-administrators as teachers, or as administrators. Being somewhat insulated from the market, academic administrators do not have to be either good or reasonably quick at making decisions. But the need of industry for quick, accurate thinking and timely rendering of decisions does not excuse "quick, dirty" fieldwork. Ethnology, as defined in this article, must be conducted with some regard for, but not necessarily in compliance with, the needs of the non-academic world. And an ethnologist should be able to think more quickly and organize tasks more effectively than academics customarily do. However, ethnologists should never forget that their usefulness to industry and government lies in their skills and insights as practitioners of ethnology, and not in being a shadow-practioner of another social science.

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