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The task of obtaining useful information as to unemployment and related problems in urban slum districts presents both unusual opportunities and unusual difficulties. Earle Gerson and Howard Stambler, in their papers dealing with problems of interviewing, methodology, and analysis of the Urban Employment Survey of the United States Department of Labor and the Census Bureau, have chosen to stress the difficulties; I would like to take the opportunity, in my capacity as a discussant, to point to the not inconsiderable merits of this enterprise. It has taken a good deal of courage and imagination to depart from the well-established and relatively comfortable traditions of Census Bureau data-gathering methods in order to tackle an extremely important area of information. It is my feeling that the Urban Employment Survey breaks new ground in the field of government-sponsored information gathering, and that the difficulties of the enterprise are in direct proportion to its significance. Marquis' paper shows a similarly imaginative approach to some very fundamental methodological problems of the survey-research method.

Because I so applaud the spirit of this enterprise as well as its execution to date, I am most reluctant to be critical of it. However, in order to conform to the traditional role of the cultural anthropologist, and particularly, in this case, a cultural anthropologist with very limited statistical sophistication, I feel obliged to indicate certain limited aspects of these studies which appear to me to have received insufficient systematic attention, and where the application of somewhat more attention in the future might result in significant improvements in the character and usefulness of the research findings.

Of the two classic statistical issues of "reliability" and "validity", it is my feeling that both Gerson and Stambler show relatively more concern with the former. The Marquis paper does go to the problem of validity, but on a rather restricted level. I would like to address briefly and in more general terms, some problems of validity as they apply in studies of this kind.

Marquis, in referring to the results of his Health Statistics surveys and related studies, cites this conclusion: "...the major sources of influence on the accuracy or completeness of data obtained in the interviews are to be found in the actual behavior of the participants and in other parameters which have an effect on the immediate situation." I would like to dispute this conclusion, and suggest that considerably more important sources of influence on the accuracy of information relate to certain larger subcultural

systems which figure directly in this kind of data-gathering enterprise. Two of these are of particular relevance here; the subculture of the respondent populations, and the sub-cultural context of the survey-research method in general.

I will use the term "subculture" to refer to those sets of conceptions, perceptions, and definitions of appropriate practice maintained by designated categories of persons, such as males, adolescents, and urbanites, by virtue of their affiliation with that category. Of particular relevance here are subcultures associated with different social status levels. I will refer, in a very gross fashion, to "middle class", "working class", and "lower class" subcultures, although a good deal of additional refinement with respect to differentiation of levels is obviously possible, and utilized elsewhere.

Survey-research methods in general, including the particular subtypes of that method employed in the federal census, were developed primarily in connection with middle-class and/or working class populations. The philosophy behind the method as well as its methodological feasibility rests on the presumed existence of a large set of conventional attitudes and customary behavioral practices characteristic of such populations. Among these are relatively stable residence, certain types and levels of communication patterns, a motivation to provide accurate answers, a sense of obligation to aid the purposes of public data-gathering enterprises, and many others. Populations at the lowest social status levels--and in particular the urban low-skilled laboring class--maintain an equally conventional set of life patterns which differ in important respects, from those of the middle and/or working class. Insofar as there are differences between higher and lower status populations which affect the probability of obtaining valid information through survey research methods, it would appear that this probability decreases as one moves down the social scale. In many cases these differences are not radical ones, but they are systematic, not random, and of sufficient magnitude as to have systematic effects on results.

The research in urban employment now under way provides an excellent opportunity to explore some of the effects of differential social status on the validity of survey results--an opportunity which has not been sufficiently exploited to date. For example, it is most significant that Marquis makes his primary analytic distinctions on the basis of age and race, and neglects social status. I would suspect that the use of a sufficiently refined method of discriminating social-status levels (one based, for example, on years of education, an item in the present questionnaire) would show substantially better discriminations

than race. I say the opportunity is an excellent one primarily because of one of the major empirical findings of the employment studies to date; namely, the unexpectedly high degree of social-status heterogeneity in the Concentrated Employment Program areas. It would not be too difficult, on the basis of data now available, to distinguish three, four, or more status levels within the lower class populations of these communities, and use these distinctions as a major dimension in the analysis of variation.

I would like to note briefly some influences of social-status subcultures on the validity of questionnaire and/or interview responses with respect to two orders of information: the first, essentially "factual" information; and the second, attitudinal. Stambler's formulation reflects this kind of distinction in his citation of "objective and factual" versus "subjective" data (a further issue, the degree of "objectivity" of the most "objective" information, cannot be treated here). Subcultural factors affect both orders of information. One can distinguish two kinds of influence; the first might be called "inadvertent misrepresentation" and the second "motivated misrepresentation". Included under the first are familiar factors such as faculty memory (phrased more impressively as "inaccurate informational retrieval"), and cognitive misunderstanding of questions (Marquis' paper treats this subject), based either on inadequate communication or a "true" incapacity to grasp the intent of the question, or both.

It is also possible to distinguish two kinds of "motivated misrepresentation"--"conscious" and "unconscious". Conscious motives may involve the desire to "stay out of trouble", involving, for example, information concerning illegal sources of income (numbers, prostitution, theft) or unreported income from a job held by public welfare recipients. Less conscious motives may entail misrepresentation in an effort to create a "good impression" in the eyes of the interviewer--representing one's practices, by subtle shading or more gross distortion, in such a way as to conform to the respondent's conception of what the interviewer's conception of "proper" or acceptable forms of behavior might be. The influences of more- versus less-conscious bases of misrepresentation are difficult to separate out for particular responses, but may be distinguished on a conceptual level.

One way of approaching these very difficult problems respecting the validity of information collected among low-status populations would be to select a limited number of subsamples of the Employment Survey study populations, and subject these to highly intensive research--based not on the standard survey approach of one, two, or three interview contacts of one-half to one hour each, but rather on the

long-term, continuing, direct-contact methods developed in anthropological field study, and often referred to as the "participant observation" approach. Such research would center on exactly the same informational areas as those covered in the Employment Survey questionnaire, but obtain this information through very different means. If one could arrange a coverage of even 5 or 10% of the total Employment survey sample by this method, much of the uncertainty as to the validity (although not necessarily the reliability) of the questionnaire responses could be reduced.

I cannot go into detail as to the specifics of this method, which has both assets and liabilities for present purposes, other than to say that it has been developed in some detail, and has shown excellent results under circumstances where survey methods would be quite inappropriate--for example, the study of urban adolescent corner gangs--many of whose most important activities involve illegal practices which probably would not be reported to an unfamiliar interviewer, or if reported, reported inaccurately. Such an enterprise is in fact currently being planned under the auspices of the Labor Department, and specifics might perhaps be reported at some future session. One of the major features of this method is the opportunity it affords to record relatively independent bodies of data with respect to two orders of information: "expressed sentiments" (statements involving attitudes, intentions, and/or verbal characterizations of current situations) and "observed practice" (what individuals are actually observed to do with respect to areas to which their verbal statements refer). An informant might tell an interviewer, for example, that he is "actively seeking" employment, whereas continued observation of his behavior over three or four months might reveal his actual search to be quite casual or even non-existent.

Since the papers by both Stambler and Gerson have stressed methodological deficiencies in data collected in the present studies, and I have indicated still others, I would like to conclude by posing, but in no sense resolving, what can be seen as a fundamental issue with respect to the informational product of such studies. In areas of high policy relevance where very little reliable data are available, are the interests of effective policy formulation better served by the availability of information incorporating known and systematic sources of bias, or by continuing to base policy on those more impressionistic and less systematic forms of information which are available? I am by no means convinced that the former alternative is the preferable one, but see this question as one which each concerned individual will resolve according to his own values and special interests.