

DISCUSSION

Norman M. Bradburn, The University of Chicago

Statistics has been called a guide to the unknown. Contemplating these excellent papers, we might think that at least sometimes it is a guide to the unknowable. Certainly, the topics that have been addressed in this session are among the most challenging in the application of survey methods to the study of human populations.

There are three themes that, in different combinations, run through these papers. These are: first, methods to sample rare populations; second, methods related to sensitive measurement issues; and third, methods to sample populations in which individuals cannot be uniquely identified with a geographically-located household. Not all papers are concerned with all three issues but most deal with more than one.

In studying topics that come as close to being unknowable as the ones addressed in these papers, it is unlikely that any single survey will even approach being considered definitive. In these cases, to a greater extent than in studies on more common topics where there is less controversy about the size of the population of interest, consensus about population size estimates will only come from the convergence of a number of studies using different methods rather than from any single study or even coordinated group of studies. The implication of this fact is that there will be many investigators in many different places addressing the same topic using the same, similar, or widely divergent methods. It is of the utmost importance in these cases that the definition of membership in the population be identical across the different studies, that is to say, it is vital that different attempts to estimate the size of a particular population at least agree on the definition of the population.

If I have any general disappointment in the papers, it is that they give relatively little attention to this problem of definition. While all of the authors discuss to some extent the problems of population definition, they do not discuss in any detail the alternative ways to define the population and what implications alternative definitions might have for measurement problems.

For example, there is an important time dimension regarding homelessness which is rarely addressed. Is homelessness like poverty in that people move in and out of the state very often, or is it more like having a chronic disease where individuals persist in being homeless for long periods of time? While both studies that we have heard today agree that spending a night in a shelter is a sufficient criterion for being homeless (presumably even if it's just one night), and they mention that

screening questions must be developed if one is measuring the homeless in other settings, they do not elaborate on what those criteria might be. It is important if one is going to compare across cities or across studies that the screening criteria be the same.

There appear to be at least two types of nomads--permanent nomads and semi-nomads who spend some portion of the year in settled agriculture. Whether or not this distinction needs to be kept separate in measuring the nomadic population of a country depends on whether there is an interaction between the season of the year and nomadic status. If all of the semi-nomads are nomadic during the same time of year, then there will be great seasonal variations in the number of nomads. If, on the other hand, the semi-nomadic peoples settle down at different times of the year, there may be little or no variation in the overall number of nomads at different times of the year. This clearly is something that will have to be taken into account when one is doing surveys of nomads and, particularly, if one is going to compare across studies.

A somewhat similar problem exists for missing children. How long does a child have to be missing before it is considered missing? Is it the same amount of time for children of different ages? My impression is that officials have a somewhat graduated view about the length of time a child must be missing before it will officially regard it as missing. The older the child the longer the period of time before it would be officially viewed as missing. Sudman notes that there is probably an interaction between the length of time a child is missing and the probability of that fact being known to different members of the family or neighborhood. Thus, this differential may affect the accuracy of reporting with multiplicity samples. It is important, however, in comparing across different studies to make sure that the researchers are using the same base definitions for what qualifies as missing.

An analogous, but perhaps more complicated problem, exists regarding illegal aliens and their employment status. If a large portion of illegal aliens are apprehended at the border and returned virtually immediately, they would have no opportunity to enter the labor force. In estimating the number of illegal aliens, one might well want to make some divisions regarding how long they had been in the country, particularly if one is interested in their impact on employment.

I think it can be seen from these examples that a time dimension is an important part of the definition of population membership and that we need to work toward some standard definition in order to get the maximum benefit out of successive studies.

Three of the papers deal with problems of estimating populations that cannot be attached to any fixed address, a particular challenge to probability sampling methods in human populations because sampling frames are usually based on the assumption that every individual can be uniquely attached to some geographically-located household. While the problems of counting the homeless in American urban centers might appear to be quite different from counting the nomadic population of Somalia, there are, in fact, considerable methodological similarities in the approaches taken in these papers. They all work from the assumption that there is some place where all or most all of the population of interest will come at some time or other during a fairly short period of time--in the case of the nomads, a water source; in the case of the urban homeless, a shelter where food and protection from weather is provided. The adequacy of relying on these points is, of course, dependent on several factors. One factor is how necessary it is for people to flow through the centers and, second, how complete an enumeration of the centers does one have from which to draw a sample. In the case of the oases that provide water for the nomads and their animals, there seems to be considerable necessity for nomadic peoples to visit water sources periodically, the period being determined by their own needs and those of their animals. A major problem here might be the adequacy of the known universe of such watering places from which a sample might be drawn. Kalsbeek's cost model is impressive but, as he notes, the model might produce results which are impossible or impracticable to carry out. While not denying the utility of such models, it seems likely to me that in cases of extreme field difficulty, practical field problems are more likely to dictate what can and can't be done pretty much independent of what the cost models might say. Designs which call for operationally simpler field procedures are almost certain to be preferable in difficult field conditions, if nothing else because of the lower risk of failures in carrying out the design.

The question of what proportion of the homeless in American urban areas find their way into shelters is, of course, one of the major problems at issue in counting the homeless. The unknown proportion of homeless who live on the streets and never set foot in a shelter is one of the issues that gives rise to such widely different estimates about the size of this population. The two papers that we've heard today take somewhat divergent approaches to handling this problem. The Baltimore study described by Cowan relies primarily on shelter records and a capture/recapture methodology to estimate the

number of people who are missed by counting only those in shelters. The Chicago study described by Frankel combines direct enumeration in shelters with an area field sweep to pick up the homeless living outside of shelters. This latter approach is not only expensive but requires a field virtuosity that is truly impressive. Of course, it would be fascinating to see both methods applied in the same city to see if there is a convergence of estimates stemming from the two different methods. It wasn't clear to me from Frankel's paper exactly how the people in the shelters were handled during the twelve sampling periods. Were different shelters sampled on different nights, or were different individuals sampled within the same shelters on different nights? If so, how was the capture/recapture problem handled?

Capture/recapture methods with human populations have a number of practical problems because matching, even with fairly good records, is often difficult. Cowan alludes to a number of these problems with regard to the Baltimore study. It is an empirical question whether in a particular instance the matching problems may not be such a serious source of error that the method does not yield the extra information that one's depending on it for. The nomad study design, in which households are counted at an oasis only on the day of their arrival, might be seen as a capture/recapture study in which the recaptures are rejected. There were two aspects of that design that were not clear to me. The first was: are people counted who are already at an oasis and have been there for several days as of day one, or are only those people who actually arrive on day one counted? Second, what does one do about families that leave and come back within the twelve-day period? Are they counted twice or only the first time they arrive? There is, of course, always the practical problem of whether one has enough information about families to know whether this is their first day or whether they have been there before.

Capture/recapture problems are also present in multiplicity sampling as described in the Sudman paper. If the incidence of missing children is somewhat clustered either by geographical area or family, the matching problem might be quite severe as one interviews further out in the networks. If reports are spread over some considerable length of time, the rate of mismatches or partial matching might also increase. I would imagine a fair amount of work would have to be done to try to find the optimal time periods and network size to balance off higher incidence with more accurate information.

The Chiswick, the Sudman and the Cowan papers all describe an imaginative use of administrative records, a very useful tool

in the study of rare populations, particularly on sensitive topics. A blending of a sampling frame from records and a probability sample of the population gets around a number of the problems that one would have working from records only, particularly in providing for the possibility of picking up cases that might not be in the records. Of course, dual-frame sampling of this sort has its own problems requiring complex calculations of selection probabilities. The methods are sufficiently cost effective to be worth the extra troubles in analysis. I hope that these methods will be widely enough publicized so that they will be used more frequently in this type of work. The Chiswick paper is a particularly good example of the use of administrative records to learn a lot about a topic which people believe is virtually unknowable. There is,

of course, the problem of the generality of the findings. How do those that are apprehended differ from those who are not apprehended? If there are important interactions between the probability of being apprehended and the characteristics of interest such as employment level, industry, etc., then the results based on a sampling frame starting from apprehended aliens may mislead us. And, of course, there's always the problem about the degree to which Chicago differs from other SMSAs. The latter problem is less difficult to solve than the former since the study could be repeated in other SMSAs.

In closing, let me commend the authors for their stimulating papers and excellent presentations. I hope that their work will stimulate others to continue working on these difficult topics.