

DISCUSSION

Robert Ferber, University of Illinois

The six papers presented at this session are very diverse and cover a wide variety of topics. From a subject area point of view, they relate to health care, crime, economic status and survey response. They can also be classified in terms of techniques, which seems more appropriate in the present instance, into three categories--data collection techniques (the papers by Verbrugge, and by Murphy, Cowan and Weiner), response rates (Wiseman and McDonald, and the paper by Hutcheson and Prather), and use of administrative records for estimating response errors (the papers by Marquis, and by Vaughan and Coder). The papers are discussed in this order.

The Verbrugge paper is a very informative review of past studies relating to health diaries and provides a great deal of useful information on the subject. It was gratifying to read the findings on uniformly high response rates (usually in the 90's) and the evidence that the diaries tend to provide more detail and more information than other approaches.

In one way, this is no surprise, since the diary component of the 1972-73 Consumer Expenditures Survey received response rates well in the 90's, and this even after requiring highly detailed expenditure information. On the other hand, these results must be treated with some skepticism, for reasons that Dr. Verbrugge mentions in her paper, but which need emphasis. This is that all of these surveys dealt with white urban populations. There is no evidence at the present time how well a diary approach would work on, say, a rural poorly literate population or a black inner-city population.

One other aspect of these studies that needs special attention is the danger of conditioning. In the case of these health care studies, the evidence is not clear whether such conditioning exists. For other types of studies, however, there is clear evidence of conditioning, as with consumer expenditures studies. This topic needs further investigation.

The paper by Murphy, Cowan, and Weiner is a very long one, and tells, at least me, more about victimization rates than I really want to know. The principal result of this study is not surprising, namely, that supplementary questions on attitudes provide higher reports of victimization, especially on crimes reported to police. This result is in line with the newer ideas about "training respondents to give information", such as those developed by Cannell and Sudman.

Actually, I suspect that the effect of these supplementary questions helps to build rapport in some instances, with the result that more information is then provided, but helps to tire people in other instances, with the result that less information is provided. I would hypothesize that crimes not reported to police are more likely to be mentioned by people who are "turned on" by the supplementary questions. Whether such a hypothesis is valid can only be ascertained through more detailed examination of these data.

Turning to the studies on response rates, the paper by Wiseman and McDonald provides some very interesting information on refusal rates in marketing research surveys. The study contains some major limitations, as the authors recognize, but the results are nevertheless quite useful. My guess is that the median refusal rates shown in the paper are lower limit estimates, in view of the fact that the participating organizations could exclude any studies they felt like, and that 30% of the organizations did not even participate. The fact that half of the organizations in the New York and the New Jersey area did not participate strengthens my suspicion that these are lower limit estimates, since it is precisely surveys in the New York Metropolitan area that are likely to encounter the highest rates of refusal (assuming, as seems reasonable, that these organizations were doing surveys in that area).

The attempt to relate refusal rates to different survey and methodological variables in Table 3 only whets our appetite. A number of variables are shown in that table, but many other variables for which the authors seem to have data are not included. There is at least one other key variable that should be included, though it is not easy to measure. This is some measure of efficiency or experience of the survey organization. Clearly, putting together such a variable is a tricky business, but there are possibilities, such as combining information on availability of a central telephone interviewing facility and the type of interviewer supervision.

A different sort of nonresponse is considered in the paper by Hutcheson and Prather, namely, nonresponse to a question on income. However, a number of aspects about this paper puzzle me. For one thing, the separation by the authors of "don't know" from refusals seems questionable. Certainly in the case of response to income questions there is ample reason to believe that a "don't know" is another way of refusing. Second, no information is given about response rates, or the context in which the income data were sought or even the characteristics of the interviewers. The statement that "inexperienced interviewers were used" naturally makes one suspicious of the quality of the data.

The discriminant analysis raises further questions. For one thing, no attempt seems to have been made to deal with the danger of search bias. This may not be too important in the present instance, in view of the fact that the accuracy of classification of the observations using the discriminant function is only 42%. This is quite poor in view of the fact that the naive approach of classifying every observation in the modal category yields an accuracy of 59%.

The two papers on the use of administrative records for estimating response errors are different, but are both very useful. Marquis presents a very useful conceptual framework for bringing out the relationships between the different sorts of record checks, and the types of biases that can be expected. It should be stressed, however, that the empirical results, and

the generalizations, refer to only surveys relating to medical experiences, and may not hold for surveys on other subjects. For example, the conclusion of there being little or no response bias in survey reports of hospital episodes is hardly applicable to income or financial assets where underreporting is substantial in the survey interviews. Similarly, memory decay may not be important in the report of hospital episodes, but is extremely important when it comes to reports of different types of income and expenditures, especially small expenditures.

To illustrate the type of asymmetry involved in considering different types of record checks, let me show how the different definitions of survey bias turn out using the author's own formulas as applied to what we know about reports of savings accounts. We postulate that 90% of the population owns one or more savings accounts, that only 60% of the owners report this fact in a survey, that 10% of non-owners report that they own a savings account, that record checks are 90% accurate for owners and 100% accurate for non-owners. On that basis, we obtain, using the author's expressions, that the true survey bias is 37%, that the bias due to an ex post check of survey responses (the author calls this ex ante) is only 6%, while the bias due to a population survey for which records served as a basis of 32%. Clearly, the survey based on records is by far the more important for yielding accurate information on this characteristic. Indeed, for a widely-dispersed asset like savings accounts, the other type of

record check is hardly feasible.

In his paper, Vaughan presents results from a pilot survey on the reporting of supplemental security income by recipients. The results are by far the most encouraging I have yet seen with regard to income reporting. In the past, the principal source of error and of bias in this type of study has been nonreporting of the income or asset. In this instance, however, nonreporting was less than 4%. This figure is remarkably low, and almost too good to be true. A possible fly in the ointment is that approximately 30% of the original sample was not interviewed, and one may speculate whether nonreporting among this group would not have been much higher.

The misclassification of SSI with Social Security income as a major source of error is not surprising, since I suspect that the far majority of even professional people may not be able to clearly distinguish between these two types of income. The author is very well aware of this, and the steps suggested to take care of this problem in the future seem very reasonable.

In future studies of this question, I would suggest that consideration be given to the effect of the size of SSI on reporting and nonreporting. Also, I would hope that future surveys would put much more emphasis on dealing with the problem of minimizing the number of nonrespondents.