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I shall discuss the papers in the order they were presented, trying to highlight some of the findings that I found most interesting and pointing out some areas that still need more research.

Scott's results indicating greater satisfaction with face-to-face interviewing are not by themselves likely to stem the tide toward telephone interviewing which is primarily due to the reduced cost and the greater accessibility of respondents on the phone.

Phone interviewing, however, does have several problems that are highlighted in the Scott paper and unless these problems are solved there is a real possibility that legal restrictions against phone interviewing may be imposed at local or federal levels. Fortunately, in bills introduced to date a distinction has been made between legitimate surveys and sales calls. If this distinction is blurred, however, it may lead to serious restrictions on phone surveys.

Scott points out, and I think other phone surveyors would agree, that feelings of apprehension about the legitimacy of the survey and the feeling of being interrupted by the call are two chief negative comments that are given. We definitely need more work of the type proposed by Cannell and Groves to provide initial information at the beginning or end of the survey to legitimate the survey. As Scott points out, positive experience with a professional phone survey would lead to greater future acceptance.

The use of advance notification through advance letters as well as press releases to the newspapers and notification of local authorities is still common practice on face-to-face surveys. It is my impression that on most RDD national surveys nothing is done in the way of advance notification. And yet it is clearly more necessary on the phone where the interviewer cannot show anything. Certainly there are some middle-range alternatives. The local press and police can still be notified, although this may require some effort or more clustering than is done at present.

Sending an advance letter to selected numbers that are listed or a thank you letter to respondents who provide an address at the end of the interview also helps legitimize the survey at some additional cost. To summarize, more can be done than is being done currently--some of the limitations are not inherent in phone procedures.

The problem of intrusion can be handled through proper interviewer sensitization and training. It's obviously possible to get someone out of bed or the bathtub to answer the phone, just as it is on a face-to-face interview. On a face-to-face interview the interviewer will see what is happening and offer to wait or come back at a more convenient time. Interviewers should be sensitized to do the same thing on the phone, and to offer to call back if this is not a convenient time for the respondent.

In his introduction Scott makes the statement that respondent feelings about a survey strongly influence the quality of information the researcher obtains. It is unfortunate that he and his

colleagues have not in this paper or elsewhere looked at the differences in response to substantive questions using respondent preference for mode of data-gathering as an explanatory variable. I suspect that the results would probably show little difference between those interviewed by phone who did or did not like the phone mode of interviewing. Indeed, this variable reminds me of another which has frequently been collected--the interviewer's judgment of how interested and cooperative the respondent was in the interview. Although some respondents are perceived as very cooperative and some as uncooperative, this appears to have no impact on the quality of data provided. Unless preference for mode of interviewing can be shown to have an impact on either response or cooperation it will not be of much use to those of us designing surveys.

Professor Hershey Friedman is put into double jeopardy by having me discuss both of his papers, although the senior authors are his colleagues Linda Friedman and Andre San Augustine. Since these two papers report research done on a shoestring it is unfair to evaluate them on the same basis as one would a Census methodological study. They really raise more questions than they answer.

The Friedman and Friedman study utilizing only one interviewer does appear to confirm results that have been observed earlier in face-to-face interviews, that observable characteristics such as race do influence responses directly related to racial questions. If one substitutes voice and name cues for appearance the same results would hold to the extent that the cues are unambiguous. Thus, one would expect the same response differences between male and female interviewers to sex-related attitudes on the phone as in face-to-face interviews.

One must be concerned about the low cooperation rate of 37% obtained by this one interviewer, which is half that usually obtained on phone surveys. As the authors speculate, it may be due to a weak introduction and/or the lack of interviewing skills.

Perhaps more serious are the possibilities that some of the results may be due to that one interviewer's behavior or expectations. As the Rosenthal experiments show, experimenter expectations can influence the results in the direction of those expectations in subtle ways. For example, it is not clear how the response categories were given to the respondent for the 16 items. Was there a standard wording? Did the interviewer use the standard wording for each item and respondent? It has been shown that respondents have some difficulty keeping all alternatives in mind in a telephone interview. An inexperienced interviewer might well be tempted to modify the wordings to avoid monotony. These modifications might be responsible for some of the observed effects.

One final comment on the data analysis Perhaps more interesting than the mean differences are the differences in the variances. This suggests that what happens is not that the entire

distribution shifts, but rather that the most bigoted answers are omitted to the perceived Black interviewer. One way of looking at this would be to look at the proportion agree, disagree and neutral.

To summarize, this paper with all its methodological flaws, still seems convincing because it is consistent with all the earlier work in the area.

The Augustine-Friedman paper compares three methods of data collection on attitudes toward Blacks. I would question the author's assumptions that this topic is very ego-involving to the respondents although it is to the investigators. Thus, the threat level of these items is considerably lower than asking a respondent if he or she has been arrested for drunken driving. The questions are those used in the Friedman and Friedman study discussed above.

One method, the mail questionnaire produced a 30 percent response rate compared to the 77% on the phone and the 80% self-administered. Obviously the very low mail response makes it impossible to compare response effects between mail and the other methods. One reason for this poor mail response is the very poor introduction used and the lack of follow-ups. Dillman's new book Mail and Telephone Surveys gives some good advice on the

types of appeals that produce very good response on mail procedures. As has been often observed the low mail response is also a biased response with more highly educated, less bigoted persons more likely to respond.

The other two procedures were not significantly different in response. It is interesting to compare the mean scores of attitudes in the two papers. Here the means are about 57 while the combined mean in the previous study is 56. I find this encouraging since it is an indication of the reliability of the measuring instrument with a similar population.

The conclusion that the personally delivered, self-administered questionnaire is the most reliable for obtaining information of a complex, embarrassing, ego-involving or sensitive nature is neither supported from the data of this study nor from earlier work that has been done. On a self-administered form, the respondent must perceive someone reading the responses. Thus, the same under-reporting of threatening events such as arrests is seen as is found in more personal methods. There does seem to be some evidence that there is less over-reporting of socially desirable behavior on the less personal self-administered forms.