

INTERACTION OF SURVEY QUESTIONS AS IT RELATES TO INTERVIEWER-RESPONDENT BIAS

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I. Introduction. Analysis of data from the Current Population Survey (CPS), Health Interview Survey (HIS), and National Crime Survey (NCS) - Cities Sample shows there is serious impact on results from interaction of questions on respondents and from subtle differences in how interviewers choose to emphasize and use probing questions. For the four cases that will be discussed, supplemental questions were added to the main questionnaire without advance controlled experimentation to analyze the impact on the original data series or on the estimates from the supplements. The results for the four cases proved to be startling.

The remainder of this first section summarizes the background of the three surveys to which the four supplements were added. In section II, results of the four supplement cases are presented. Possible explanations for the results, as well as implications for survey design and the magnitude of nonsampling error are discussed in the last section; they serve as warnings to questionnaire designers regarding new concerns that must be addressed.

A. Brief Description of the Three Surveys. The Current Population Survey, Health Interview Survey, and National Crime Survey - Cities Sample are conducted by the U.S. Bureau of the Census for the Bureau of Labor Statistics, the National Center for Health Statistics, and the Law Enforcement Assistance Administration, respectively. The CPS is a household sample survey conducted monthly to provide estimates of employment, unemployment, and other characteristics of the general labor force, of the civilian noninstitutional population as a whole, and of various subgroups of the population. The HIS gathers information on personal and demographic characteristics, illnesses, injuries, impairments, chronic conditions, and other health topics from weekly samples of households. The NCS - Cities Sample was designed to develop information on the nature and extent of crime victimization and its impact in selected large central cities. Information was collected from victims identified by screening a general population sample about recent experiences with selected crimes of violence and theft, including events not reported to police as well as those that were.

B. Rotation Groups in the CPS. Each CPS sample is divided into eight different parts called rotation groups. Each rotation group is a scientifically selected subsample of the complete sample, and each month one of the rotation groups is introduced into the survey. The households in a rotation group are interviewed in each of four consecutive months, dropped from the survey for eight months, and then interviewed for four more consecutive months. The month-in-sample designation indicates how many months a rotation group has been in the survey.

One phenomenon associated with this rotation pattern is "rotation group bias" where the level of the estimates for many characteristics vary depending on the number of months the household has been in sample. For example, the unemployment rate is usually highest for first month-in-sample households, drops off for the second, is lower

again for the third and fourth, rebounds somewhat for the fifth, and then continues dropping off for the sixth, seventh, and eighth.

II. Cases

A. Effect of Attitude Supplement on NCS-- Cities Sample Victimization Data. In each of the NCS cities surveys, a lengthy series of attitude questions was asked of respondents 16 years and older in a random half of the sample units as a supplement to the basic crime victimization questions. For each respondent, the attitude questions were purposely asked before the victimization questions in order to prevent influencing or conditioning the attitude responses, since the attitude questionnaire included such topics as neighborhood safety, behavior patterns, and opinions regarding local police, crime trends, and news coverage of crime.

The attitude supplement considerably lengthened total interview time for the half-sample in which it was administered, and concern arose that the additional time might cause increased respondent fatigue, which in turn might result in fewer reports of victimization than in the half-sample not asked the attitude supplement. To determine whether this hypothesized effect actually occurred, special tabulations were produced for each of the 13 cities surveyed in 1975, showing personal and property crime victimization rates separately for each of the half samples. Personal crimes included completed and attempted assault, rape, robbery, purse snatching, and pocket picking. Property crimes included completed and attempted burglary, larceny (both household and personal), and auto theft.

Data in table 1 show relative differences in victimization rates reported in the half samples containing the attitude supplement versus those without the supplement. They clearly indicate that inclusion of the attitude supplement in the NCS interview had a statistically significant and substantial impact on the victimization rates obtained.

The most striking aspect of the effect of the attitude supplement, however, is that the differences are not in the hypothesized direction. Rather, in almost every city, the victimization rate measured for the half-sample in which the attitude questions were asked was higher than for the other half-sample in which they were not asked, both for property and for personal crimes. The original hypothesis, then, that the increased interview time with attitude questions would result in lower productivity in reporting crime incidents, is not supported by these data; in fact, it is dramatically refuted.

B. Impact of the Discouraged Workers Questions on CPS Unemployment Data. During the CPS interview, a series of questions are asked only of those people who had answered in previous questions that they are neither working, nor looking for work, and therefore are not in the labor force. Further, they are only asked of people in two specific rotation groups out of eight rotation groups in the CPS. These questions are as follows (not all are asked of each eligible respondent):

24A. When did...last work for pay at a regular job or business, either full- or part-time?

Table 1. Victimization Rates and Relative Differences in Rates Between the Half-Samples

| City | Rate for Half-Sample With the Attitude Supplement | | Rate for Half-Sample Without the Attitude Supplement | | Relative Differences in Rates Between the Half-Samples | |
|----------------|---|-----------------|--|-----------------|--|-----------------|
| | Personal Crimes | Property Crimes | Personal Crimes | Property Crimes | Personal Crimes | Property Crimes |
| Chicago | 8.40 | 42.95 | 7.09 | 38.97 | +18.47** | +10.21** |
| Philadelphia | 6.64 | 37.83 | 5.63 | 35.72 | +17.99** | + 5.93** |
| New York | 6.16 | 27.43 | 5.36 | 23.89 | +14.97** | +14.82** |
| Los Angeles | 7.50 | 60.43 | 5.81 | 51.22 | +29.02** | +17.97** |
| Detroit | 8.93 | 52.95 | 8.21 | 48.66 | + 8.82* | + 8.82** |
| Portland, Ore. | 8.40 | 69.24 | 6.77 | 63.70 | +24.09** | + 8.68** |
| Denver | 8.67 | 68.98 | 6.76 | 59.99 | +28.12** | +14.97** |
| Baltimore | 10.67 | 49.91 | 8.67 | 45.60 | +23.03** | + 9.45** |
| Atlanta | 6.06 | 49.81 | 4.54 | 44.35 | +33.39** | +12.31** |
| Dallas | 6.32 | 63.91 | 4.62 | 54.38 | +36.84** | +17.53** |
| Cleveland | 8.34 | 50.90 | 6.90 | 44.39 | +20.90** | +14.67** |
| St. Louis | 5.95 | 49.45 | 5.51 | 40.02 | + 7.85 | +23.57** |
| Newark | 5.30 | 27.20 | 4.38 | 24.96 | +21.02** | + 8.99** |

* Indicate statistically significant difference at the 10 percent level.

** Indicates statistically significant difference at the 5 percent level.

- 24B. Why did...leave that job?
- 24C. Does...want a regular job now, either full- or part-time?
- 24D. What are the reasons...is not looking for work?
- 24E. Does...intend to look for work of any kind in the next 12 months?

This series was not asked in the CPS prior to January 1967. From January 1967 through December 1969, the series was asked in housing units in sample for their 1st and 5th months interviews. From January 1970 to the present, the series has been asked in 4th and 8th month sample housing units only. The data are averaged and published once a quarter as information on discouraged workers, that is, people who want a job now but are classified as not in the labor force.

The questions are asked at the end of the interview, and interviewers are instructed not to go back and change answers to previous questions which are inconsistent with answers to these probing questions. Thus, theoretically, inclusion of this series should have no effect on the basic labor force estimates, but it does!

Analysis of the effect of asking this series of questions for different months-in-sample is unfortunately complicated by the interaction of the questions with rotation group bias, which was explained in the introduction. The rotation group bias pattern changes as a function of when and if the discouraged workers questions are asked. A direct comparison across time is also made more difficult because the rotation group bias pattern for a characteristic tends to be affected by the magnitude of the characteristic.

The results in table 2 show comparisons between the three situations regarding the discouraged workers questions. To derive the figures in column 1, rotation group bias patterns were determined for a period of time prior to January 1967. Adjustments were then made to these observed patterns to take into account the different levels of labor force characteristics of the 1970-1972 period. Some assumptions and subjective decisions were required to make these adjustments. The adjusted rotation group bias pattern was then applied to the actual data for mid-1970 to mid-1972 by rotation group to arrive at the speculated results. A similar procedure was used to produce

the figures given in column 2.

The differences among the three columns are small and subject to unknown errors as a result of the assumptions and subjective decisions in the adjustment procedure. But keep in mind that these differences occur when asking the discouraged worker questions for only one-quarter of the sample (the differences might be twice as great if asked for half the sample), and the sampling errors on these estimates are small.

Table 3 shows that the impact of the discouraged workers questions is greater for individual months-in-sample than for the overall estimates across all months-in-sample. The rotation group bias indices in the table were computed for the *i*th month-in-sample as follows:

$$\frac{\text{Estimate for the } i^{\text{th}} \text{ month-in-sample}}{\text{Total Estimate}/8} \times 100$$

Thus, an index above 100 means a disproportionately large estimate for that month-in-sample, while an index below 100 means a small estimate. Circled figures in the table are for those months-in-sample in which the discouraged workers questions are asked. The effect is largest for estimates of unemployed, which indicates that when the discouraged workers questions are asked, there is a substantial increase in the number of unemployed. The difference between 1968-1969 and 1970-1972 may, of course, reflect real differences over time in rotation group bias as well as the effect of the discouraged workers questions.

C. Impact of Acute Conditions Supplement on Number of Acute Conditions Reported in the Health Interview Survey. A special supplement of questions, asked about acute conditions first noticed only within the two weeks prior to the week of interview, was included in the HIS in 1973 and 1974. An acute condition is defined as a condition which has involved either medical attention or restriction of routine daily activity. The interviewer was required to complete a separate form during the initial interview when possible, or by return visit or telephone callback if the person who had the acute condition was not available for interview.

The dramatic effect of including the supplemental questions on acute conditions is shown in table 4. Incidence and number of acute conditions which had onset in the two-week period preceding

Table 2. Estimates of Labor Force Characteristics for Different Uses of the Discouraged Workers Questions
24 Month Average of Noncomposite Estimates, Mid-1970 to Mid-1972 (in thousands) ⁴

| | Speculated CPS Results | | Actual CPS Results |
|-----------------------------|--|--|--|
| | Discouraged Workers Questions Not Asked at all | Discouraged Workers Questions Asked in MIS 1 and 5 | Discouraged Workers Questions Asked in MIS 4 and 8 |
| | TOTAL | | |
| Not in civilian labor force | 55,624 | 55,331 | 55,436 (156) |
| Civilian labor force | 84,357 | 84,651 | 84,545 (160) |
| Employed | 79,453 | 79,630 | 79,566 (161) |
| Unemployed | 4,904 | 5,021 | 4,979 (41) |
| Rate | 5.81 | 5.93 | 5.89 (0.05) |
| MALES 20+ | | | |
| Not in civilian labor force | 10,516 | 10,450 | 10,474 (82) |
| Civilian labor force | 47,875 | 47,940 | 47,917 (150) |
| Employed | 45,854 | 45,904 | 45,886 (149) |
| Unemployed | 2,021 | 2,036 | 2,031 (25) |
| Rate | 4.22 | 4.25 | 4.24 (0.05) |
| FEMALES 20+ | | | |
| Not in civilian labor force | 37,697 | 37,544 | 37,598 (140) |
| Civilian labor force | 28,897 | 29,050 | 28,995 (127) |
| Employed | 27,281 | 27,380 | 27,344 (125) |
| Unemployed | 1,616 | 1,671 | 1,541 (21) |
| Rate | 5.59 | 5.75 | 5.69 (0.08) |

() = one standard error.

Table 3. Rotation Group Indices in the CPS for Two Periods, 1968-69 (T1) and 1970-72 (T2)

| | | Month in sample | | | | | | | | Monthly avg. size (000) | Standard error of index |
|------------------------------|----|-----------------|-------|-------|-------|-------|------|------|-------|-------------------------------|-------------------------------|
| | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | | |
| Total population 16 and over | | | | | | | | | | | |
| Civilian labor force | T1 | 102.3 | 100.3 | 99.8 | 99.5 | 100.8 | 99.3 | 99.1 | 99.0 | 80,340 | 0.3 |
| | T2 | 101.6 | 100.0 | 99.6 | 100.3 | 100.0 | 99.1 | 99.2 | 100.0 | 84,654 | 0.2 |
| Employed | T1 | 101.6 | 100.2 | 99.9 | 99.8 | 100.4 | 99.4 | 99.4 | 99.3 | 77,285 | 0.3 |
| | T2 | 101.1 | 100.0 | 99.7 | 100.3 | 99.9 | 99.4 | 99.5 | 100.1 | 79,913 | 0.2 |
| Unemployed | T1 | 120.0 | 101.5 | 96.4 | 92.8 | 109.3 | 96.5 | 92.6 | 91.0 | 3,055 | 2.4 |
| | T2 | 109.2 | 100.3 | 98.1 | 101.2 | 102.3 | 96.7 | 94.1 | 98.2 | 4,741 | 1.2 |
| Males 16 and over | | | | | | | | | | | |
| Employed | T1 | 100.9 | 100.0 | 100.0 | 99.8 | 100.2 | 99.8 | 99.7 | 99.7 | 48,589 | 0.3 |
| | T2 | 100.7 | 99.9 | 99.8 | 100.2 | 99.9 | 99.7 | 99.7 | 100.2 | 49,637 | 0.2 |
| Unemployed | T1 | 114.1 | 102.6 | 98.0 | 95.6 | 106.0 | 97.7 | 93.4 | 92.6 | 1,490 | 3.5 |
| | T2 | 105.4 | 101.4 | 99.9 | 101.6 | 100.3 | 98.0 | 95.6 | 97.9 | 2,578 | 1.6 |
| Females 16 and over | | | | | | | | | | | |
| Civilian labor force | T1 | 104.0 | 100.6 | 99.5 | 99.2 | 101.4 | 98.7 | 98.4 | 98.1 | 30,261 | 0.6 |
| | T2 | 102.7 | 100.0 | 99.4 | 100.4 | 100.2 | 98.6 | 98.6 | 100.0 | 32,439 | 0.5 |
| Unemployed | T1 | 125.5 | 100.2 | 94.6 | 90.5 | 112.5 | 95.1 | 91.8 | 89.5 | 1,564 | 2.8 |
| | T2 | 113.8 | 99.0 | 95.9 | 100.7 | 104.4 | 95.2 | 92.2 | 98.6 | 2,163 | 1.5 |

Circled figures are for those months-in-sample in which the discouraged workers questions are asked.

the interview were substantially lower for 1973 and 1974 (the two years the supplement was asked) than for 1971, 1972, and 1975. The number of recent acute conditions per 100 persons declined by 20.3 percent from 1972 to 1973 and increased by 20.7 percent from 1974 to 1975. We would emphasize that although these results were not obtained under controlled experimental conditions, the differences are so striking that it seems highly unlikely that real changes in the nation's health account for these changes. Additional analysis showed that the drop from 1972 to 1973 could not be associated with any particular segment of the population, and it did not appear that there was any appreciable reduction in medical attention or activity restriction that would account for the lower incidence rates. The incidence rates of 175.1 and 175.7 per 100 persons per year for 1973 and 1974, respectively, were the lowest reported in the Health Interview Survey since its inception in July 1957.

Acute conditions lasting less than three months

may be reported in any of the probe questions in the early part of the main questionnaire. Interestingly, the relative number of such acute conditions lasting less than three months did not change between 1971 and 1975; it remained at about 120 acute conditions per 100 sample persons per year. However, the percent of acute conditions reported as beginning in the two weeks prior to interview week was lower in 1973 and 1974, as shown at the bottom of table 4. The only change in the questionnaire relating to acute conditions for 1973 and 1974 was the requirement to complete an additional supplement about the conditions first noticed during the two-week reference period, and the effect of this change had been expected to be minimal.

D. Impact of the July/August Not in the Labor Force Supplement. The Survey of Work History and Job Search Activities of Persons Not in the Labor Force was a CPS supplement administered to a portion of the two outgoing rotation groups (months-in-sample 4 and 8) in both July and August 1976.

Table 4. Incidence and Number of Acute Conditions by Sex and Age

| All Acute Conditions | 1971 | 1972 | 1973 ¹ | 1974 ¹ | 1975 |
|---|-----------------|-----------------|-------------------|-------------------|-----------------|
| Incidence in Thousands | | | | | |
| Both Sexes | 442,203 (7,998) | 448,607 (8,093) | 360,448 (6,784) | 364,278 (6,841) | 443,119 (8,012) |
| Male | 202,787 (4,387) | 202,558 (4,383) | 170,046 (3,871) | 171,661 (3,896) | 204,920 (4,420) |
| Female | 239,416 (4,954) | 246,049 (5,056) | 190,402 (4,193) | 192,617 (4,228) | 238,199 (4,936) |
| Number of Acute Conditions Per 100 Persons Per Year | | | | | |
| Both Sexes | 218.5 (4.0) | 219.7 (4.0) | 175.1 (3.3) | 175.7 (3.3) | 212.0 (3.8) |
| Male | 207.8 (4.5) | 205.8 (4.5) | 171.3 (3.9) | 171.6 (3.9) | 203.2 (4.4) |
| Female | 228.5 (4.7) | 232.8 (4.8) | 178.7 (3.9) | 179.5 (3.9) | 220.1 (4.6) |
| Percent of all Reported Acute Conditions Reported as Beginning in the 2-Week Reference Period (Based on unweighted numbers) | | | | | |
| | 75.4 | 73.6 | 56.6 | 57.9 | 67.6 |

(.) = One standard error.

¹ The Acute Conditions Supplement was asked this year.

It was completed immediately after the basic CPS interview whenever possible. If a person to whom the questions applied was not available, a self-enumeration questionnaire was left or mailed for the respondent to return by mail. Follow-up was done as part of the next month's CPS for those people who had not mailed back the questionnaire. Up to 60 additional questions had to be answered, which took an average of twenty more minutes to complete.

The potential universe eligible for the supplement consisted of persons 14 years old and over who were not in the labor force and who answered "yes," "maybe," or "don't know" to question 24C and/or 24E ("Does...want a regular job now, either full- or part-time?" and/or "Does...intend to look for work of any kind in the next 12 months?"), intended to identify discouraged workers.

Occasionally interviewers forget to ask the discouraged workers questions because it is only administered to a small part of the sample, and responses are imputed for the missing entries by the basic CPS computer edit. In July and August 1976 sample persons for which question 24C and/or 24E were allocated¹ were included in the universe for the supplement and a questionnaire mailed to them. Since interviewers were not involved in determining their eligibility, the inclusion of these people was indeed fortunate as they became a pseudo-control group.

The final eligible universe interviewed for the supplement was about half of that expected! The noninterview rate was high (about 20 percent) but too few sample persons were identified as eligible for the universe! Three separate sets of data support this statement. First, the number of persons reported as not in the labor force and intending to look for work in the next 12 months (a "yes" or "maybe" response to question 24E) decreased significantly in July and August 1976 from the June 1976 count (about 27 percent) and from the July and August 1975 figures (about 25 percent, see table 5). The seasonally adjusted number of persons who "want a job now" (a "yes" or "maybe" response to question 24C) in 1976 dropped 20.0 percent from the second to the third quarter and increased 25.3 percent in the fourth quarter; this pattern was not repeated in 1975 or 1977 (see table 6).

Secondly, this indication of a universe problem was further supported by comparing sample persons who were allocated to be in the target population without interviewer intervention (pseudo-control

group) with the nonallocated eligibles. Assuming the allocated eligibles group is representative of the larger nonallocated group, the behavioral characteristics of the two groups should be similar. However, the data revealed an in-scope interview rate ((interviews minus out-of-scopes²):NILF) more than three times greater for the allocated eligibles (33.3 percent) than for the nonallocated eligibles (9.1 percent).

Third, data from the CPS Content Reinterview and Reconciliation Program³ and from the June 1976 CPS were used to estimate that the "true" noninterview rate for the supplement was between 40.3 and 50.3 percent. These rates included regular non-interviews plus an estimated number of sample persons who should have been asked the additional questions based on potential universes for the supplement when it wasn't going to be administered.

Thus, there is strong evidence that the interviewers either intentionally or unintentionally changed the classification of some sample persons on the basic CPS questionnaire so as to make them ineligible for the supplement. Due to the magnitude of this problem, the supplement data were not considered publishable.

III. Conclusion

A. Possible Explanations for Results.

1. NCS - Cities Sample Attitude Supplement. The half samples administered the NCS-Cities Sample Attitude Supplement exhibited consistently higher victimization rates, opposite the hypothesized direction of lower rates. The most plausible explanation is that asking attitude questions before the victimization questions had a conditioning effect, whereby the respondents' awareness or memory regarding victimization experience was stimulated.

An alternative, but less likely, explanation for these results is that preceding the victimization questions with crime-related attitude questions increased the respondents' desire to be accommodating and more productive in what they perceived to be the goal of the survey--to elicit information about their experience with crime. This greater wish to accommodate, it could be concluded, results in a higher level of exaggeration and fabrication.

A third, more plausible explanation, which combines elements of both of the above is that administering the attitude questions serves to stimulate both memory and the desire to accommodate, with the result that more real (not fabricated) incidents that actually happened before the reference period are recalled and reported as having occurred within the reference period, i.e., increased

Table 5. Persons Aged 16 Years and Over Not in the Labor Force Who Intend to Look for Work in the Next 12 Months for Certain Months of Certain Years (in thousands)

| Month | Total Persons Not in the Labor Force Who Intend to Look for Work in the Next Twelve Months | | | | | |
|-----------|--|-------------------------------|-------|-------------------------------|--------|-------------------------------|
| | 1975 | Month-to-Month Percent Change | 1976 | Month-to-Month Percent Change | 1977 | Month-to-Month Percent Change |
| April | 9,701 | -12.5%** | 9,637 | -14.0%** | 10,004 | - 8.0%** |
| May | 8,488 | -12.9%** | 8,291 | - 8.7%** | 9,313 | -15.6%** |
| June | 7,397 | - 3.3 | 7,571 | -28.0%** ¹ | 7,856 | + 0.5 |
| July | 7,151 | + 6.5%** | 5,452 | + 3.1 | 7,897 | + 2.8 |
| August | 7,616 | +27.9%** | 5,620 | +76.4%** | 8,122 | +26.5%** |
| September | 9,739 | | 9,912 | | 10,278 | |

¹ Represents figures for the two months in which the supplement was administered.

Table 6. Persons Aged 16 Years and Over Not in the Labor Force Who Want a Regular Job Now, Either Full- or Part-Time: Quarterly Averages, Seasonally Adjusted (in thousands)

| Quarter | Quarter-to-Quarter | | Quarter-to-Quarter | | Quarter-to-Quarter | |
|------------------|--------------------|----------------|--------------------|-----------------------|--------------------|----------------|
| | 1975 | Percent Change | 1976 | Percent Change | 1977 | Percent Change |
| January-March | 5,211 | -2.4% | 5,388 | +0.7% | 5,663 | +1.7% |
| April-June | 5,084 | +5.3%** | 5,426 | -20.0%** ¹ | 5,762 | +2.6 |
| July-September | 5,354 | -1.8 | 4,339 | +25.3%** | 5,909 | -5.8** |
| October-December | 5,256 | +2.5 | 5,436 | + 4.2* | 5,565 | |

¹ This supplement was administered in two of the three months in this quarter.

* Indicates statistically significant difference at the 10 percent level.

** Indicates statistically significant difference at the 5 percent level.

telescoping. It is important to point out that if this or the second explanation is the true one, the higher victimization rates do not indicate better data, but simply an undesirable response bias.

2. CPS-Discouraged Workers Questions. Asking the discouraged workers questions of people determined earlier in the interview not to be in the labor force results in adding people to the labor force. The largest effect is an increase in unemployed, but estimates of employed and civilian labor force are also affected. The probable explanation is that interviewers appear to learn new information when asking these probing questions, and as a result, go back and reclassify people even though this is contrary to their instructions; that is, when answers to the supplemental questions contradicted answers on the original questionnaire, interviewers sometimes reacted by changing the original answers. An alternative, but not as widely accepted explanation, is that the household respondent becomes better attuned for answering questions about the labor force participation of subsequent household members after answering discouraged workers questions for one household member.

3. HIS-Acute Conditions Supplement. Requiring HIS interviewers to ask supplemental questions about only those acute conditions first noticed within the two weeks prior to the week of interview caused a reduction in the number of such acute conditions, while the number of total acute conditions reported as lasting less than three months did not change. It is probable that more care was exercised during the two years the supplement was administered in finding out if an acute condition did start within the two-week reference period as opposed to starting prior to the reference period. It is difficult for respondents to remember exactly when an acute condition actually started. Therefore, because of the additional requirements for completing a supplement, more extensive probing of respondents with greater use of memory aids resulted in more precise pinpointing of the date of onset of the acute condition. An alternative explanation is that the interviewer did not want the burden of or to burden the respondent with more questions and

therefore deliberately classified some respondents incorrectly.

4. CPS-July/August 1976 Not in the Labor Force Supplement. A requirement in July and August 1976 for the CPS interviewers to ask supplemental questions of persons who were not in the labor force, but who were identified as wanting work, dramatically reduced the target population to about half of what it should have been. Similar explanations to those given for the consequences of the Acute Conditions Supplement are possible here as well. More care through the use of the probing discouraged workers questions could have been used by the interviewer to determine if the respondent truly wanted a job now or in the next 12 months, even though this behavior stopped when the supplement stopped; or the interviewer did not wish to ask more burdensome questions and consciously or unconsciously changed the classification of the respondent accordingly. Another possible explanation, similar to that advanced for the effect of the discouraged workers questions on determining labor force status (case 2), is that the additional information obtained in the supplement resulted in interviewers reclassifying some respondents by changing original answers.

B. Implications. In the normal survey situation, a set of questions is included in a questionnaire depending on whether the answers are important in their own right. In all cases presented, results obtained from a basic set of questions were compared to results from the same basic set of questions but asked with a set of supplementary questions, which were related to the basic set but had no direct bearing on them. In each case, the inclusion of the supplementary set made a significant difference. There was serious impact on the data from interaction of the questions on respondents and from subtle differences in how interviewers chose to emphasize and use probing questions, and this impact was not a result of poor recruitment, training, or supervision of the interviewers.

There are implications here for two situations: first, where a questionnaire is being initially designed for a survey; second, where a basic

questionnaire is used in a recurring survey and there is interest in adding a supplementary set of questions.

For the first situation, a questionnaire designer cannot simply derive what appears to be a straightforward set of questions and assume that they will yield reasonably valid answers. For example, if one is interested in accurate estimates of the unemployment rate, it may not be sufficient to ask a set of questions about recent activities. It may be necessary to structure a whole set of probing questions on side issues, where the answers to the probes impact the determination of the basic labor force category. As a second example, one may be interested in detailed information about housing and also about the attitudes of persons in certain types of housing, e.g., low rent or low value. Ordinarily, one would then design a questionnaire with a set of attitudinal questions asked only when a certain set of answers to the "basic" housing questions is obtained, and would presume to get good housing data and good attitudinal data. It is our hope that, after reading this paper, a person would not proceed this way. (One alternative might be to ask attitudinal questions of everyone.)

When adding a set of supplementary questions to a recurring survey (the second situation), the problem then becomes retaining a consistent time series so that estimates of change over time are unbiased. If there is less interest in unbiased estimates of level rather than of change, it might become undesirable to add a set of supplementary questions, even if it is known that it will improve the accuracy of the answers to the basic questions. There are several possible solutions. Additional questions could be administered to only a portion of the sample, small enough not to have an appreciable affect on the total data, or to some portion of the sample which may not be used to produce basic estimates. Or, a different series of questions could be administered each enumeration period with the effect of consistently producing positive results, if the assumption that the interaction produces better data is valid. When interviewers can be expected to adjust for inconsistencies contrary to instructions (as shown by the discouraged workers series of questions in the CPS) then contradictory responses could be allowed for in the wording of the questionnaire. To prevent interviewers from changing or incorrectly marking the answers to certain questions on the original questionnaire, thereby influencing the expected universe for the additional questions and causing bumps in the data series (as may have happened with the HIS and CPS July/August supplements), control over distribution of the supplements must be taken out of the interviewers' hands. Either all respondents must be asked supplemental questions so that there is no reason for interviewers to modify the answers, or the determination of the universe for the questions must be made independently of the interviewer, and possibly even without the interviewers' knowledge.

Ultimately better data is needed, obtained through rigorous experiments within surveys, to give more precise estimates of the contributions of interviewers and/or respondents to the outcome of the data or of the interaction of the questions, since the nature, magnitude, and direction of such

nonsampling error cannot always be anticipated or predicted. Then the trade-offs between damage to the basic survey data, the value of the additional information, the costs of other ways of collecting the data (e.g., a separate survey), and the procedures to lessen observed biases can be evaluated in a more informed manner.

In conclusion, the findings presented here are rather disturbing. One knows that questions and questionnaire design and interviewers make a difference. On the surface, these were cases with a set of relatively innocuous questionnaire changes; and yet when examined closely, quantitatively large nonsampling errors became apparent. Are there other substantial nonsampling errors still hidden from view in these or other surveys? Likely! Can superficially simple differences or changes in questionnaires or interviewing procedures result in large biases? Certainly!

(contact the authors for a copy of the complete paper and references)

¹ A "blank" in question 24C or 24E was allocated as "Don't Know." Respondents with "blanks" in both questions or a "blank" in one question and a "No" in the other became eligible for the supplement.

² Out-of-scopes are persons who were eligible for the survey according to answers to the basic CPS questionnaire but whose answers on the supplement questionnaire showed them not to be eligible.

³ A portion of the sample is re-asked the basic labor force questions the week after interview week by senior interviewers for quality control purposes.

⁴ Composite estimates rather than noncomposite estimates are actually published, so the data in table 2 are not indicative of changes that would occur in publications. The noncomposite estimate is shown here because the effect of the discouraged workers questions is clearer and simpler for the noncomposite estimate than for the composite estimate.