RECALL BIASES FOR DIFFERENT FERENCE PERIODS IN THE NATIONAL CRIME SURVEY

John M. Bushery, U.S. Bureau of the Census

INTRODUCTION

The Census Bureau currently uses a 6-month reference period in the National Crime Survey (NCS). We conducted an experiment to evaluate the effectiveness of this 6-month span in collecting data about criminal victimizations, relative to a shorter reference period (3 months) and a longer period (12 months). The data reveal that a shorter reference period will elicit higher reported victimization rates than a longer period. Specifically, reported victimization rates under the 3-month reference period were higher than those reported under the 6-month reference period. The 6-month reference, in turn, elicited higher reported victimization rates than the 12-month reference period.

This paper provides a short background discussion of recall loss and allied memory related sources of response error. We next describe the National Crime Survey and discuss the design of the reference period research experiment. We also develop the models and estimators used in the analyses, present the main findings and discuss some implications of the findings and some investigations which may be made in the future.

RECALL LOSS

Many surveys use a procedure in which respondents are questioned about events which occurred during a set period extending into the past from the time of the interview. This reference period may cover a few days, several months, or more.

Research has revealed that as the reference period is lengthened, the levels of reporting tend to decrease. (1) This reduction in reporting levels has been called "recall loss." Recall loss is caused by two related phenomena. The first of these, "memory decay," is the decreasing ability of the respondent to remember an event as the time between the event and the interview increases. The second, which has been called the "reporting load" effect, results because the number of reportable events which occur is proportional to the length of the reference period. This increases respondent burden, or the reporting load, which may increase interview time. This may motivate some respondents to shorten the interview by failing to report events which are actually remembered. An extremely heavy reporting load may also cause under-reporting of less recent or less memorable events. (This aspect of the reporting load effect is closely associated with memory decay.) A third memory related phenomenon, telescoping, is involved in errors in reporting the time an event occurred. This time of occurrence may be shifted forward to a more recent time or backward to a time further in the past. In the NCS telescoping of events from the past into the reference period is controlled to a large extent by a procedure called "bounding." (2) In bounding, events reported during the current interview are matched against similar events reported during the preceding interview, so that duplicate reporting is usually eliminated. Telescoping events which occurred during the reference period to some time prior to the reference period is not detectable and its effect cannot be distinguished from that of memory decay. Misreporting the time of occurrence within the reference period, which is sometimes called "internal telescoping," does not affect levels of reporting, but does introduce some error into the data collected.

DESCRIPTION OF THE NATIONAL CRIME SURVEY (NCS)

The NCS is a nationwide, general population survey conducted monthly and based on a stratified multistage cluster sample of about 60,000 interviewed housing units. The sample is divided into rotating panels so that every month a different panel, consisting of one-sixth of the total sample (about 10,000 of the 60,000 units) is interviewed. Panels are interviewed at 6-month intervals for a period of three years. Each group of units which completes its three year tenure is retired from the NCS and replaced by a new rotation group. The sample clusters (segments) at the final stage of selection consist of an expected four housing units. Respondents to the survey are asked to supply information about criminal victimizations they may have experienced during the preceding six months.

The decision to use this 6-month reference period in the NCS was based on information obtained in a reverse record check study conducted to develop the survey methodology of the NCS. (3)

THE REFERENCE PERIOD RESEARCH EXPERIMENT

In preparation for the redesign of the NCS, the Bureau of the Census and the survey sponsor, the Bureau of Justice Statistics, decided to perform an experiment to evaluate the effect of reference period length on data collected in the survey. Reference periods of 3 months and 12 months were compared with the currently used 6-month reference period. The findings relate primarily to recall loss, the combined effect of memory decay and reporting load. Operational and budgetary constraints prohibited designing an experiment which could measure these factors separately.

One of the primary statistics of the NCS is the annual victimization rate, the number of victimizations reported as having occurred during a calendar year, divided by the number of units at risk (persons or households). This experiment measured differences in reported victimization rates under the three reference treatments.

Twelve mutually exclusive subsamples of the regular NCS samples were selected to receive the 3-month treatment before being returned to the regular NCS sample (i.e., 6-month treatment) for the rest of its participation in the NCS. Fifteen other mutually exclusive subsamples of the NCS sample were selected for the 12-month reference period treatment. Each of these subsamples received a single bounded interview using a 12-month reference period, after which it was returned to the regular 6-month treatment for the remainder of its participation in the NCS. The portion of the NCS sample interviewed using the 6-month reference period treatment served as the control in this experiment. With the exception of replacement households with no prior exposure to the NCS which moved into sample units, all households in the experimental subsamples received at least one interview using a 6-month reference period prior to the experimental interviews.

The length of participation in the experimental treatments was limited in an effort to minimize the differences between the experimental subsamples and the control group in the distribution by time-in-sample. Such differences could result in differential time-in-sample bias (also called panel bias or rotation group bias) which would confound the comparisons made to detect the bias due to recall loss (4).

During the course of the experiment the samples for all three treatments deviated somewhat from the ideal uniform distribution, in which one-sixth of the sample falls into each of six time-in-sample categories. Fortunately, the differences between treatments in the time-in-sample distribution appear small enough to have little or no confounding effect on the reference period comparisons.

Each of the 27 different subsamples consisted of one-twelfth of a regular monthly NCS sample (about 833 interviewed units). Since two experimental subsamples were usually interviewed during the same month (one using the 3-month treatment, the other using the 12-month treatment), only about five-sixths of the full NCS sample was available as the control group each month.

In effect, two separate experiments were conducted in this study. One measured the differences expected between a 3-month reference period and the current 6-month reference period. The other compared the 6-month reference period with a 12-month reference period.

Two basic assumptions have been made in this experiment. The primary assumption is that lower reporting rates for one methodology relative to another imply under-reporting of victimizations for that methodology. This assumption is supported by the fact that internally consistent reports of incidents are difficult to fabricate and respondents are unlikely to be motivated to do so. There is, however, no empirical evidence to support this assumption. Any lower level of reporting experienced with a longer reference period is therefore assumed to be caused by recall loss. If in fact over-reporting occurs in the shorter reference period, our estimates of the effects of recall loss will themselves be biased and our conclusions about the superiority of the shorter reference period will be suspect. A related assumption is that external telescoping is controlled through the "bounding" procedure. If this assumption is violated, each treatment will suffer from a source of "over-reporting," but not necessarily to the same extent. This too could throw doubt on any conclusions we reach.

CONSTRUCTION OF ESTIMATES

The data from the experimental subsamples can be combined to form annual estimates similar to those published for the NCS. The data from each experimental treatment were processed and weighted using the standard NCS procedures. Twelve difference (but overlapping) annual estimates were obtained using the 3-month treatment. The corresponding annual estimates for the 6-month treatment were compared with these. Similarly, 15 different annual estimates were obtained using the 12-month treatment. The corresponding estimates for the 6-month treatment were compared with these. The 12 pairs of annual estimates used in the 3-month versus 6-month comparison began with the period January 1978-December 1978 and moved in one-month increments through the period December 1978-November 1979. The first of the 15 pairs of annual estimates used in the 6-month versus 12-month comparison covered the period October 1977-September 1978. The last of these covered the period December 1978-November 1979. We must point out that the same data were sometimes used in two or more different annual estimates. For example, data collected in January 1979 using the 3-month treatment were used in four of the annual estimates. (These data refer to the period October-December 1978.) A similar situation occurs for data collected using the 6-month treatment in both comparisons. Only for the 12-month treatment are the data used in only one annual estimate.

The victimization rates used in the comparisons between treatments are averages of the 12 (or 15) individual annual victimization rates just described. Each may be considered a sort of "moving average" annual victimization rate spanning the period January 1978 through November 1979 in the 3-month versus 6-month comparison, or the period from October 1977 through November 1979 in the 6-month versus 12-month comparison.

As mentioned earlier, use of a reference period results in some bias due to recall loss. The expected annual victimization rate obtained using a 6-month reference period, θ_6, is more affected by this bias than the corresponding rate, θ_3, obtained using a 3-month reference period. If we define θ to be the expected victimization rate which would be obtained with no bias due to recall loss, then the bias caused by recall loss in the 6-month reference period can be written

$$B_6 = \theta_6 - \theta.$$

and the bias caused by recall loss in the 3-month reference period is

$$B_3 = \theta_3 - \theta.$$

The additional bias due to recall loss in estimates obtained using a 6-month reference period, relative to estimates obtained using a 3-month period is thus

$$B_6 - B_3 = \theta_6 - \theta_3.$$

If we assume θ is unbiased, the effect of this additional bias on the accuracy of the estimate can be seen by examining the mean squared error (MSE) for each estimate:

$$\begin{aligned}
\text{MSE}(\hat{\theta}_3) &= \sigma^2(\hat{\theta}_3) + (\theta_3-\theta)^2 \\
\text{MSE}(\hat{\theta}_6) &= \sigma^2(\hat{\theta}_6) + (\theta_6-\theta)^2 \\
&= \sigma^2(\hat{\theta}_6) + (\theta_6-\theta_3)^2 + (\theta_3-\theta)^2 \\
&\quad + 2(\theta_6-\theta_3)(\theta_3-\theta)
\end{aligned}$$

where $\sigma^2(\hat{\theta}_r)$ is the variance which would be obtained in the NCS using an r-month reference period. Note that use of a 3-month reference period would require interviewing approximately half as many sample units as could be interviewed with a 6-month reference period. Therefore, $\sigma^2(\hat{\theta}_3)$ is on the order of twice the magnitude of $\sigma^2(\hat{\theta}_6)$.

The contribution of recall bias from the 6-month reference period is greater than that from the 3-month reference period by the quantity

$$(\theta_6-\theta_3)^2 + 2(\theta_6-\theta_3)(\theta_3-\theta).$$

Unfortunately, we have no unbiased estimate of θ, so only the term $(\theta_6-\theta_3)^2$ can be estimated. This may be considered a lower bound on the contribution of the additional bias to the MSE, since by the assumption $\theta_6 \leq \theta_3 \leq \theta$ the term $2(\theta_6-\theta_3)(\theta_3-\theta) \geq 0$.

Similar relationships hold between the estimates obtained using 6-month and 12-month reference periods. That is, the additional bias due to recall loss is given by:

$$B_{12}-B_6 = \theta_{12}-\theta_6.$$

The contributions of recall bias to the MSE of the estimators for the 6-month and 12-month reference periods can be understood by examining the MSEs

$$\text{MSE } (\hat{\theta}_6) = \sigma^2(\hat{\theta}_6) + (\theta_6-\theta)^2$$

$$\text{MSE } (\hat{\theta}_{12}) = \sigma^2(\hat{\theta}_{12}) + (\theta_{12}-\theta_6)^2 + (\theta_6-\theta)^2 + 2(\theta_{12}-\theta_6)(\theta_6-\theta).$$

Again, only the term $(\theta_{12}-\theta_6)^2$ can be estimated and this term represents a lower bound on the difference of the bias terms in the MSE.

RELIABILITY OF THE ESTIMATES

Approximate variances on victimization rates for individual annual estimates were computed using the standard NCS variance approximation, with an appropriate adjustment for the reduced sample size of the experimental treatment group.

Since the same data were used in more than one of the individual annual estimates, the variances on the victimization rates obtained by averaging the individual rates include covariance terms for the repeated data. Our estimates of the variance of these averages take account of these covariances.

FINDINGS

Table 1 compares the victimization rates obtained using a 6-month reference period with those obtained using a 3-month reference period, for several types of crime. Crimes of violence, crimes of theft, and total personal crimes (the sum of the two preceding) are crimes for which the units at risk are persons 12 or more years old. Burglary, household larcency, auto theft, and total household crimes (the sum of the three preceding) are crimes for which the units at risk are households. Analyses have been completed to date only for these crimes among the general population. Further analyses are planned to explore how reference period length is related to levels of reporting among various population subgroups and for different crime types.

As can be seen in Table 1, victimization rates were reported in the 3-month treatment at significantly higher levels than in the 6-month treatment for all types of crime except burglary and auto theft. That is, the additional bias due to recall loss in the 6-month treatment over that occurring in the 3-month treatment is statistically significant at the 5 percent alpha level. For example, under the 3-month treatment the reported victimization rate for crimes against persons was 15.49 crimes per 100 persons 12 years or older. Under the 6-month treatment, only 12.85 such crimes were reported per 100 persons, a level of reporting 17 percent lower than under the 3-month treatment. Total crimes against households were reported at the rate of 26.83 per 100 households in the 3-month treatment and 23.00 in the 6-month treatment, a level of reporting 14 percent lower.

Table 1 also reveals that reported victimization rates under the 12-month reference period treatment are significantly lower than under the 6-month treatment for all crime types except crimes of violence and auto theft.

The additional bias due to recall loss in the 12-month treatment resulted in a victimization rate of 11.20 crimes per 100 persons for total personal crimes, 13 percent lower than the rate of 12.91 obtained using the 6-month treatment. For total household crimes the rate obtained using the 12-month treatment was also 13 percent lower than the rate obtained with the 6-month treatment, 19.86 per 100 households versus 22.75.

The additional bias due to recall loss, as shown earlier, increases the MSE of an estimate obtained using a 6-month reference period. Table 1 also displays estimates of $(\theta_6-\theta_3)^2$ and $(\theta_{12}-\theta_6)^2$, the lower bound on the contribution of additional recall bias to the MSE of an estimate obtained with a longer reference period. Estimates of $\sigma^2(\hat{\theta}_6)$, based on the current NCS sample design and estimation procedure, are shown in these tables as well, to indicate the contribution of sampling error to MSE $(\hat{\theta}_6)$ and MSE $(\hat{\theta}_{12})$.

It is interesting to note that a sufficient condition for MSE $(\hat{\theta}_6)$ < MSE $(\hat{\theta}_{12})$ is $\sigma^2(\theta_6) < (\theta_{12}-\theta_6)^2$. It is also interesting to note that for all types of crime examined, the estimates obtained from the experimental data have this relationship.

As can be seen, the contribution to the MSE of the additional bias due to recall loss in the longer reference period is usually many multiples of the contribution of sampling variability. This provides a good indication of the impact of recall loss on the accuracy of data currently collected in the NCS. Unfortunately, no confidence statements can be made about the relationship between $\sigma^2(\hat{\theta}_6)$ and $(\theta_{12}-\theta_6)^2$ because the complicated nature of the estimates of these parameters do not permit accurate variance estimation. The data in Table 1 nevertheless suggest that a substantial loss of accuracy can be expected if a 12-month reference period were adopted for the NCS.

There are two types of noninterview encountered in the NCS which might affect data quality, type A and type Z noninterviews. The type A noninterview occurs when no interview is obtained at an occupied housing unit. Reasons for failing to obtain an interview include refusals, the interviewer could find no one home (even after repeated visits), the occupants were temporarily absent (e.g., on vacation during the interview period, or other reasons (such as impassable roads, quarantined housing units, etc.). The other type of noninterview, the type Z noninterview, occurs when a person in the sample unit is not interviewed but other persons in the unit are interviewed. In some circumstances a proxy interview may be obtained from another household member, but usually the rules require accepting a noninterview if self-response cannot be achieved.

Table 2 compares type A and type Z noninterview rates for the experimental treatments with the rates for the 6-month treatment.

It was feared that the more frequent contact between interviewer and respondent in the 3-month treatment might result in an increased rate of refusals. The data in Table 2 reveal that this problem failed to materialize. However, since only two consecutive interviews were conducted at 3-month intervals at any given housing unit, the possibility still remains that repeated interviewing at these shorter intervals may have an adverse impact on noninterview rates. More research is required before a definite answer can be obtained. The type A noninterview rates for the 3-month and 6-month treatments were both 4.2 percent. In each treatment, refusals were the leading cause of noninterview, with 55.6 percent of all type A's in the 3-month treatment and 54.3 percent in the 6-month treatment caused by refusals. This difference in treatments is not significant at the 5 percent alpha level. The type Z noninterview rates under the two treatments were also equal, 2.4 percent for each treatment.

As mentioned earlier, self-response is the preferred method of data collection in the NCS. Proxy response is usually felt to yield less accurate data, so proxy interviews are conducted as a last resort, and only under special conditions. The proportions of interviews conducted using self-response in the 3-month and 6-month treatments were 93.5 and 93.3 percent, respectively, not a significant difference at the 5 percent alpha level.

There is some evidence that interview mode (i.e., personal visit or telephone) may affect the data collected in the NCS (5). If the distribution of interviews by mode in the experimental groups differed from that in the control group this could have a confounding effect on any analyses. However, as Table 2 reveals, any difference between the 3-month and 6-month treatments in the proportion of telephone interviews is negligible. This also indicates that reference period length (for 3-month versus 6-month reference periods) has no measurable effect on respondent type or interview mode.

Table 2 indicates that there were also no significant differences in noninterview rates in the distribution of interviews by respondent type or interview mode between the 6-month and 12-month treatments.

Finally, it appears from Table 2 that coverage of the target population within interviewed housing units is at most negligibly affected by reference period length.

LIMITATIONS OF THE DATA

Despite every effort to equalize the distribution by time-in-sample among treatments, units interviewed under the 3-month treatment were subjected to a different number of interviews than control group units. This probably caused a small amount of differential time-in-sample bias between the 3-month treatment and the control. However, since the differences in the time-in-sample distributions were slight, this differential bias should also be slight.

The interaction between time-in-sample and reference period length is another important factor to be considered, since shortening the reference period may aggravate time-in-sample bias.

The regular NCS bounding procedure was employed in all three treatment groups. However, as discussed earlier, this procedure may not prevent all external telescoping. In addition, some individuals or households will receive unbounded interviews. For example, households moving into a sample unit will receive an unbounded interview in their first enumeration in the NCS. So too will households in units added to update the sample for new construction.

The effect of unbounded data is to inflate the victimization rates. The proportion of bounded interviews decreases as reference period is lengthened. For example, about 91 percent of the households in the 3-month treatment group received bounded interviews versus about 86 perent in the 6-month group and about 78 percent in the 12-month group. This inverse relationship between bounded data and length of reference means that fewer interviews are bounded in the group receiving the longer reference period treatment. Because fewer interviews are bounded in the longer reference treatment, external telescoping from outside the reference period is more likely to occur, causing a greater inflation in reported victimization rates. This makes comparisons between treatments more conservation when reported victimization rates are expected to be higher under the shorter reference period.

Clerical errors in the New York and Denver regional offices resulted in improper assignment of some units to treatment groups. We deleted all affected units so that only comparable data were tabulated for both treatments analyzed. As a result we excluded about 0.7 percent of the total sample from the 3-month versus 6-month comparisons and about 1.1 percent of the sample from the 6-month versus 12-month comparisons.

As mentioned earlier, some data were reused in forming the individual annual estimators. However, the resulting correlations were accounted for in the variance computations.

In this analysis we have made the assumption that data collected from the experimental subsamples are not correlated with data collected from the control group. Since units in the experimental subsamples are returned to the control group after receiving the experimental treatment, this will not be strictly true if there is any correlation from one interview to the next for the same group of sample units. Recent unpublished research by Census indicates there may be some slight between-interview correlation. However, the correlation affects only a very small part of the control group sample (less than 8 percent), so the effects of such a correlation can probably be ignored.

Series crimes consist of several similar or related incidents which occur during the same reference period. Because the respondent is unable to separate month of occurrence and other details of individual incidents in the series, series crimes are not tabulated with regular NCS data, but appear in separate tabulations.

We have also excluded series crimes from the analyses of this experiments. It is, however, likely that length of reference period has some effect on series crimes. With a shorter reference period any "series" of incidents is also likely to be shorter, perhaps short enough so that the details of individual incidents could be recalled. In some cases, this may eliminate the series altogether. Crimes which would have been reported as series under a longer reference period may be reported as individual incidents under the shorter period, thus increasing the reported victimization rate. The data in this report include any effect of converting potential series incidents into individual incidents. The extent to which this has occurred is not currently available. Future analyses of the RPR data may shed some light on possible reductions in series reports brought about by a shortened reference period.

SUMMARY

There is strong evidence that the levels of reported victimization rates decrease seriously as reference period length increases from 3 months to 6 months to 12 months. The additional bias caused by this recall loss appears to be a much more serious source of error than sampling variability for estimated victimization rates.

Another advantage of a shorter reference period would be more timely publishing of annual victimization data. The 3-month reference period allows annual estimates to be compued as soon as data from the March interview of the following year are processed; with a 6-month reference period the annual estimate cannot be computed until data from the June interview of the following year are processed.

Disadvantages include the fact that for the same interviewing costs, the variance on the annual estimates would about double (since the effective estimates were being produced. It may be difficult to explain to data users that, despite higher variances, the estimates produced are now more accurate.

Some unanswered questions require further investigation: What time-in-sample biases can be expected after repeated interviewing with a 3-month reference period? What is the optimum number of times to interview sample units with a 3-month reference period? How much will sampling costs increase, since using a 3-month reference period in a rotating panel design may "use up" sample faster than the 6-month reference period? What is the effect of length of reference on estimates of year-to-year change?

Finally, there appears to be little to recommend a change from the current 6-month reference period to a 12-month reference period. Timeliness of estimates would be reduced. More importantly, the accuracy of the estimates would probably be reduced by much more than sampling variability would be decreased.

Table 1 -- Comparison of Victimization Rates Obtained for Reference Period Treatments

Type of Crime	Estimated Victimization Rate1/ by Treatment		Estimated Difference in Rates	Standard Error on Estimated Difference	Estimated Square of Difference	Estimated Variance on Annual Rate Using 6-month Treatment
	6-Month (θ_6)	3-Month (θ_3)	($\theta_6-\theta_3$)		$(\theta_6-\theta_3)^2$	$\sigma^2(\hat{\theta}_6)$
Total Personal Crimes	12.85	15.49	-2.64 *	0.57	6.65	0.03
Crimes of Violence	3.46	4.29	-0.83 *	0.30	0.60	0.01
Crimes of Theft	9.39	11.20	-1.81 *	0.49	3.04	0.02
Total Household Crimes	23.00	26.83	-3.83 *	1.10	13.46	0.10
Burglary	8.53	9.68	-1.15 +	0.68	0.86	0.04
Household Larceny	12.70	15.09	-2.39 *	0.83	5.02	0.06
Auto Theft	1.78	2.07	-0.29	0.33	0	0.01
	12-Month (θ_{12})	3-Month (θ_6)	($\theta_{12}-\theta_6$)		$(\theta_{12}-\theta_6)^2$	$\sigma^2(\hat{\theta}_6)$
Total Personal Crimes	11.20	12.91	-1.71 *	0.27	2.85	0.03
Crimes of Violence	3.19	3.44	-0.25 +	0.13	0.04	0.01
Crimes of Theft	8.01	9.47	-1.46 *	0.23	2.08	0.02
Total Household Crimes	19.86	22.75	-2.89 *	0.51	8.10	0.10
Burglary	7.34	8.52	-1.18 *	0.33	1.28	0.04
Household Larceny	11.04	12.46	-1.42 *	0.40	1.86	0.06
Auto Theft	1.49	1.77	-0.28 +	0.16	0.06	0.01

1/ Victimization rate per 100 persons 12 or more year old or per 100 households.
* Significant at 5 percent alpha level.
+ Significant at 10 percent alpha level.

Table 2 -- Noninterview Rates for Reference Period Treatments

	Rate by Treatment		Difference in Rates[2/]	Standard Error of Difference
	3-Month	6-Month	(for Interviews conducted April 1978-December 1979)	
	Noninterview Rates			
Type A (by reason)[1/]	4.2%	4.2%	0.0%	0.2
Total Type A	100.0%	100.0%	-	-
No One Home	17.9%	18.2%	-0.3%	1.7
Temporarily Absent	20.7%	19.9%	0.7%	1.8
Refused	55.6%	54.3%	1.3%	2.2
Other	5.8%	7.6%	-1.8%	1.0
Type Z	2.4%	2.4%	0.1	0.1
	Respondent Type			
Self-response	93.5%	93.3%	0.2	0.2
Proxy response	6.5%	6.7%	-0.2	0.2
	Interview Mode			
Personal Visit	78.5%	78.4%	0.1	0.2
Telephone	21.5%	21.6%	-0.1	0.2
	Coverage Within Interviewed Households			
Persons per Household	2.22	2.24		
	6-Month	12-Month	(for Interviews conducted October 1978-December 1979)	
	Noninterview Rates			
Type A (by reasons)[1/]	4.1%	4.2%	-0.2	0.2
Total Type A	100.0%	100.0%	-	-
No One Home	17.8%	17.3%	0.5	2.0
Temporarily Absent	18.6%	18.0%	0.6	2.0
Refused	55.7%	55.7%	0.0	2.6
Other	7.9%	9.0%	1.1	1.5
Type Z	2.4%	2.2%	0.1	0.1
	Respondent Type			
Self-response	93.4%	93.5%	-0.1	0.2
Proxy response	6.6%	6.5%	0.1	0.2
	Interview Mode			
Personal Visit	78.4%	78.8%	-0.4	0.3
Telephone	21.6%	21.2%	0.4	0.3
	Coverage Within Interviewed Households			
Persons per Household	2.23	2.21		

1/ Type A noninterview rate is computed as ratio of total type A noninterviews to the sum of interviews plus type A noninterviews.

2/ Differences shown here may not equal differences between percentages, due to rounding.

REFERENCES

(1) Waksberg, Joseph, and Neter, John, "A Study of Response Errors in Expenditures Data from Household Surveys," Journal of the American Statistical Association, 1964, 59, pp. 18-55.

(2) Murphy, Linda R., and Cowan, Charles D., "Effects of Bounding on Telescoping in the National Crime Survey," American Statistical Association, Proceedings of the Social Statistics Section, Part II, 1976, pp. 633-638.

(3) San Jose Methods Test of Known Crime Victims (Statistics Technical Report No. 1) United States Department of Justice LEAA, National Institute of Law Enforcement and Criminal Justice, Statistics Division, June 1972, Washington, D.C.

(4) Woltman, Henry, and Bushery, John, "A Panel Bias Study in the National Crime Survey." Presented at the 1975 annual meeting of the ASA, Atlanta, Georgia, August 25-28.

(5) Woltman, Henry F., Turner, Anthony G., and Bushery, John M. (1980), "A Comparison of Three Mixed-Mode Interviewing Procedures in the National Crime Survey." Journal of the American Statistical Association, 75, pp. 534-543.