ALTERNATIVE REFERENCE PERIODS FOR THE NATIONAL CRIME SURVEY

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I. INTRODUCTION

In making specific sample design decisions, there are usually tradeoffs between bias and variance and among different survey objectives, e.g., decision A will reduce the bias for statistic #1 but decision B will reduce the variance for statistic #2. Usually objectives are not well specified, priorities among objectives are vague, and little is known about bias. As a result, decisions are not made in a careful and rational manner, and bias and variance are rarely combined into mean square error estimates. In the above example, decision A might be made because of a general feeling that the bias in statistic 1 would be excessive if decision B were made, without careful thought about what statistic 1 is needed for or how important it is. Or, conversely, decision B might be made because minimizing the variance of statistic 2 is deemed important, without much thought about the bias effects on statistic 1 or the relative importance of the two statistics.

In this paper, we approach a particular decision in what we hope is a careful and rational manner. Specifically, we are concerned here with determining what length reference period to use in the redesigned National Crime Survey (NCS). Since we have the usual problems of multiple objectives, uncertain priorities among them, and incomplete bias information, we do not come to a final decision here. We do, however, specify five alternative decisions and how these would impact each of the three primary objectives. Two of the plans are unusual in that they feature a mixture of reference periods. We believe that this paper will be helpful to statisticians in their approach to design and estimation decisions in which tradeoffs between bias and variance and among objectives exist.

As part of making these decisions, we have obtained new data about the effect of reference period length on NCS estimates. Specific areas where more information is needed before an informed decision can be made are also identified.

Since 1972 the NCS has provided information on various types of criminal victimizations in the United States. The survey was designed to satisfy the demand for complete and accurate statistical data about crime based on information from victims. The main emphasis was on measuring national levels of crime and obtaining details on the characteristics of victimizations. Several methodological features of the NCS were developed to help in meeting these objectives. One such feature was the adoption of the present 6-month reference period rather than a longer period [1, 2, 3]. As part of the overall redesign effort for the NCS, it was decided to assess whether continued use of the 6-month period, or a change to a different reference period, would be better for the future demands of the survey.

The reference period directly affects the achievement of survey objectives. Depending on the exact survey objectives and their priorities, the reference period could be tailored in such a way that the survey results would most efficiently reflect them. It is our understanding that the following are the three primary objectives of the NCS:

Objective A. Obtaining an accurate measure of the amount and kinds of crime in the United States.

Objective B. Timely production of an index of change in the level of crime.

Objective C. Comparison of victimization rates between demographic subgroups, and other similar detailed comparisons of crime levels.

The relative priority of these objectives is difficult to determine. The priority changes over time. Early in the survey, objective A was paramount, but more recently objective B seems to be the most important. In evaluating reference period alternatives, the discussion of the tradeoffs between them focused on the following four issues:

1. Amount of recall bias in estimates of level
2. Amount of recall bias in estimates of change
3. Variance
4. Cost

These tradeoffs will be explained in greater detail in later sections of this paper.

The new data presented here are primarily additional tabulations of data collected for an earlier study on reference period length in the NCS, the reference period research (RPR) study. A previous paper by John Bushery, "Recall Biases for Different Reference Periods in the National Crime Survey," [5] presents the findings of the RPR study. The analyses of the new data are concerned with areas not covered in the earlier study. Throughout the rest of this paper, the examination of the additional data from the RPR study is referred to as the follow-up analysis.

The analysis in both cases evaluated the effectiveness of the current 6-month reference period relative to 3-month and 12-month reference periods. The RPR study was concerned primarily with the amount of recall loss bias (see Section II) in estimates of crime levels for the total population. The follow-up analysis expanded the examination to include estimates of change and estimates of level for the following population subgroups: blacks, males, females and several age classifications.

The remainder of the paper presents the approach we used in trying to make a decision on the NCS reference period length. Section II defines the major sources of bias with which we are concerned in this paper. Section III describes the experimental design of the study, while Section IV presents how we constructed our estimates. Section V discusses our findings on the effect of reference period length on NCS estimates. Section VI describes how well alternative reference period plans meet various NCS objectives, taking into account the bias, variance, cost and operational efficiency of each plan.

In our deliberations, we only considered cer-

tain combinations of the 3-month, 6-month, and 12-month periods for which we have data. Reference periods of other lengths have been discussed elsewhere. We do not discuss these alternatives since we have no information on their properties.

II. RESPONSE BIAS ASSOCIATED WITH REFERENCE PERIOD LENGTH

Of interest when evaluating reference period alternatives are the problems associated with respondent recall. Survey results from the NCS show that the reported rate of victimizations declines as the time lag between the interview and the reported month of occurrence increases. This phenomenon is called the recency effect. As shown in the chart below, the recency effect is made up of several components.
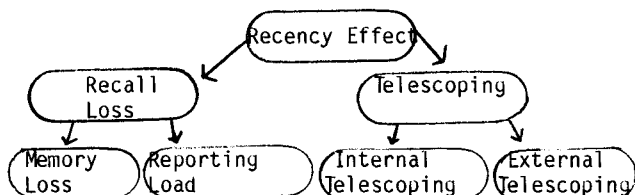
Recall loss describes the reduction in reporting levels caused by increasing the length of the reference period. This component is broken down further into memory loss and the reporting load. Memory loss is thought to occur when the respondent completely forgets an event because it is too far from the time of interview. It depends on the length of time between the interview and the month of occurrence. As we use the term it does not affect recall in the period immediately prior to the interview, but pertains to the relative decrease in reporting as the time lag increases. Reporting load is thought to increase directly with the ˆreference period length, and reduces the report throughout the period. It might be due to ᵤ. desire of respondents to shorten the interview by withholding crimes throughout the interview.

Telescoping results when the respondent remembers and recounts an event, but incorrectly reports the event as having occurred either earlier or later than it actually occurred. Internal telescoping occurs when a respondent correctly places an event in the reference period, but incorrectly reports the actual time of occurrence within the period. This does not affect the overall reported levels of crime, but still can introduce error into data collected for a particular time period. External telescoping results when the respondent incorrectly reports an event as having occurred in the reference period, when it actually occurred either before or after.

This type of telescoping is partially controlled in the NCS by a procedure known as "bounding." The procedure is designed to prevent the reporting of the same incidents in consecutive reference periods by eliminating in the present interview reports of incidents which were reported in the previous interview.

(The above discussion provides an oversimplified view of the recency effect and is intended only as an aid in identifying some of the prob-

lems related to respondent recall. Our terminology is meant to be consistent with the usage in [10] and [11] and relates to the effects measured by the reference period research. It is not necessarily consistent with the usage of cognitive psychology.)

Other effects linked to reference period length include the time-in-sample bias (i.e., change in response patterns caused by repeated interviewing of the same household) and the rate of noninterview, but little evidence is available about them. Compared to the problems with respondent recall, these effects are probably of only minor concern.

The RPR study only examined the total recall loss bias when examining alternatives. The study originally considered providing separate estimates of the effects of memory loss and the reporting load in the NCS in addition to comparing the overall mean square error (MSE) using different reference periods. However, operational constraints prevented the design of an experiment to get around the confounding of these effects. Analysis of internal telescoping was also deferred because of this problem.

In the follow-up analysis, we did not separately estimate the effects of memory loss and reporting load when examining the recall loss effect for level estimates. This is not to suggest that we considered these separate effects as uninteresting. Rather, for our purposes, we were more interested in the magnitude of overall recall loss bias on NCS estimates rather than isolating the components of the bias.

There is little evidence about how these biases affect estimates of year-to-year relative change. It has usually been assumed that the percentage bias for level estimates is fairly constant so that there is much less bias for estimates of change than for estimates of level. In fact, one reason for using a regular rotation pattern like the NCS is to eliminate bias in year-to-year change. However, there is no evidence on whether this bias really is eliminated for change estimates; the data in this paper are inconclusive. In this paper it is assumed that there may be a bias for estimates of change, but that it is much less important than the bias in level estimates.

III. EXPERIMENTAL DESIGN OF THE RPR STUDY

Twelve mutually exclusive subsamples of the regular NCS sample received the 3-month reference period treatment. Each subsample received two consecutive 3-month interviews before being returned to the regular NCS sample (i.e., 6-month reference period) for the remainder of its time in sample. Fifteen other mutually exclusive subsamples were selected for the 12-month reference period treatment. Each of these subsamples received one interview under a 12-month reference period, after which it returned to the regular sample for a given month, while the 3-month and sample for the remainder of its participation in the NCS. Each of these twenty-seven subsamples comprised one-twelfth of a regular monthly NCS sample. Thus, the 6-month reference period group consisted of about five-sixths of the regular NCS 12-month treatments each consisted of about one-twelfth of the sample.

The experimental design minimized the length

of participation of a subsample in the 3-month
and 12-month treatments. This was done in an
attempt to minimize the differences between the
experimental reference periods and the 6-month
reference period in the distribution by time-in-
sample. The 3-month groups end up with one more
interview than the 6-month groups, and the 12-
month groups with one less. Large differences
in the time-in-sample disbributions between the
experimental treatments and the control group
would have confounded analysis of the results.

More details on the design of the study can
be found in [5] and [16].

IV. CONSTRUCTION OF ESTIMATES
The data from the experimental subsamples were
combined to form collection quarter estimates.
A collection quarter consists of 3 consecutive
months of interview and forms one of the 4 quar-
ters quarters of the year (e.g. QI = January-
March). Separate collection quarter estimates
were created for each treatment. For the 3-month
treatment, each month in the quarter contained
information from respondents who were questioned
about crimes which occurred in the 3-month period
prior to the month of interview. A similar sit-
uation exists for collection quarter estimates
from the 6-month and 12-month treatments.

The original RPR study combined collection
quarters to form annual estimates similar to
those published for the NCS. The follow-up a-
nalysis examined various individual collection
quarter and averages of collection quarter es-
timates.
    A. Estimates of Levels
Comparisons of estimates of levels from the 3
treatments covered identical time periods. In
the 3-month versus 6-month comparison, two col-
lection quarter estimates from the 3-month treat-
ment were combined, and compared to a single
collection quarter estimate from the 6-month
treatment. (Note that the time period covered
in this comparison was of length 8 months.) A
similar situation occurs in the 6-month versus
12-month comparison. In all, 6 comparisons of
the 3-month versus 6-month treatments and 5 com-
parisons of the 6-month versus 12-month treat-
ments were created. The estimates used in the
comparisons between treatments are the averages
of the 5 (or 6) comparisons described above.
The comparisons are not all independent since
there is some overlap in the 3-month and 6-month
interviews used for different comparisons.
    B. Estimates of Change
By using data tabulated by collection quarter,
it was possible in the 3-month versus 6-month
comparison to examine estimates of change based
on 6 months of data which occurred one year apart,
(e.g. the change between January-June 1978 and
January-June 1979). These estimates are not e-
quivalent to the annual change estimates produced
in the survey, but they do provide some general
indication of the effect of reference period
length on year-to-year change estimates. The
construction of these estimates can be found in
Table 3 in the appendix.

Unfortunately, the change estimates from each
treatment were too variable to be useful. As
such, no conclusion is made here as to the effect
of reference period length on estimates of change.

This is one major area where more information is
needed.
    C. Estimates of Variance
The variances used in making comparisons were
computed using the approximation normally used
by the Census Bureau for the NCS. Factors which
account for a design effect and the correlation
from one interview to the next for the same
sample units were used in the calculation. Com-
parisons in this paper were tested for signifi-
cance at the .05 level.

V. RESULTS FROM THE FOLLOW-UP ANALYSIS
The tables in the appendix present the find-
ings relating to the comparisons of the 3 treat-
ments for the general population and various
population subgroups. Dicussion of the results
here is limited to the following major crime
categories: total personal crimes, crimes of
violence, crimes of theft, total household
crimes, burglary, household larceny and auto
theft.
    A. Comparison of Victimization Rates in 3-
Month and 6-Month Treatments
            1. Estimates of Level
                a. Total Population
Victimization rates were reported in the 3-
month treatment at significantly higher levels
than in the 6-month treatment for all major
crimes except burglary and auto theft.
                b. Blacks
Victimization rates for blacks were reported
in the 3-month treatment at significantly higher
levels than in the 6-month treatment for total
personal crimes and crimes of violence.
                c. Sex
For both males and females, victimization
rates in the 3-month treatment were reported at
significantly higher levels than in the 6-month
treatment for the three crimes examined: total
personal crimes, crimes of violence, and crimes
of theft.
                d. Age Classifications
Victimization rates for persons age 12-24 were
significantly higher in the 3-month treatment than
in the 6-month treatment for the three crimes ex-
amined: total personal crimes, crimes of violence
and crimes of theft. No significant difference
was found between the two treatments for persons
age 25-49 and age 50+.
            2. Comparison Between Population Sub-
                groups
To examine whether the recall loss bias dis-
torts comparisons between subgroups, we compared
the difference of the differences between the 3-
month and 6-month treatments among the different
subgroups. The findings show that:
                a. The difference between the 3-month
and 6-month treatments for blacks was significant-
ly higher than the difference reported by the
total population for crimes of violence. This
means that the comparison of blacks and the total
population is distorted by reference period
length.
                b. Except for crimes of violence for
persons age 25-49, the differences between the
two treatments for persons age 12-24 were signif-
icantly higher than the other two age classifica-
tions. This means that the comparison of age
groups is also distorted by reference period
length.

c. No significant difference was found between the differences for males and females between the two treatments.

B. Comparison of Victimization Rates in 6-Month and 12-Month Treatments

The examination of the victimization rates between the two treatments was limited to the total population. Victimization rates were reported by the total population in the 6-month treatment at significantly higher levels than in the 12-month treatment for all crimes examined except crimes of violence.

C. Comparison of Year-to-Year Change Estimates in 3-Month and 6-Month Treatments

The examination of year-to-year change estimates was conducted for the total population, blacks, males, and females. The change estimates from both treatments were very variable, and as such we do not make any conclusive statements concerning the effect of reference period length on change estimates.

VI. FIVE ALTERNATIVE PLANS FOR NCS REFERENCE PERIOD LENGTH

Presented below are five reference period plans for the NCS. We draw conclusions here about how well various plans would satisfy the primary objectives of the NCS. Note that two of the plans are unusual in that they feature a mixture of reference periods. Discussion of the plans is made relative to the present 6-month reference period. The following plans are discussed.

A. 6-month
B. 3-month
C. 3-month/6-month
D. 12-month
E. 3-month/12-month

A. The 6-Month Plan

The current 6-month reference period has served NCS adequately for almost ten years, but there are some problems. One major complaint has been about the delay in publication due to the length of the reference period. Another is on a history of questions about data quality due to what appears to be a high level of recall loss bias using the 6-month period. Findings in the RPR study and follow-up analysis show that the 6-month reference period has lower reported victimization rates than a 3-month period. To date, there is no evidence of bias in estimates of change, but this is a possibility.

B. The 3-Month Plan

This plan would reduce the bias over the present reference period at the expense of a large increase in variance.

Findings from the RPR study suggest based on a simple mean squared error calculation, that the 3-month reference period gives a better MSE for estimates of level than does a 6-month reference period. (This assumes that the actual crime rate is at least as high as the 3-month rate.) This is in spite of the fact that, within the present NCS budget, using a 3-month period would roughly double the sampling variance. This higher variance would have an adverse effect on estimates of change. One major advantage of this plan over the present 6-month plan is that more timely release of level estimates is possible.

C. The 3-Month/6-Month Plan

This plan allows measurement of (and adjustment for) the bias, with only a small increase in variance. In this plan, the first bounded interview would use a 3-month reference period. The five subsequent interviews would use a 6-month period. The overall sampling variance would increase somewhat. The data from the 3-month interview could be used to produce estimates which would be less biased than those from the full sample. The 3-month data thus could be used to estimate the relative bias in estimates from the full sample, and, perhaps, used to "calibrate" the estimates from the full sample. (However, any such adjustment for bias increases the variance of the estimates, because of the variance in the adjustment factor itself.) After one year's data, a fairly good estimate of this bias could be made for major crime items. Several years' data would need to be accumulated to obtain good reliability for level estimates using only the 3-month interviews.

D. The 12-Month Plan

Compared to the present 6-month reference period, this plan would increase the bias (and the MSE) on estimates of level and would not allow any measurement of the bias, but it would lead to substantial variance improvements which could lead to cost savings.

The plan seems questionable as a response to a history of questions about data quality because of recall loss bias. Use of a 12-month reference period will lead to a greater delay in publication of final estimates.

In spite of this, if cost savings become of primary concern and some sacrifice of data quality is acceptable, a 12-month reference period warrants consideration. However, the 12-month plan shares some operational problems with the 3-month/12-month plan discussed below.

E. The 3-Month/12-Month Plan

This plan increases the bias for some estimates but allows it to be measured and adjusted for. It also would lead to substantial improvements in in variance which could lead to cost savings. The overall recall loss bias picture is arguably as good as or better than the present plan, but the cost of maintaining the present variance is reduced.

However, there will be no way to correct for the increased bias during the period until enough 3-month data have been accumulated to permit measurement and adjustment for the bias.

This plan would cause serious operational problems which would make it very difficult to implement. If the plan is gradually phased in, a long period (up to five years) of drastically fluctuating monthly interviewer workloads would result. If an abrupt phase-in plan is used in all sample areas, it will be five years before the sample is fully aged. Even if the operational problems can be lived with, they may adversely affect the quality of the data.

VII. CONCLUSION

This paper has identified issues that need to be addressed when deciding whether the present 6-month reference period, or some other period, should be used in the redesigned NCS sample. The decision is complex, and involves tradeoffs be-

tween the variance, cost, and the amount of bias for estimates of level and change, and how these relate to the survey objectives.

With regard to statistical issues, factual evidence is available concerning the variance, cost and the amount of the bias for level estimates, but information is needed on the bias for change estimates before deciding on the reference period. To obtain this information a research project would need to compare estimates of change using different reference periods. Though we have no evidence to the contrary, we feel the bias on change estimates is much less severe than estimates of levels.

A definitive statement is needed on the future objectives of the NCS and their relative priority. In particular, knowledge of the importance of estimates of change and level, as well as the desired reliability and cost is needed. If estimates of level are most important, then clearly the best option among the five alternative plans is the 3-month plan. If estimates of level and change are of about equal importance, the 6-month and 12-month plans are clearly to be avoided, but it is uncertain whether the 3-month plan or one of the mixed plans is best. If estimates of level are of little or no interest, the 6-month and 12-month plans are probably the best choices, but there remain unanswered questions which make a decision difficult.

Once these basic questions concerning statistical issues and the priority of the objectives are answered, an informed decision can be made about the reference period. This could ultimately result in a reference period which would more efficiently reflect the objectives of the survey and balance the tradeoffs. A reference period adapted to the primary objectives of the survey could result in some or all of the following benefits to the survey: substantial cost savings, increased reliability, and/or greater operational efficiency.

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Acknowledgements

The following are a few selected tables from the original paper.

Table 1

Comparison of Victimization Rates Obtained Using the
3-Month and 6-Month Reference Periods

Total Population

Type of Crime	Victimization Rate + (per 100) 6-Month Reference (S.E.)	3-Month Reference (S.E.)	Difference 6-Month Minus 3-Month (S.E.)	Difference ÷ S.E. of Difference	Percent Relative Difference $\frac{6-3}{3}$ X 100
Total Personal Crimes	6.39 (0.058)	7.82 (0.271)	-1.43 (0.277)	-5.16*	-18%
Crimes of Violence	1.72 (0.030)	2.17 (0.142)	-0.45 (0.145)	-3.10*	-21%
Robbery	0.30 (0.012)	0.34 (0.053)	-0.04 (0.054)	-0.74	-12%
Assault	1.37 (0.027)	1.76 (0.128)	-0.39 (0.131)	-2.98*	-22%
Aggravated	0.50 (0.016)	0.67 (0.079)	-0.17 (0.081)	-2.10*	-25%
Simple	0.87 (0.021)	1.10 (0.101)	-0.23 (0.103)	-2.23*	-21%
Crimes of Theft	4.67 (0.049)	5.56 (0.226)	-0.89 (0.231)	-3.85*	-16%
Total Household Crimes	11.68 (0.112)	13.50 (0.519)	-1.82 (0.531)	-3.43*	-13%
Burglary	4.31 (0.069)	4.84 (0.313)	-0.53 (0.321)	-1.65	-11%
Household Larceny	6.49 (0.084)	7.68 (0.392)	-1.19 (0.401)	-2.97*	-15%
< $50	3.56 (0.063)	4.31 (0.296)	-0.75 (0.303)	-2.48*	-17%
> $50	2.18 (0.050)	2.23 (0.214)	-0.05 (0.220)	-0.23	-2%
Auto Theft	0.88 (0.031)	0.99 (0.141)	-0.11 (0.144)	-0.76	-11%

Table 2

Comparison of the Victimization Rate Differences Between the
3-Month and 6-Month Reference Periods

Total Population Difference vs. Black Difference

Type of Crime	Difference Between Victimization Rates + (6-Month Minus 3-Month) Total Population (S.E.)	Blacks (S.E.)	Difference Total Population Minus Blacks (S.E.)	Difference S.E. of Difference
Total Personal Crimes	-1.43 (0.277)	-2.55 (0.870)	1.12 (0.913)	1.23
Crimes of Violence	-0.45 (0.145)	-1.65 (0.555)	1.20 (0.574)	2.09*
Robbery	-0.04 (0.054)	-0.58 (0.329)	0.54 (0.333)	1.62
Assault	-0.39 (0.131)	-1.11 (0.461)	0.72 (0.479)	1.50
Aggravated	-0.17 (0.081)	-0.89 (0.356)	0.72 (0.365)	1.97*
Simple	-0.23 (0.103)	-0.21 (0.290)	-0.02 (0.308)	-0.06
Crimes of Theft	-0.89 (0.231)	-0.91 (0.666)	0.02 (0.705)	0.03
Total Household Crimes	-1.82 (0.531)	-0.75 (1.631)	-1.07 (1.715)	-0.62
Burglary	-0.53 (0.321)	-1.45 (1.180)	0.92 (1.223)	0.75
Household Larceny	-1.19 (0.401)	0.50 (1.053)	-1.69 (1.127)	-1.50

* Significant at 5% level

+ One-half of annual level victimization rate

Table 4

Relative Crime Rates+ for Various Population Subgroups
Using Different Reference Periods

Type of Crime	Black (3) Total (3)	Black (6) Total (6)	Male (3) Female (3)	Male (6) Female (6)	Age 12-24 (3) Age 25-49 (3)	Age 12-24 (6) Age 25-49 (6)	Age 12-24 (3) Age 50+ (3)	Age 12-24 (6) Age 50+ (6)	Age 25-49 (3) Age 50+ (3)	Age 25-49 (6) Age 50+ (6)
Total Personal Crimes	1.14	1.00	1.34	1.35	1.87	1.65	4.58	4.25	2.45	2.58
Crimes of Violence	1.71	1.19	1.83	1.96	2.27	2.14	7.86	7.16	3.46	3.35
Crimes of Theft	0.94	0.93	1.19	1.18	1.72	1.49	3.84	3.59	2.23	2.41
Total Household Crimes	1.04	1.14	-	-	-	-	-	-	-	-
Burglary	1.49	1.33	-	-	-	-	-	-	-	-
Household Larceny	0.78	1.00	-	-	-	-	-	-	-	-

+ One-half of annual level victimization rate