

A LONGITUDINAL ANALYSIS OF BOUNDING, RESPONDENT CONDITIONING AND
MOBILITY AS SOURCES OF PANEL BIAS IN THE NATIONAL CRIME SURVEY

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Introduction

Bounding

The National Crime Survey (NCS) was designed to provide cross-sectional national data on victimization by crime. It employs a longitudinal panel design of seven successive interviews¹ at six-month intervals of each panel in its samples. A key reason for adopting this design was the finding in methodological testing preliminary to the survey that there was pronounced tendency for respondents to report incidents as having occurred within the six-month reference period that actually had occurred prior to it, i.e., "telescoping in." A panel design permits controlling this behavior by bounding the data in that each interview affords a bound for the data collected at the next one. Because of the importance of employing this control against telescoping, the NCS makes no use for data purposes of the first interviews with housing units of rotation groups in sample for the first time (Dodge and Turner, 1971 [1981]). About one-seventh of the data the NCS collects is, therefore, wasted insofar as use for the incidence estimates is concerned. Considerable interest exists in being able to avoid this expensive control procedure and to put first interview data to use. The possibilities of employing post facto statistical adjustments for "telescoping" or some collection procedure that would substitute for the bounding control have been given serious consideration. One of the purposes of the present inquiry is to illuminate these possibilities.

The bounding control employed by the NCS is itself an imperfect one, however. Only the first interviews of the rotation group are not used for estimates--data from all other unbounded interviews are so employed.² The basic sampling unit of the NCS is the housing unit. Interviews are attempted with all persons 14 years of age or over in each sampled household and proxy data are collected for 12- and 13-year-old children in the household. There can be change in all or some of the persons occupying a unit at successive interviews. "Telescoping" in interviews with new occupants is not at all affected by a bounding control, in the case of instances of purely individual victimization. Victimization affecting the entire household or several members of the household are also unbounded when the entire household at an interview is a replacement household. When there has been some change in the composition of a household between interviews, the "telescoped in" report of a household or plural victim incident by a new occupant may or may not match an event reported at the prior interview by another affected member of the household. The bounding control also can be ineffective because there can be missed interviews for persons continually occupying a premises across successive interviews. New sample is also added periodically to the NCS, principally to adjust the sample for new construction. Units created by conversion of existing structures also can receive first interviews when the rotation group to which they belong is receiving some interview other than the first.

Most failures of the bounding control come about because of respondent mobility. We estimate that about 18 percent of the NCS interviews contributing to estimates during the 1975-1978 period was unbounded. About 15 percent was unbounded because of respondent mobility. The remaining 3 percent was due to the failure of interviewers to complete an interview with respondents known to reside in the housing unit at the previous time in sample.

Mobility and Other Sources of Unbounded Data

Overall, bounded interviews at times-in-sample employed for NCS estimates yield from 40-50 percent more personal victimization reports than do bounded ones. The characteristics of persons who move as individuals or as members of a moving household and who contribute unbounded data differ systematically from those who had a prior interview. The circumstances of persons at a particular time also determine whether their interviews are bounded or not. People who have just moved from one household to another, for instance, may have been at greater risk of victimization during this particular period than those in a settled state. Persons who miss an interview (for example, because they are in hospital or jail during the collection period or who are always away from home when the interviewer calls) differ from homebodies who are almost always successfully interviewed. To assess the aggregate effects of the bounding interview procedure on NCS data, one must separate the effects of bounding interviews from those of personal characteristics and life circumstances. Multivariate analyses have been applied to cross-sectional NCS data for this purpose, but bounding status is inherently subject to contamination by differences in life circumstances in these cross-sectional analyses.

Longitudinal analysis of data for identical sets of persons at successive interviews permits a nicer approach to disentangling effects of interview bounding from variables that are confounded with it in the cross-sectional approaches previously explored.

Ever since Gray (1955) identified the phenomenon that Neter and Waksberg (1964) later labelled "telescoping," responsible survey practice has had to confront this major source of response error.³ The favored, albeit usually expensive, attempted solution has been to "bound" interviews by a prior interview in a panel design. The rationale is that recorded information collected by a bounding interview at the beginning of the reference period for a subsequent interview can be used by interviewers to screen out events that might otherwise be subject to "telescoping" forward into that reference period. Informal observations of interviews in progress, however, suggest that interviewers infrequently have to employ their record of the previous interview to delete duplicate incidents. It may be, then, that prior interviews serve for "self-bounding." One possible way in which self-bounding may operate is that at any time during the process of retrieving an incident from memory, the respondent may also remember

having previously reported that incident to the survey. The bounding interview may also serve more simply as a nice anchor point for structuring (i.e., "bounding") the reference period for respondents, memory work.

The lower incident reporting rates in bounded than in unbounded interviews may also have sources other than the reduction of "external telescoping." Presumably, some decline at a second interview would be due simply to whatever factors produce the "panel bias" observed in this and other surveys--the decline with time in sample that is observed at successive interviews. These time-in-sample effects may simply be curvilinear, and it is plausible that the effect should be far greater from the first to second time-in-sample than from the second to third, just as it is much greater from the second to third than from the third to fourth.

A favored, though purely speculative explanation of the "time-in-sample panel bias" is respondent motivational decline--"respondent fatigue," loss of interest, wearing out of welcome, accumulation of burden, decay of novelty.

A more specific motivational speculation may explain the effect of bounding. The NCS employs screening questions followed up by intensive questioning about an event mentioned in the screen. The follow-up incident report questions are much more burdensome and demanding than is the screening. Those respondents who give positive answers to screen questions in the first interview learn they are letting themselves in for the burden of detailed incident reporting by so doing and, it is speculated, may develop reluctance to court this burden by giving positive screen responses when next interviewed. (Lehnen and Reiss, 1978.) We are attracted to an alternative interpretation because we find evidence that more "burdened" respondents are more inclined to report incidents than are less burdened ones. Exposure to detailed questioning about any incident mentioned in screening, we believe may change a respondent's definition of the interview. The incident questioning impresses on respondents the high concern of the survey with temporal and other accuracy and may reduce tendencies toward loose and expressive incident mentions in subsequent screening.

Because observations of the effects of bounding have been mainly based upon observations of panels as aggregates, rather than upon longitudinal data for successive interviews with the same respondents, selective panel attrition has also been advanced to explain the lower rates of interviews with bounded panels. Because panel losses are heaviest from the first to second interviews than at subsequent times, a much larger panel bias effect should be observed immediately after the bounding interview than at later times in sample.

Mobility

The present analysis is concerned with relations of respondent residential mobility to victimization recounting in the NCS, particularly as mobility is associated with differences in interview treatments that are known to influence recounting.

Major features of the design of the NCS reflect concern with a few sources of measurement error, in addition to bounding failures:

1. A short and constant reference period is used because of strong evidence of recall decay.

2. A panel design follows from the requirement for a bounding interview, but panel bias (presumed effects of repeated exposure to interviews and of cumulative interview burden) limits the numbers of times persons can be retained in sample.

3. The need for balance of panels' time-in-sample is reflected in the rotating panel structure of the design.

4. In addition, for purposes of economy and coverage, the use of telephone interviews has been progressively extended in the NCS, although there has been concern with effects of victimization recounting.

5. The strong recency bias of the data is also evened out by the balanced panel structure. In-house (i.e., Census Bureau) research on nonsampling error in the NCS has largely focused on these concerns--bounding and telescoping, panel bias, recall loss, recency bias, and mode effects.

Inherent features of the NCS design also make for systematic variation of each of these sources of measurement error with respondent residential mobility. We will deal below with some of the direct relations of mobility to these error concerns.

The systematic relation of mobility to sources of error awkwardly confounds error sources with variations of major substantive concern. Time series are affected, as well as cross-sectional analyses of victimization rates by type of household, person or respondent. Recent declines in NCS victimization rates, for example, coincide with declines in rates of residential mobility. Mobility also varies with personal and household characteristics with which NCS victimization rates are strongly correlated: for example, age and size of household. There are also major substantive concerns with victimization, itself, as cause of changes of residence and the consequences of mobility for changes in victimization rates.

The importance of mobility for NCS data objectives has led the NCS Redesign Program to recommend that the NCS adopt a true longitudinal survey design, with follow-up of respondents who move from the originally sampled residential locations. A plan for a test of such design is, at time of writing, under review by the Bureau of Justice Statistics.

The present analysis has been undertaken because it is felt that proper analytic use of the data accumulated by the NCS from its inception until such point as a redesigned true-longitudinal survey may be implemented requires serious attention to the confounding of substantive and error sources of the variation of victimization recounting with mobility. To date, only one of the major methodological studies of the NCS has addressed the problem (Reiss, 1977). Mobility has not been introduced as a control variable in other studies despite many important ways in which moving affects, or is noncausally correlated with, the interview situation for a respondent. Among these relationships are the following:

1. The first interview with a new occupant of an in-sample location is unbounded

2. The mean time-in-sample for interviews with any component of the population varies as a function of its mobility.

3. When a move is that for an entire household, the first interview contact with the replace-

ment household is much less likely than for other households to be a telephone interview.

4. Persons who move in (or move out) of households as individuals are more commonly interviewed as secondary respondents than as the designated household respondent. Biderman, Cantor and Reiss (1984) have shown the same persons have much higher victimization rates when they are interviewed in the latter status than in the former.

There is particular importance for NCS rate estimates of that mobility which results in new household formations or in mergings of existing households.

The same number of occurrences of crimes such as burglaries, which are properly treated as household crimes, when spread out over a larger number of households, should yield a lower rate of household crime incidence. There is also some increase in net underreporting of such crimes due to there being fewer respondents in each household who may recall a crime that the household respondent fails to report. Many events treated by the NCS as household crimes, however, actually impact only a particular individual and such are better reported when that particular person receives the household screen questions which ask specifically for such crimes. For such events, there is more complete counting when the ratio of persons to households increases. Rates for crimes classified as larceny without contact by the NCS are particularly subject to more complete counting when household sizes are smaller, because of the effects of several procedural aspects of the NCS (Biderman, Cantor and Reiss, 1984).

New household formations are associated with rapid housing stock increases (and, presumably, doubling-up occurs when the rate of formations lags behind demographic growth). During a period of high rate of net new household formations, there would be an increase in the double-counting error of the NCS for household victimizations occurring in that portion of reference period during which members were in a prior household, provided the NCS rules were followed and all incidents were reported by those eligible to do so. During doubling-up periods, there would be a net undercount.

Relation to Broader Research Program

In order to explore the effects on NCS victimization rate measurement of such differences in collection and analysis procedures as bounding and other repeat interview contact, mode of interview, respondent status, (household or secondary respondent, and proxy or self-respondent) and household composition, we have been employing a quasi-experimental approach. This approach has involved examination of rates for sets of respondents who receive successive interviews and comparison of changes from interview-to-interview for those who receive the same or different interview treatments at successive times. We have focused particularly on quasi-experimental analyses of data for the first three interviews. Selecting quasi-experimental populations in this way affords many advantages for our analytic purposes. For the high-mobility period of the 1970's from which our data are drawn, however, the restrictions eliminate quite high proportions of all NCS samples. The stable households in which all eligible persons are successfully interviewed at all first three times in sample have low victimization rates. They con-

tribute to NCS estimates only 68 percent of the data for these times in sample. (See Biderman, Cantor and Reiss, 1982, 1984.)

For illuminating the phenomenon of "bounding," the nonmobile cases used in our prior work also provide only partial answers. We have not been able to tell from our analyses if the effects of bounding would be the same for mobile households and persons who enter NCS panels at various times-in-sample as they are for the stable components of incoming rotation groups we have previously analyzed. It is the former kinds of respondents whose unbounded data are retained for estimates. We also could not assess how the unbounded data for persons who miss an interview affect rates.

Data and Analysis

The analysis to be presented in the present paper was conducted for extending our prior quasi-experimental inquiries to address these issues. Our analysis uses respondents who were interviewed at least once during the first three times the unit in which they resided was in sample. The three-interview sequence permits analysis of bounding and other time-in-sample change in victimization rates with attention to both earlier or later mobility of the sample, and to the occurrence of interview non-completions at one or two times. Eligible units were restricted to those that: 1) entered the NCS sample between January 1975 and June 1978, 2) were not dropped in sample cuts after interviewing began for similarly located units and 3) did not participate in a Census Bureau interview methodology experiment during these times in sample.⁴ Persons who were under 12 years old at any time during these interviews were also deleted.

Data were taken from a longitudinal file built by retrospectively matching NCS interviews, from the ICPSR public use files, over the 1975-1979 time period. Because the NCS procedures were not designed with longitudinal analysis in mind, there are inconsistencies in the identifiers used to match persons and housing units across panels. Records for 7 percent of the persons in the file did not consistently meet matching criteria for all times in sample (Roistacher and Noble, 1982). This analysis does not correct for these mismatches. While no systematic analysis has been undertaken to identify how these mismatches were distributed among our study population, we presume that a large proportion of the match errors will be represented in our analysis as respondents moving out of sample.

The display of our data in Table 1 gives total aggregate victimization reporting rates for sequence groups defined as follows:

$T_1 T_2 T_3$ represents the first three times-in-sample for NCS panels. (In the tabular display and elsewhere, we will omit the subscripts where the order of the symbols indicates the time-in-sample.)

For a given respondent, we identify whether the person was

X_i = interviewed at T_i
 O_i = not interviewed at T_i .

Respondent groups without an interview at T_i will be discriminated further, as follows

N_i = not in sample or unit not eligible for interview at T_i
 Z_i = eligible but not interviewed (Type A or Type Z noninterview at T_i)

X_i will be discriminated further for T_1 and T_2 , as follows:

V_i = reported one or more personal victimizations at T_i
 U_i = did not report a personal victimization at T_i

In addition to the rate for each group at each applicable X_i , the table includes two kinds of ratio measures. The ratios $X_{pi}/X_{(p)i+k}$ ($K=1, 2$) are used as measures of change in rate with time in sample. That ratio for the first interview with any group to its next is an indicator of bounding effect. Ratios of X_2 to X_3 rates for those groups with three interviews and X_1/X_3 ratios for those with a missing T_2 interview provide change measures which afford a relative perspective for interpreting those changes associated with a bounding interview.

The second ratio measure in the table is indicative of the recency concentration in a group's rate for an X_i . It is the ratio of incidents dated to the most recent month of the reference period to those in the first two months. Telescoping is expected to affect largely incidents dated in the earliest part of a reference period. For aggregate rates, telescoping offsets the undercount of incidents due to the "forgetting" of older experiences. Unbounded interviews are found to have flatter distributions by month of reference period than do bounded interviews. But both telescoping and recall have been found to vary considerably by type of incident. There is evidence that more consequential incidents are more subject to telescoping and less subject to "forgetting." (Reiss, 1977; Biderman and Lynch, 1982.) For example, distributions within reference periods of incidents reported to the police have less recency bias than those not so reported; "completed" crimes have less recency bias than "attempts." There are differences in recency ratios for various classes of respondents, but the reasons for such differences are unclear. Differences in recall, in tendencies to "telescope" or in characteristics of experience all could be implicated. Bounded interviews have extremely high recency bias, as can be noted in the recency ratios in Table 1, the rates for the most recent single month for a bounded interview exceed those for the two earliest months, combined.

Discussion

Relation of Mobility to Victimization

Victimization is highly related to mobility. The group interviewed at each of the first three interviews (XXX) has much lower rates at any time in sample than do those who either enter or leave the sample at any point. The nonmobile groups with one or two noninterviews also have low rates. With bounding taken into account, the rates for stable groups with one noninterview are in one instance (XXZ) somewhat higher than that for XXX, and in another instance (XZX) somewhat lower. The stable groups with two missing interviews all have lower rates than the first interview rate for the XXX group. With the exception of one group with very few cases (ZNX), in-movers and out-movers that also have one noninterview have very high rates.

Victimization, itself, has predictive power for mobility. Of the groups interviewed at T_1 , rates for those who leave the sample at T_2 are about two-thirds higher than the rates for groups that remain in sample three times. T_1 rates for those who leave after T_2 are about a third higher

TABLE 1
 VICTIMIZATION RATES (PER 1,000), REGENCY RATIOS AND X_i/X_{i+K} RATIOS FOR FIRST THREE TIMES IN SAMPLE BY INTERVIEW STATUS SEQUENCE AND PRIOR VICTIMIZATION

Interview Status Sequence	N	T ₁		T ₂		T ₃			
		Rate	Recency	Rate	Recency	X ₁ /X ₂	Rate	Recency	X ₁ /X ₃
XXX	102288	152.17	0.65	104.95	1.19	1.45	95.26	1.21	1.10
VXX	12205	1275.22	0.65	233.10	1.29	5.47	206.55	1.34	1.13
VVX	2194	1432.54	0.68	1296.72	1.29	1.10	375.57	1.42	3.45
VUX	10011	1240.74	0.64	-	-	-	169.51	1.30	7.32
UXX	90084	-	-	87.58	1.15	-	80.18	1.17	1.09
UVX	6743	-	-	1169.51	1.15	-	177.96	1.11	6.57
UUX	83341	-	-	-	-	-	72.27	1.19	-
OKX	16551	-	-	198.78	0.77	-	137.03	1.17	1.45
OVX	2474	-	-	1329.43	0.77	-	284.16	1.24	4.68
OUX	14077	-	-	-	-	-	111.17	1.14	-
NXX	13655	-	-	213.04	0.79	-	144.27	1.18	1.48
NVX	2172	-	-	1338.86	0.79	-	294.66	1.22	4.54
NUX	11843	-	-	-	-	-	112.30	1.16	-
UXV	2856	-	-	131.56	0.64	-	102.90	1.15	1.28
ZXX	302	-	-	1261.59	0.64	-	208.61	1.55	6.05
ZUX	2594	-	-	-	-	-	90.59	1.07	-
XXO	16702	210.81	0.65	139.56	1.23	1.51	-	-	-
VXO	2613	1346.73	0.65	308.16	1.48	4.40	-	-	-
UXO	14089	-	-	108.67	1.12	-	-	-	-
XXN	13588	219.61	0.65	146.45	1.24	1.50	-	-	-
VXN	2193	1359.78	0.65	313.73	1.56	4.33	-	-	-
UXN	11395	-	-	114.26	1.10	-	-	-	-
XXZ	3114	172.45	0.60	109.51	1.17	1.57	-	-	-
VXZ	421	1275.53	0.60	266.03	1.08	4.79	-	-	-
UXZ	2693	-	-	85.04	1.21	-	-	-	-
XXV	4438	170.35	0.58	-	-	-	124.83	0.83	1.36
VXV	573	1319.37	0.58	-	-	-	286.21	1.05	4.61
UXV	3865	-	-	-	-	-	100.91	0.75	-
XXN	2049	191.80	0.56	-	-	-	142.02	0.87	1.35
VXN	297	1323.23	0.56	-	-	-	262.63	1.10	5.04
UXN	1752	-	-	-	-	-	121.58	0.79	-
XZX	2389	151.95	0.61	-	-	-	110.09	0.80	1.38
VZX	276	1315.22	0.61	-	-	-	311.59	1.00	4.22
UXZ	2113	-	-	-	-	-	83.77	0.71	-
XOO	24739	253.00	0.75	-	-	-	-	-	-
VXO	22447	258.34	0.76	-	-	-	-	-	-
UXO	1205	144.40	0.43	-	-	-	-	-	-
ZXN	208	201.92	0.38	-	-	-	-	-	-
ZXN	879	282.14	0.99	-	-	-	-	-	-
OXO	10314	-	-	279.33	0.84	-	-	-	-
NXN	8434	-	-	296.42	0.83	-	-	-	-
ZXZ	361	-	-	131.91	1.10	-	-	-	-
NXZ	662	-	-	252.27	0.72	-	-	-	-
ZXN	658	-	-	212.77	1.17	-	-	-	-
OOX	21801	-	-	-	-	-	240.59	0.77	-
NNX	20448	-	-	-	-	-	245.01	0.77	-
ZZX	642	-	-	-	-	-	144.86	0.77	-
NZX	454	-	-	-	-	-	211.45	0.55	-
ZXZ	257	-	-	-	-	-	167.32	1.86	-

¹ is the proximate completed interview.

KEY TO MOBILITY TABLE:

X = interviewed
 V = victimized
 U = not victimized
 O = not interviewed
 N = not in sample (mover)
 Z = Type A/Z interview

Example:

XXX = group interviewed all three times in sample

Recency: Number of incidents reported in the first month divided by the sum of the incidents in the fifth and sixth months.

than rates for the nonmobile groups.

Bounding

The familiar decrease in rates from a first to second interview is evident in the ratios of first to second interview rates ("bounding ratios") shown in Table 1 for groups interviewed at least two times. The ratio is somewhat lower for the nonmobile (XXX) group (1.45) than it is for the two pertinent mobile (NXX, XXN) groups (1.48, 1.50). The difference in bounding ratios may possibly be a function of the much higher victimization rates of the latter. The group with two successive interviews but a noninterview at T_3 (XXZ) has a very high (1.57) bounding ratio. That would be consistent with a hypothesis of declining motivation with repeated interview contacts.

From the standpoint of theories regarding the causes of the decline in rates with bounding, the most striking observations in the table are the ratios of X_1 to X_3 rates for the groups that are interviewed at T_1 and T_3 but not at T_2 . For XNX the ratio is 1.35 and for ZZX, 1.38--not much different than the bounding ratios for the groups with two consecutive interviews, which were generally around 1.40 to 1.50.

It is possible that interviewer recording or matching errors, rather than leaving and reentering sample locations, lead us to attribute Rs erroneously to the XNX pattern who actually were interviewed at T_2 . We suspect that 15 percent of XNX cases are affected by such error. As a true pattern, however, XNX should not be rare--college students and mariners, for example, would often be correctly identified this way. Furthermore, NXX and XXN sequences, for which one would expect equal proportions of errors of assignment, conform with the expected rate sequences. We would expect the ZZX sequence would be less prone to interviewer recording error than XNX because interviewers have strong reason to avoid misidentifying cases as non-interviews.

We have neglected, above, attention to the ZXX group which has a 1.28 bounding ratio, the lowest of that for any group with two interviews. We believe that the component of our noninterview category for which there is one or more interviews completed in the noninterviewed respondent's household (Type Z non-interviews), has in some degree the properties of interviewed cases insofar as bounding is concerned. It is clear in the data of Table 1 for all groups Z at any time that it is erroneous to consider such cases "bounded," however, as is done by the variable identified as "bounded," in Census analyses and in the NCS public use tapes.

Bounding and Recency

As has been noted, unbounded interviews have low recency concentration--0.72 being the median ratio for unbounded cases, with the range among the sequence groups varying from 0.38 to 1.86. Bounded interviews have recency concentrations that are usually almost twice as high, with a range from 0.80 to 1.21 (with a median of 1.18).

Inmovers have recency ratios that are relatively high for first interviews. This may be due to a tendency to interpret NCS screen questions that are phrased in terms of "this household" as referring to current household or location. As a result, persons new to a household or new at a location may omit incidents that happened at their previous residence.

The recency concentrations are much greater for bounded interviews which follow an interview in which a victimization was reported than for others. The ratios range from 1.29 to 1.48 for most of the former (see groups VXV, OVX and VXO) and 1.12 to 1.18 for the latter (see UXX, OUX, UXO). This is consistent with theories that attribute much or most of the bounding effect to previous exposure to the incident report questions.

With regard to recency, the groups with a missing X_2 fall in between the recency concentrations observed for bounded and unbounded interviews, with .83 as the mean recency for both XNX and ZZX. Again, the recency ratios are higher for such groups that have a victimization reported at T_1 than for those without prior victimization (1.05 vs .75).

Balance in Estimates and Samples

NCS rate estimation procedures clearly are subject to a bias due to the inclusion in estimates of unbounded data for respondents who are first interviewed at T_{i+1} for their panels. At T_2 and again at T_3 , in our analysis, unbounded persons make up 18 percent of our respondents. (The cases deleted from our analysis--12-year-olds and cut sample--are unbounded in much higher proportion than the cases retained in our analysis. We, therefore, are understating the importance of unbounded interviews.)

From the ratios X_i/X_{i+1} where i is the first interview for a given K , we can estimate bounding effects. Were we to adjust the rates for unbounded cases to equate them with those for bounded interviews, the total rate for our analysis population at T_2 would be reduced from 131.89 to 117.5 or 10 percent. The reduction at T_3 would be from 122.77 to 110.36 or 10 percent.

The presence of unbounded interviews at T_{i+1} is due largely to residential mobility. The mobile components of NCS panels also have much higher rates in bounded interviews, largely if not wholly, we presume, because of differences in their actual victimization experience. That the first interviews with inmovers are unbounded, we estimate, inflates the total T_2 and T_3 rate estimates by 9 percent.

Another question has also been raised as to the implicit assumption in the NCS design that the experience of those moving out of sample will be balanced by that of persons moving in during the life of panels (Biderman, 1981). It has been suggested that there may be significant imbalances in the movements of persons into and out of the NCS sampling frames: principally, migration to or from the 50 states, institutions, and military barracks. In addition to imbalance in the number of persons in these flows, the inmovers may differ from the outmovers in significant characteristics. A complication is also introduced by the fact that a portion of the reference period about which a recent inmover from out-of-frame is asked will be time prior to entering the frame. Victimizations occurring during the out-of-frame period are retained for estimates.

We can examine the degree to which rates of the first interviews of inmovers are like those of the first interviews of outmovers. Combining all groups interviewed at T_1 but not in sample at T_2 and, then, all those interviewed at T_2 but who are not in sample at T_1 , we find the outmover rates to be slightly higher--252.77 and 244.87, respectively. Presumably, the slight excess of "outmovers" in our analysis is due to the sample reductions that were made during this period.

If we wish to compare bounded inmover and outmover data, the pertinent groups are the T_3 rates for inmovers and the T_2 rates for outmovers. The outmovers again have somewhat higher rates, 146.45 as compared with 144.27 for inmovers.

The higher rates for outmovers relative to inmovers we observe is probably due to the erroneous inclusion in the latter group of nonmobile respondents who were in reduced-sample units. In sum, our evidence, which is not altogether satisfactory for the purpose, suggests that there is not great imbalance in rates of inmovers and outmovers.

Analyses by Type of Crime and Respondent Status

The analysis presented above aggregates all classes of crime and all respondents. The aggrega-

tions obscure some aspects of bounding and mobility. Certain classes of crime, which include serious incidents disproportionately have higher bounding ratios but have less recency concentration in bounded interviews. For victimizations treated as household crimes and for personal larcenies without contact, because of NCS interviewing procedures, the attribution of incidents to a particular respondent in a plural-respondent household is highly dependent on the reporting behavior of other respondents in the household. This is true particularly for secondary respondents, relative to the designated household respondent. For these classes of crime, reports given at the previous time-in-sample by another member of a household can bound an interview with a respondent who was not personally interviewed at the prior time-in-sample. Such intrahousehold respondent reporting and victimization attribution interactions are less important for assaultive crimes and thefts with contact than for the more numerous thefts without contact and household crimes. (Biderman, Cantor and Reiss, 1984.)

These intrahousehold considerations are relevant to interpretations of the data presented here, particularly for sequences involving one or more noninterviews. We have carried out disaggregated analyses which are not presented here because of limitations of space. Particularly pertinent is an analysis like that above, but limited to assaultive and other contact crimes. This analysis is consistent with all the conclusions and interpretations of the aggregate analysis presented here.

FOOTNOTES

¹During the start up years of the NCS early rotation groups stayed in sample for as many as 12 interviews.

²Almost all analyses of the effects of the bounding interview in the NCS have treated as "bounded interviews" all interviews conducted in "the same household" at the preceding time in sample; that is, if all occupants had not moved out and been replaced by others, if there had been one or more interviews completed in the household at the prior time-in-sample, and if the household was in sample at the prior time. This "household status" variable is the only means available for identifying bounding in the NCS cross-sectional public-use tapes described by the Inter-University Consortium for Political and Social Research (1981).

³Summary discussion of bounding and telescoping in surveys is given by Neter (1970) and a review focused on the NCS by Skogan (1981).

⁴These experiments include: 1) the reference period research study (Bushery, 1981) and 2) the maximum telephone procedure study (Woltman and Bushery, 1977).

⁵Among those we treat as outmovers are those respondents for whom there is no record at a particular time-in-sample because their units were cut as part of the sample reductions during their panel's time-in-sample. We could not identify such cases and we are falsely attributing mobility to an appreciable portion of the N_{i+k} cases.

⁶We also examined recency ratios constructed by splitting reference periods into two three-month segments. For the purposes of the present paper, the two measures yield the same interpretations of the data.

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