THE POLLS—A REVIEW
THE NATIONAL CRIME SURVEY REDESIGN

WESLEY G. SKOGAN

The National Crime Survey (NCS) provides estimates of the level of criminal victimization in the United States and information on the detailed characteristics of crime incidents and victims. It is sponsored by the Bureau of Justice Statistics (BJS) of the U.S. Department of Justice and conducted by the Census Bureau's Crime Surveys Branch. There are a number of interesting methodological features of the NCS, many of which are examined in a recent report on the survey from BJS. The NCS is a *retrospective* survey; like studies of voting behavior, spells of unemployment, and episodes of ill health, it poses a recall task and relies upon the accuracy with which respondents can describe their past experiences. The survey opens with a checklist designed to elicit reports of recent encounters with crime, and proceeds to a set of detailed questions for those who respond affirmatively. Most of the 18,000 or so NCS respondents each month have little to report, for recent victimization is relatively infrequent and geographically concentrated. Thus, using a national sample to ferret out these few requires that the survey be among the very *largest* conducted by the Census Bureau. Cost considerations and some technical features of measuring victimization also demand that the NCS be organized as a *panel* survey as well.

Many of the methodological problems involved in fielding large retrospective panel surveys are confounded with the topical content of the NCS, for the distribution of criminal victimization turns out to be closely linked to many of the sources of sampling and nonsampling error which affect such surveys. Recognizing this, the launch of the NCS in 1972 was preceded by a series of six pilot studies that tested alternative questionnaire strategies, respondent selection procedures, and sampling designs for the survey (reports of the studies are reprinted in Lehnen and Skogan, 1981, 1985). This methodological scrutiny continues; almost immediately after the NCS went into the field it was reviewed by a panel convened by the National Research

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Council, and BJS has made public-use data sets from the survey widely available through the University of Michigan's criminal justice data archive. The report of the National Research Council (1976), reactions to published NCS reports, and the experiences of the research community led in turn to the formation of a research consortium to consider how the NCS could be redesigned to deal with issues that became apparent once the survey was in the field. The redesign consortium issued its final report in 1986, and since then the Bureau of Justice Statistics and the Census Bureau have been considering its operational implications and testing revisions in the NCS. Some changes have already been made in the survey, and many more are in the offing. All of this is summarized in the 1989 BJS report *Redesign of the National Crime Survey;* its release provides the occasion for this review.[1]

Problems and Solutions

The redesign effort was complex, for the NCS faces a number of formidable methodological challenges. The difficulty of the enterprise was redoubled by the fact that by the time the recommendations of the redesign consortium could be considered the NCS was a mature enterprise; stakeholders in the survey feel an enormous financial and organizational commitment to the years of trend data it has generated, and they feel they have to be cautious about making changes. Although the categorization is too neat (for there are many cross-cutting issues), the NCS redesign group faced six major challenges. First, the survey produced odd findings that no one believed. Victimization rates for some key social groups looked too low, and some kinds of crime seemed to be severely underrepresented; this signaled that there might be important response biases in the survey. Second, the survey did not adequately account for repetitive events; for many policy purposes multiple victimizations are the most important crimes to understand, but the procedures employed by the survey made it unsuitable for measuring them. Third, the sample design was troubled. The NCS employs a sample of addresses; seven interviews are conducted at each address, at six-month intervals. Over this period there is a considerable amount of individual and household attrition. That attrition has significant implications for estimates of the victimization rate. In addition, the survey was probably undercounting and misclassifying a considerable number of crimes. There was also some dissatisfaction with the range

1. There is also an abbreviated version of the redesign report by Bruce Taylor of BJS (Bureau of Justice Statistics, 1989a), and a report by Catherine Whitaker on findings from the first changes in the survey (Bureau of Justice Statistics, 1989c). There is a great deal of additional material on plans for the NCS in a paper by Alexander and Taylor (1989).

of crime covered in the survey and with the absence of survey measures shedding any light on *why* people were victimized. Finally, the survey is very expensive. The nature of the survey has had to change radically over time in response to declining support for research in the federal government, and the anticipated cost of some of the recommendations of the redesign group has foreclosed their adoption at this time.

ANOMALOUS FINDINGS

The widespread suspicion that some groups of victims and some kinds of crime were underrepresented in the findings of the NCS is ironic, for the principal rationale for the survey was that *other* ways of knowing about crime are so fraught with error that researchers must go directly to victims in order to understand the true dimensions of the problem. However, when the NCS did so, few believed some of the results. In the first Washington, DC, study conducted using the new survey, rates of assault and theft were more than twice as high for affluent whites than for blacks. The second Washington survey—using a new and presumably improved methodology—found the same pattern, and in addition revealed that whites were much more likely to be robbed. In the national NCS, assault rates increased with levels of education, and those who went to college reported being assaulted at two and one-half times the rate for those with less than an eighth-grade education; most assaults involved strangers rather than acquaintances, neighbors, or relatives; and very little domestic violence was reported. All of these findings defied common criminological wisdom, and interpreters of the NCS were quick to speculate that there were substantial response biases in the survey.

Some of these anomalies were apparent even before the NCS went into the field. Part of the NCS development effort involved record-check studies in which victims sampled from police files were tracked down to determine the accuracy with which known victimizations could be elicited in interviews. These studies documented that crimes by nonstrangers were not very accurately recalled, and the recall rate for incidents involving relatives and marital partners was terrible. The NCS also gathered data on 12- and 13-year-old "respondents" by interviewing their parents; that practice has already been discontinued, leading to a jump in the rate of crime against younger people. The anomalous relationships between assault rates and race or education were less predictable before the survey began, but once these findings appeared in official reports and were frozen in the time series they proved to be very difficult to explain. Many observers suspect that they reflect differential productivity in the recall task posed by the

survey. To be sure, the anomalies are very robust; I have gathered special tabulations of victim surveys conducted in Britain, Germany, Canada, and Holland, and in every case assault rates rise with levels of education. The often-replicated finding that life is nastier and more brutish at the top of the social ladder—but not shorter, for homicide rates there are lower—presents a persistent problem for those involved in these surveys.

The solution to apparent class and culture biases in measures of victimization was to redesign the part of the NCS interview that imposes the most crucial and difficult burden on respondents, the screening questions by which victims are initially identified. The old sequence was composed of a short checklist of questions that reflected in fairly straightforward fashion the crime categories of interest in the survey. The new questionnaire is designed to put less of an "instant recall" burden on respondents; instead, they will be asked about crime in the context of their daily routines. They will be presented with a longer list of questions about things that may have happened to them while they were shopping, riding public transportation, staying around home or leaving at night, and at work or school. The interviewers will explain more fully what the survey is after, and give many examples of the kinds of events they are looking for. The screening questions are more numerous and more naturally organized. Field tests indicate that the new approach elicits about 25% more incidents. It is hoped—but it has not been demonstrated—that this everyday-life approach to the recall task will also reduce education and race differences in respondent productivity in the survey as well. A field test of the preliminary version of the new questionnaire did not increase the proportion of nonstranger and related-party incidents uncovered by the survey, one of the suspect features of the old NCS, so the production version specifically notes that "it is important to get the fact about crimes done by co-workers, friends and relatives," and asks separately about attacks or threats by four categories of friends and relatives.

REPETITIVE EVENTS

Criminal victimization does not follow a random distribution: if it did, the chances of an individual being *revictimized* would be independent of the fact that he or she had been a victim in the past. But it does not work that way; victims have a greater than chance likelihood of being victimized again, for there is a real "victim proneness" in parts of the population. Spousal and child abuse are defining instances of multiple victimization, and similar patterns can be seen which are linked to occupation, household organization, and place of residence. Repetitive victimizations are important for policy purposes because they are pre-

dictable from past reported crimes, they typically involve offenders who are immediately identifiable, intervention is possible, and they add disproportionately to the overall crime count. Even using poor measures, repeat assault victims contribute 20% of the assault measured by the NCS (Skogan, 1981: table 1). The second challenge facing the redesign group was to devise procedures which enabled the NCS to make more reasonable estimates of the extent and nature of multiple victimization.

Some of the impediments to understanding multiple victimization are deeply embedded in the structure of the survey. The NCS asks (and will ask in the future) about incidents which have occurred during the six months preceding the interview; by many definitions this is a fairly brief "window" through which to look for bouts of serious, violent revictimization. Although it is conducted as a series of reinterviews with residents of households at sample addresses, official NCS reports have largely ignored the panel design of the survey. It has been difficult and expensive to assemble the experiences of individuals over multiple waves of interviews. One of the few studies which has done so (Langan and Innes, 1986) found extremely provocative evidence for the impact of calling the police on breaking the over-time chain of domestic violence. However, assembling NCS panel data is complex and fraught with at least four more difficulties: it is difficult to untangle complex, repetitive events in the interviews; the task of doing so is plagued with large interviewer effects; matching the panel data over time runs into a severe respondent attrition problem; and it still has not been documented how effectively the new survey will elicit reports of the related-party crimes that we think should dominate repeat victimizations. The respondent attrition problem will be detailed in the next section of this review; it suffices at this point to note that the movement of individuals and entire households out of the NCS's address sample makes it difficult to assemble multi-wave data to measure multiple victimization, particularly because one of the reasons *why* people move is that they are victimized.

Retrospective surveys which gather data on similar, repetitive events all are faced with the fact that the details of individual behaviors or events often will blur in respondents' minds; they cannot be relied upon to give an accurate count of the number of times those incidents took place, nor to recall details which might differentiate one from another—especially if those details are important to the survey sponsor but not to the respondent. In the NCS, these are termed "series incidents." Before the redesign, they were defined as groups of three or more similar incidents which respondents could not adequately differentiate in terms of their placement in time. Because incidents in this category are presumably too frequent and similar to be enumerated

individually, they have been dealt with in the worst way possible—they are not counted at all. Series incidents (even the most recent episode, which is fully described in the interview) have always been excluded from estimates of the victimization rate, on the grounds that their frequency is uncertain and that they might not all fall in the same NCS category. This of course makes a shambles of any effort to use the NCS to document the extent of multiple victimization.

The series incident problem is further confounded by interviewer effects in the NCS. The Census Bureau employed interpenetrated sample designs in a number of city-level victimization surveys in order to estimate the variance in victimization rates due to differences among interviewers, and they proved to be quite large (for example, they increase the sampling error range for all victimizations by about 30%). The frequency of series incidents seems in part to be related to the skills and inclinations of interviewers, and a close reading indicates that series frequently "break off" with a change in interviewers.

The redesign group was quick to recommend changes in how series incidents are handled in the questionnaire and estimation process; the supervision of interviewers on this matter has been tightened up, and the minimum number of related incidents which can be written off to the series category without filling out separate offense forms has been increased from three to six. The new questionnaire asks a number of useful questions about series of crimes as a whole, including whether or not they were committed by the same person. The more general issue of interviewer effects has not gone away, but the shift of the NCS to more rigorously structured and supervised telephone interviewing probably will help (see below). Hopefully, the new cuing and screening plan described above will lighten the incident recall task sufficiently to reduce the effect of interviewer persistence on the results.

PANEL ATTRITION

The third major challenge facing the redesign group was the impact of wholesale respondent attrition on NCS estimates of the victimization rate. Residential addresses enter the NCS sample and remain eligible for interviewing for three and one-half years, and during that time interviewers periodically attempt to question everyone 12 years of age and older who lives there. When new residents appear at a sample address, interviewers relist the household, update the control card for the address, and conduct new interviews. The new residents replace the old ones in the panel, and unless an address falls vacant or is demolished, NCS estimation routines are unfazed by the turnover. There are two major difficulties with this procedure, however: one involves an important technical issue, the other a substantive one.

The technical issue is that the replacement interviews are "unbounded." One of the clearest methodological findings of the pilot studies which were conducted before the NCS was frozen in form was that people draw incidents into the temporal window that they are *supposed* to describe ("the past six months") when in fact those incidents occurred outside of the time frame. The effect of these out-of-range incidents is very large, increasing the victimization count by between 40% and 50% depending on the type of crime; the inflation rate is greatest for violent crimes and those (often more serious) that were reported to the police, and it is smallest for simple thefts. The tendency to draw out-of-bounds incidents into the interview is so significant that the first interviews at addresses entering the NCS sample are set aside and not used for estimation purposes despite their substantial cost; repeated mentions of earlier incidents during subsequent household visits then can be edited out. However, this "bounding" process is *not* repeated when there is a turnover of respondents at a sample address; the NCS treats the new interview as bounded, inflating estimates of the victimization rate. The number of respondents involved is substantial; the yearly attrition rate from the NCS sometimes approaches 20%, although that figure fluctuates (and thus the resulting bias varies) in response to demographic and macroeconomic forces that shape the frequency of moving and new household formation.

The substantive import of panel attrition is found in the reasons people move; one reason is that they have been victimized. People move away from bad neighborhoods, and they move (and change their telephone number) to break off abusive relationships. Moving is also linked to age, housing tenure, and household size, all factors that in turn are linked to victimization. Out-moving households have premove victimization rates that are one-third to two-thirds higher than households which remain in the sample. To be sure, many out-migrants are replaced by in-migrants, but in high-crime neighborhoods families are replaced over time by younger and unattached individuals, more people move out than in, the worst buildings fall into abandonment, and the areas depopulate. Over time, NCS households which remain intact report fewer and fewer victimizations. This *might* be due in part to time-in-sample bias, for as the novelty of participation in the panel wears off, repeat respondents may lose their enthusiasm for saying "yes" sitting through long interviews. However, the evidence is that this problem is small when compared to other panel-related issues.

One solution to the attrition problem is to follow out-movers. The redesign group convened a special subcommittee to investigate the adoption of longitudinal design for the NCS; this design would address the attrition issue and have important analytic uses. The subcommittee

proposed a scheme for retaining individuals who move in the sample, tracking them over time for up to several more years; the design was akin to that now utilized by the Census Bureau's Survey of Income and Program Participation (SIPP). A longitudinal design would yield more precise estimates of annual changes in victimization rates, and it would help maintain the representativeness of the NCS sample. It would also be more appropriate for addressing issues like multiple victimization; the impact of changes in occupation, lifestyle, and family organization that are frequently signaled by events like moving; the role of services for victims in reducing distress and discomfort; and the long-term impact of victimization on attitudes and behaviors. Linked panel data on individuals also would be useful for understanding their contacts with insurance companies and the criminal justice system, for these often take more than a few months to play themselves out. Before the redesign effort the NCS shed no light on arrests, court appearances, or the eventual fate of the cases it so carefully identified.

At this point there has been no movement on the issue of a longitudinal NCS design. The redesign group proposed a field test of the procedures required to conduct one, but that has not taken place. The logistical, data management, analytic, and cost implications of the adoption of a longitudinal panel are obviously significant and demand careful consideration. Probably, nothing will happen until the cost of developing a longitudinal panel design for the NCS can be shared with a redesign of the Current Population Survey, a Census Bureau survey with which it shares many common features. BJS will add a special supplement to the NCS to gather data on the justice system contacts and consequences of crime for victims who were identified in a previous interview, and they plan to follow these victims if they move before the follow-up interview. However, a longitudinal design for the entire survey seems justified on the basis of the substantive policy issues alone, and its adoption would also help solve important attrition and bounding problems.

UNDERCOUNTING AND MISCLASSIFICATION

The fourth challenge facing the redesign group was a bundle of survey design, sampling, and questionnaire problems which lead the survey to underestimate the victimization rate: it was apparent that the six-month recall window adopted by the NCS was still too long, interviews were not being completed at a high enough rate among the most victim-prone groups in the population, and respondents were being inadequately screened for recent victimization experiences. Unfortunately, it appears that most of these problems will not be rectified in the redesigned NCS.

One of the most important NCS design issues is the length of time in the past that respondents are asked to think about when they are quizzed about their experiences with crime. This is an important decision in any retrospective survey, and the appropriate reference period varies considerably from topic to topic: for ephemeral activities like television viewing and tooth brushing the only accurate reference period is probably "yesterday," while recall data on topics like household moves and major consumer purchases will be accurate for a longer span of time. As a practical matter, response windows can also be too short; briefer reference periods encourage more accurate reporting, but at the cost of requiring more interviews in order to gather data on the same number of incidents. A time-dependent forgetting curve can be drawn for any retrospective survey; the empirical question in each case is how steep the curve is, and the practical question is how far down it one is willing to go, or is forced to go by cost considerations. When researchers first began to talk about victim surveys they speculated that retrospective crime surveys could cover people's entire *lives,* producing a national victimization time series. However, record checks and split-sample experiments utilizing reference periods of varying lengths found that even a one-year window was too wide, and NCS designers eventually settled on a six-month time frame for the incident screening section of the questionnaire.

Six months probably is too long, however. There is a very sharp drop off in recalled crimes *within* the six-month window that was adopted. One record check indicated incidents that occurred during the three months preceding an interview were recalled almost 70% of the time, while those occurring four to six months in the past were recalled only 50% of the time. In one NCS analysis, the personal-crime victimization rate for the most recent month of the recall window was 261 per 1,000, while the comparable rate for crimes occurring during the most distant (sixth) month of the window was 162, a fall-off of 61%. Some of this within-window variation is due to respondents misremembering and shifting events around in time within the six-month time frame, but the evidence is that most of this fall-off is due to plain forgetting at the far reaches of the six-month period utilized by the NCS. Forgetting is selective, with more consequential incidents being recalled more accurately further in the past. It is estimated that a three-month recall period would yield victimization rates that are 10%–20% higher than the six-month period now utilized. However, many members of the redesign group endorsed employing a compromise four-month reference period because of cost considerations.

It is obviously more costly to conduct reinterviews three (or four) times a year rather than twice, and BJS would be forced to cut the size of the sample in order to do so. The resulting survey would gather more

accurate data. It would also be more timely, for the final interviews needed to complete estimates of the victimization rate for a calendar year could be completed three months earlier (not a small consideration in the policy world). However, the inevitably smaller samples would have greater sampling variance, and it would thus be more difficult to document significant year-to-year changes in the victimization rate. Currently, 5% annual changes in the "big rates" of wide interest (household burglary and violent crime) are statistically significant, a fairly satisfactory situation from a policy perspective. Since monitoring annual change is the first mission of the survey, and as there seems to be no money on the horizon except that which can be eked out by cutting the size of the sample, the agency rejected any reduction in the size of the recall window. Yearly *changes* in victimization rates probably are less affected by the bias this preserves than are the *levels* of the yearly estimates, but changes in crime, demography, and the economic environment can affect the direction and magnitude of the bias over time as well.

The sample undercoverage problems of the survey also are not unique; like many over surveys, young males—and especially blacks—are severely underrepresented in the NCS. Undercoverage of young black males may approach 30% in the NCS (a problem it shares with the CPS). The difficulty is that they are also the most victimization-prone group in the population. When a large proportion of all victimization is contributed by a small percentage of the population, and when undercoverage of that group is high, the overall bias that results may be much more severe than is generally expected. This may account for Cook's (1985) finding that he could count many more gunshot victims in hospital emergency rooms than the survey estimates are out there. This was probably not due to forgetting, but rather to poor coverage of the population most at risk.

The NCS makes several adjustments for undercoverage: roster data for each household are used to identify nonrespondents, and responses by other survey participants in the same general social category are upweighted to substitute for those who are missing. In addition, each estimation sample is reweighted to bring its general demographic parameters into line with Census Bureau estimates of the age-race-sex distribution of the population. These two weighting procedures work in a mechanical sense, but case weights for the comparatively few young black males who *are* interviewed are extremely large; this introduces a great deal of variance into estimates of the victimization rate for blacks, and leaves considerable doubt about what those data mean.

The final counting problem facing the redesign group stemmed from the NCS distinction between "household" and "individual" respondents. Individual respondents answer victimization questions only for

themselves, while household respondents (who are responsible residents 18 years of age or older) are administered additional questions concerning burglary, thefts in and around home, and motor vehicle theft. One general rule for retrospective surveys is, "the more you ask, the more you get," because more and detailed questioning usually increases the productivity of a recall task. The NCS turns out to be no exception to this rule. Household respondents get more screening questions about crime, and as a result they appear to be more heavily victimized. Some of this appears to be the "warm-up" effect of the initial household questions, for household respondents recall more personal crimes than do comparable respondents who get the shorter form of the questionnaire. In addition, some of what they initially recall in the household category turns out on closer inspection to fall into the personal violence or theft categories. In multivariate analyses, whether a respondent got the household questions or not is currently one of the strongest predictors of the prevalence of personal victimization. At the same time, the use of a single household respondent certainly leads to an undercount of household crimes like burglary, for there is no guarantee that the person who is designated to respond for the household is informed about all the actual or attempted crime which has taken place there. Currently, 12%–15% of the household burglary in the NCS is contributed by someone else. In one redesign pilot test, a second household member failed to mention burglaries that were mentioned by the other household member fully two-thirds of the time.

This may seem like a small problem, but it posed one of the most difficult logistical issues confronting the redesign group. The ideal solution to both the undercounting and differential counting problem would seem to be uniform screening—everyone would get the same questions, and they would cover everything. The difficulty is that the NCS frequently involves separate personal interviews with multiple household members; this generates a large number of interviews in a hurry, and at low cost, but the task of then *unduplicating* any multiple mentions of the same incident is a terrifically hard one, especially since there is no guarantee that different descriptions of the same incident will be exactly the same. The NCS now relies on the interviewer responsible for a household to do most of it on the spot, but as the survey shifts toward centralized computerized interviewing (see below), one interviewer will not be sitting there with all of the forms conveniently in her lap. The redesign team came up with a number of potential solutions to the problem, but they were all complex, error-prone, judgmental, or costly. Considering the problem did lead to a revision of the classification rules for incidents in the redesigned NCS. One great source of respondent confusion is the question of who owns what property in a family; the NCS was allocating property victimizations to

people or households in extremely arbitrary fashion, producing wildly fluctuating personal theft rates within families that were just an artifact of the allocation rules. Under the new system, everything (except cars) that is not taken directly from an individual will be classed simply as "household theft."

THE SCOPE OF THE SURVEY

The redesign group also faced several issues of questionnaire content. There was discussion of enlarging the scope of the survey to cover new kinds of victimization and including new analytic measures in the questionnaire which would enhance the research utility of the NCS.

The NCS was designed to produce victimization estimates that generally parallel the yearly Uniform Crime Reports of the FBI, and their categories drove the development of the initial incident screening section of the survey questionnaire. There have been proposals of varying merit to add crimes to the list of incidents covered by the NCS. For instance, the issue of parental kidnapping flickered briefly on the congressional agenda while the redesign was in progress, and there was a flurry of discussion about how that crime could be incorporated into the survey. In 1979, congressional legislation forced the FBI to add arson to their major statistical series, and there was some interest in making that an NCS household crime. There was more unrest about the way in which the survey precluded collecting data on threats made over the telephone or obscene telephone calls; both experiences are extremely common, especially among women, they tend to be highly repetitive, they are amenable to technological solution, and they are fear provoking. In the end, the redesign group decided to press only for the inclusion of vandalism. Vandalism is also a frequent and fear-provoking crime, one leaving visible signs that presumably evidence higher levels of intersubjective reliability than, say, verbal threats. The Census Bureau and BJS were concerned that household vandalism could not adequately be untangled from damage to public facilities or common areas in multiple-unit dwellings, and that it might be confused with attempted burglary. The latter would be a "rate-affecting" change; as we shall see below, changes to the NCS in this category were approached with caution. The solution was to place new vandalism items at the end of the screening section of the questionnaire, even after the catch-all "anything else" questions. In that position they do not fit into the scheme of organizing the recall task by life domains; that would require placing them earlier, among the questions about things that may have happened around home.

While the scope of the survey has not changed much, BJS has made use of the richness of the survey to devise new analytic categories in

which to classify incidents. They have issued reports on domestic violence, home invasions, crimes involving motor vehicles, and other topics which cut across the FBI's traditional way of classifying incidents, and the ease of doing this is one of the most useful features of the public-use tapes that are available from the NCS. The redesign group and BJS also devised a new annual social indicator—the percentage of households "touched by crime"—that may come to rival the crime rates released by the FBI.

However, the absence in the survey of measures *explaining* differential risk of victimization, or of indicators of the consequences of crime for fear and behavior, has been a source of disappointment to the research community since the inauguration of the NCS. An analytic survey would include measures of peoples' exposure to risk, their attractiveness as targets, their ability and willingness to resist crime, ratings of neighborhood risks, checklists of personal precautions and household security arrangements, and accounts of the extent of neighborhood anticrime programs. However, explanations are not fundamental to the first stated purpose of the survey, which is to monitor national trends in victimization; objectives for the survey that call for understanding the *reasons* for the distribution of victimization are only fifth on the list (see Bureau of Justice Statistics, 1989). The most visible analyses of the NCS have focused almost exclusively on the demographic correlates of victimization, surrounding them with speculative comments on the lifestyle and personal vulnerability factors that they may involve. In the absence of analytic variables there has been relatively limited use of the survey by social scientists. This may change a bit. BJS plans to use supplemental questionnaires to gather data on attitudes and risk factors on an occasional basis and to add new items on risky occupations, crime locations, justice system contacts, and weapon use to the core questionnaire. The survey will also include several new questions on victim resistance and bystander intervention, and the warm-up section of the new screening questionnaire includes four useful questions assessing the exposure of respondents to potentially risky situations. It is telling that many of the most interesting analyses of the NCS have been based on a modest supplemental questionnaire that was administered to NCS respondents for one month in 1984. It included a number of measures of attitudes and behavior, and assessments of neighborhood conditions. BJS plans to repeat this supplement, hopefully for a longer period so that it can be linked to more robust data on victimization. Otherwise, theoretical advances in understanding the distribution and trend in victimization (such as "opportunity theory") thus far have largely been based on continued analyses of the very data the NCS was to supplant for research purposes—official crime reports from the FBI.

COST ISSUES

When contrasted with many surveys conducted by private firms or university-based survey laboratories, the NCS is extremely cheap. This is partly due to the structure of the interview. The NCS opens with a brief crime-incident screener, and if a respondent answers negatively to all of those questions the interview quickly comes to a close. Most interviews do, and as a result the average NCS personal interview only lasts 16 minutes, and the average telephone interview only 12 minutes. In the old NCS, interviewers attempted to question every eligible respondent in the household on a single visit, and when they could not they could contact the remainder by telephone; this minimized recontact and travel costs. The survey is also very large, which enables BJS to spread its administrative expenses over a large number of interviews. As a result, the total per-interview cost of the NCS in 1989 was only about $40. However, because the NCS is a *very* large survey (involving almost one-quarter of a million interviews each year), this still adds up to a lot of money. In order to finance the redesign consortium and pay for testing and phasing in new procedures, there was a 15% cut in the size of the NCS sample in 1984. Cost considerations scuttled a number of desirable changes in the survey, including briefer reference periods, hand unduplication of incidents mentioned by more than one household member (perhaps allowing uniform screening), and any immediate shift to a longitudinal survey design. Budgetary concerns have also led BJS to seriously consider using data from the first-time bounding interviews for estimation purposes, after weighting them to adjust for overreporting.

One cost-cutting measure has been to convert the NCS to a survey administered mainly by telephone. Even in the beginning, about 25% of all NCS interviews were recontacts conducted by telephone; since then, the use of the phone to conduct interviews has been expanded considerably. The NCS now usually involves only two personal interviews: the first establishes initial contact at new sample addresses; the second is made on the fifth round of interviews, to maintain rapport with panel members. Interviewers collect telephone numbers for new sample households during the first personal visit, and at that time they ask respondents if it is acceptable to conduct follow-up interviews with them by phone. They also ask about the best time and day of the week for making recontacts. Households with no phone, or people who prefer not to be interviewed in this way, continue to receive personal visits.

The Bureau of Justice Statistics was an early supporter of the Census Bureau's experiments with telephone interviewing, and the BJS is planning to conduct as many as 60% of NCS interviews using the

Census Bureau's new computer assisted telephone interviewing (CATI) system. There is no plan to convert the NCS to a telephone sample, however. In pretests, the Census Bureau has discovered that it cannot maintain its traditionally high response rate for "cold call" RDD interviews. Its RDD completion rate is about 80%, in contrast to a 96% rate for the mixed-mode interviews (following an initial home visit) that Census now conducts for BJS. Because the NCS already involves so many interviews by telephone, making the complete transition also would not save very much money. The NCS sampling frame now includes nontelephone households—which have higher victimization rates than do telephone households—and BJS is also unwilling to concede reduced sample coverage in this area.

The NCS has shifted to a new address sampling frame that is somewhat richer with victims, however. Crime is a relatively rare event in many places, but it is unfortunately common in a few. The basic sample for the NCS is renewed every decade from decennial enumeration tapes; when this was done using the 1980 census address list, weighted FBI crime indexes were used along with population to define the strata utilized for sample selection. This deliberate oversampling of high crime areas was intended to significantly increase the selection probability for victims, giving the survey enhanced analytic utility at a modest cost in sampling variance. The new sample was phased in at the end of 1984.

Implementation of the New NCS

BJS and the Census Bureau have been examining the utility, feasibility, and cost of the recommendations of the redesign group and conducting pilot tests of new questionnaires and procedures. A few minor changes were made in the questionnaire as early as 1979, and in 1986 a number of significant changes were introduced that have been described above; these were all judged to be "non-rate-affecting." The next round of changes will be more substantial; it will include alterations in the questionnaire (new screening questions; inclusion of vandalism and lifestyle items) and adaptations in survey procedures (in the handling of series incidents and the introduction of CATI). This package of changes will be phased in during the period 1990–92, using a very clever scheme to slowly "splice" the old series to the new in a way that will enable analysts to monitor continuously the effect of the changes and produce adjusted victimization figures that maintain some comparability with the past.

Fourteen years will have elapsed between the release of the National

Research Council's evaluation of the NCS and the introduction of this package of changes in the survey, and several more years will pass before there are any useful new data reflecting the improvements that have been wrought. Was the wait worth it? Many changes for the better are being introduced. The new incident cuing section of the questionnaire is a demonstrated improvement over the old one, but we do not really know if problems in the measurement of related-party incidents and the apparently biasing effects of education and culture that were endemic in the old NCS have been alleviated. There have been some improvements in the mechanics of handling repetitive incidents, and that problem may also be helped by the new screening strategy, but the Department of Justice has once again cut proposals for a longitudinal design out of BJS's budget for fiscal year 1990. As a result, the panel attrition problem and difficulties in making sense out of patterns of multiple victimization in the NCS will remain with us. Moving NCS telephone interviews into a CATI system may also help control interviewer effects and reduce the frequency of series incidents (and, incidentally, seems to lead to *higher* rates). The survey is better off for the addition of vandalism to the list of criminal incidents and the addition of items on victim behavior to the questionnaire. However, household respondents will continue to get longer and different screening questionnaires, and most household residents will not be asked about burglary and crimes that are still classified as against the household; both are continued sources of bias. One special supplement has been appended to the NCS to provide in-depth coverage of school crime, and more are promised. The recall period for the survey remains six months, for budgetary reasons. Budgetary concerns drove other important changes to the survey while the redesign was in progress—these include the cut in the size of the sample and the conversion of the NCS into a survey administered mainly by telephone.

The NCS redesign group took a large view of its task. They delved into cognitive psychology and tried to develop a general theory to guide the conduct of large retrospective surveys; their goal was to develop a general, all-encompassing model of sampling and nonsampling error in surveys, and to address the errors in a systematic way. This was a grand vision, especially for a project supported by an operating agency; one director of BJS despaired that the redesign group had "taken on all of the problems in social science." From the point of view of this initial vision, the actual changes wrought by the NCS redesign project would be classified as "effective, but limited." The next round of desirable changes would include mostly "big ticket" items—conversion of the survey to a longitudinal design, adoption of uniform screening, and the announcement of a firm schedule of supple-

mental studies to be appended to the core questionnaire. BJS has moved most of the lower-cost items on the redesign agenda on-stream; the next stage of the process is largely a budgetary matter.

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