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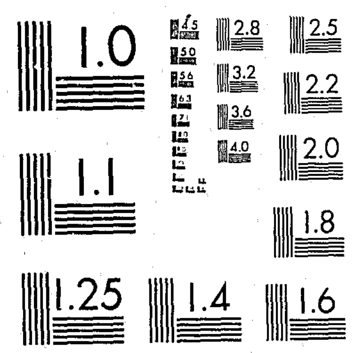

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U.S. Department of Justice
Bureau of Justice Statistics

Issues in the Measurement of Victimization

The task at hand

Conceptual issues

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Issues in the Measurement of Victimization

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by Wesley G. Skogan
Northwestern University
Evanston, Illinois 60201

U.S. Department of Justice
Bureau of Justice Statistics

Benjamin H. Renshaw, III
Acting Director

Charles R. Kindermann, Ph.D.
Acting Director
Statistics Division

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Abstract

This volume summarizes 15 years of research on methodological issues in the measurement of criminal victimization by means of population surveys. The report reviews some features of crime which affect our ability to measure it accurately, including the relative infrequency of serious victimization, the skewed distribution of victimization in the population, and the furtive character of crime. The third chapter addresses issues related to the operationalization of victimization in survey questionnaires. It examines the events orientation of victimization surveys, the assumption that crimes always are discrete incidents rather than continuous social processes, and the utility of measures of criminal activity abstracted from their social context. The fourth chapter reviews specific measurement problems: limited distribution of knowledge of incidents, forgetting or inaccurate recall of events, and differential productivity of survey respondents. The next chapter reviews three procedural issues which affect estimates of victimization rates: problems of panel bias and attrition, differences between telephone and in-person interviews, and interviewer effects. The final chapter 5 summarizes the current state of the art in this area and discusses possible future developments in victimization survey methodology.

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Chapter 1

Introduction

This volume presents an extensive overview of 15 years of methodological development in refining the methods by which criminal victimization can be measured through survey interviews. Both in the United States and abroad, there has been a great deal of research on methods for employing sample surveys to gauge the incidence of crime. This report attempts to synthesize those efforts, to report on the nature of the problems facing those conducting victimization surveys, and to assess the current state of the art. It examines the conceptualization of victimization implicit in current survey methods, the measurement techniques employed in interviews, and the procedures utilized in conducting surveys to gather data on the crime experiences of the public. The review takes a critical stance with regard to many of those concepts and methods. However, this detailed criticism is possible only because those responsible for the surveys have paid a great deal of attention to methodological aspects of their work. The Bureau of Justice Statistics (BJS)* and the U.S. Census Bureau have sponsored a number of investigations of victim survey methodology, and they continue to test the limitations of the data which their current survey produces. Reports of those efforts form the basis for much of this review. This volume also attempts to place in context these methodological problems and the efforts to solve them by reviewing related surveys and the results of more general methodological investigations. That comparative analysis suggests that many of the problems facing victimization researchers are not unique to their domain and that their solutions for those problems are generally as effective as most.

This volume is aimed at interested and relatively well-informed crime researchers and users of victimization data. It is not a "how to do it" text, but a synthesis of a substantial body of methodological research on the reliability and validity of victimization data. It is intended to inform researchers and data users of the strengths and weaknesses of those data, and, where weaknesses are apparent, to pinpoint some specific pitfalls to avoid in using the data. This report also notes a number of areas in which further methodological research is needed. There is increasing interest in criminal justice statistics with national scope, and attention to these continuing problems may speed the development of a useful Federal statistical program.

*The National Crime Survey was originally developed within the Law Enforcement Assistance Administration and was transferred to the newly created Bureau of Justice Statistics in 1980.

Methodological features

The specific focus of this volume is the methodological features of the National Crime Survey. This survey program, sponsored by BJS and conducted by the U.S. Census Bureau, is our primary source of data on patterns of criminal victimization, and the only ongoing victimization survey which is national in scope. (For details on the National Crime Survey see: Garofalo and Hindelang, 1977.) However, there has been a great deal of innovative methodological research conducted by States and municipalities, and abroad, and many of these contributions will also be reviewed here. The findings of all of those investigations have been remarkably consistent, suggesting that the problems inherent in measuring the extent of criminal victimization through sample surveys involve fundamental social and psychological issues.

Some of the consistency may reflect the fact that all of the surveys have focused on the same issue—victimization. The next chapter examines some aspects of crime itself and how they shape the nature of the survey enterprise. These include the relative infrequency of serious victimization, the skewed distribution of victimization in the population, and the covert nature of criminal action. Chapter 3 explores certain conceptual issues related to how surveys operationalize the concept of criminal victimization. It criticizes the events orientation of such surveys, the characterization of crime as a discrete rather than an occasionally continuous social process, and the abstraction of events from their social context. Chapter 4 reviews the impact of human factors on the accurate recall of events from the past. These contribute to the selective forgetting or inaccurate recall of those incidents. Chapter 5 examines problems which are procedural in character, including panel bias and attrition, interviewer effects, and decisions regarding interviewing procedures. Chapter 6 concludes with an assessment of current survey practices, a critique of data on the incidence of criminal assault, and some notes on future methodological developments in this area.

Chapter 2

The task at hand

Many methodological and procedural barriers to measuring victimization can be overcome. However, attempts to measure the frequency of crime through general population surveys face certain ineluctable difficulties stemming from the nature of crime. These difficulties ultimately limit our ability to use survey methods in gathering data on offenses. Three facts about crime are important in this regard: It is relatively infrequent, especially in its most serious and violent forms; it is unevenly distributed; and most criminals do their best to avoid detection.

Frequency of crime

Despite the large numbers which fill the columns of the FBI's Uniform Crime Report, one of the most important aspects of crime from the point of view of individual citizens is that it strikes infrequently. In any reasonably brief time period most people are not victimized. Further, there is a generally inverse relationship between the seriousness of a crime and its frequency. Recently, Marvin Wolfgang (1978) and his associates developed a supplemental questionnaire for the National Crime Survey which probed the perceived seriousness of many kinds of offenses in the eyes of the general population. The most frequent crimes—those which strike more than once for every 100 persons in the United States—all scored in the lower ranges of seriousness. This category included such offenses as petty theft (which scored 1.7 on their seriousness scale) and trivial violence (1.5). Some less serious offenses also are uncommon (like pocket picking), but no truly serious crime is very frequent. Those nearer the top of the seriousness scale, such as forcible rape (25.8) and homicide (35.6), all have incidence rates of less than 1 in 10,000.

The infrequency of serious crime in the general population has important consequences for victimization surveys. This was a major concern when the Crime Commission first considered conducting a victimization survey. Official statistics for the mid-1960's suggested, for example, that there were about 180 robberies of all kinds (including crime against businesses) for every 100,000 persons in the population. Given the apparently low frequency of such incidents, very large survey samples would be required to collect reliable information on many kinds of serious crime. All surveys are subject to sampling error, and, if victimization rates are relatively low, sampling error may lead to very substantial variations in estimates of those rates. Sampling error may also make it difficult to examine differences in victimization among population groups, because sampling variation may be greater than most true differences among them. Those who favored the surveys were convinced that the "dark figure" of unreported crime was large enough that a national sample of 10,000 households would uncover enough victims for analysis. The truth lay somewhere in between. While the survey collected data on property crimes which were common enough to make estimates of their incidence in the population, the number of victims of personal crimes who were located was very small.

The problem is more extreme in countries with lower crime rates. There random sample surveys of reasonable (and affordable) size will not allow reliable inferences either about rates of victimization or the characteristics of those affected by crime. For example, two surveys of people in Scandinavian countries (almost 2,000 people in Denmark and 1,500 in Norway) uncovered respectively 84 and 45 self-reported victims of violence or threats during the previous 2 years (Wolf, 1976b). In Göttingen, Germany, Schwind et al. (1975) found only 49 robbery victims in a sample of 1,170 persons. Sampling variances associated with national estimates of victimization rates made from data distributed in this fashion are extraordinarily large. It is unlikely that reliable differences between the personal crime victimization rates of any two European countries can be documented.

Even with its high victimization rate, the United States presents similar difficulties. Because crimes are so infrequent, researchers have to look long and hard to find recent victims of many of them. Most surveys involve relatively few people. A survey of the United States normally requires a sample of only about 1,500 carefully chosen respondents to gather reliable information about the personal attributes of people and things that are on their minds—their incomes, consumption habits, and attitudes. Virtually everyone interviewed has something to report on all of these topics, and differences among groups often are much larger than sampling error associated with samples of that size. In contrast, victimization surveys use samples of people to gather reports of particular kinds of events. Most of those who are interviewed have little information to offer, and it therefore is necessary to interview many of them. This is the major reason why LEAA's survey program has employed such large samples: 60,000 households for the national panel and 10,000 households in the city surveys. However, even based on 10,000 households (and 2,500 commercial establishments), the city surveys conducted by the U.S. Census Bureau for LEAA still could not produce estimates of victimization rates which were large relative to the magnitude of sampling error. It can be difficult to make reliable judgments about differences between and among cities due to the sampling error clouding those estimates. The surveys did not uncover many victims, especially of personal crimes. For example, estimates for rape victimization in Philadelphia were based on 29 actual interviews with rape victims, and in Detroit only about 150 robbery victims were encountered in the household survey (Jacob, 1975). While the data resulting from the city surveys have been used to make estimates of victimization rates for each city, those rates may vary enormously within the confidence interval which surrounds them. Figure 1 illustrates the problem.

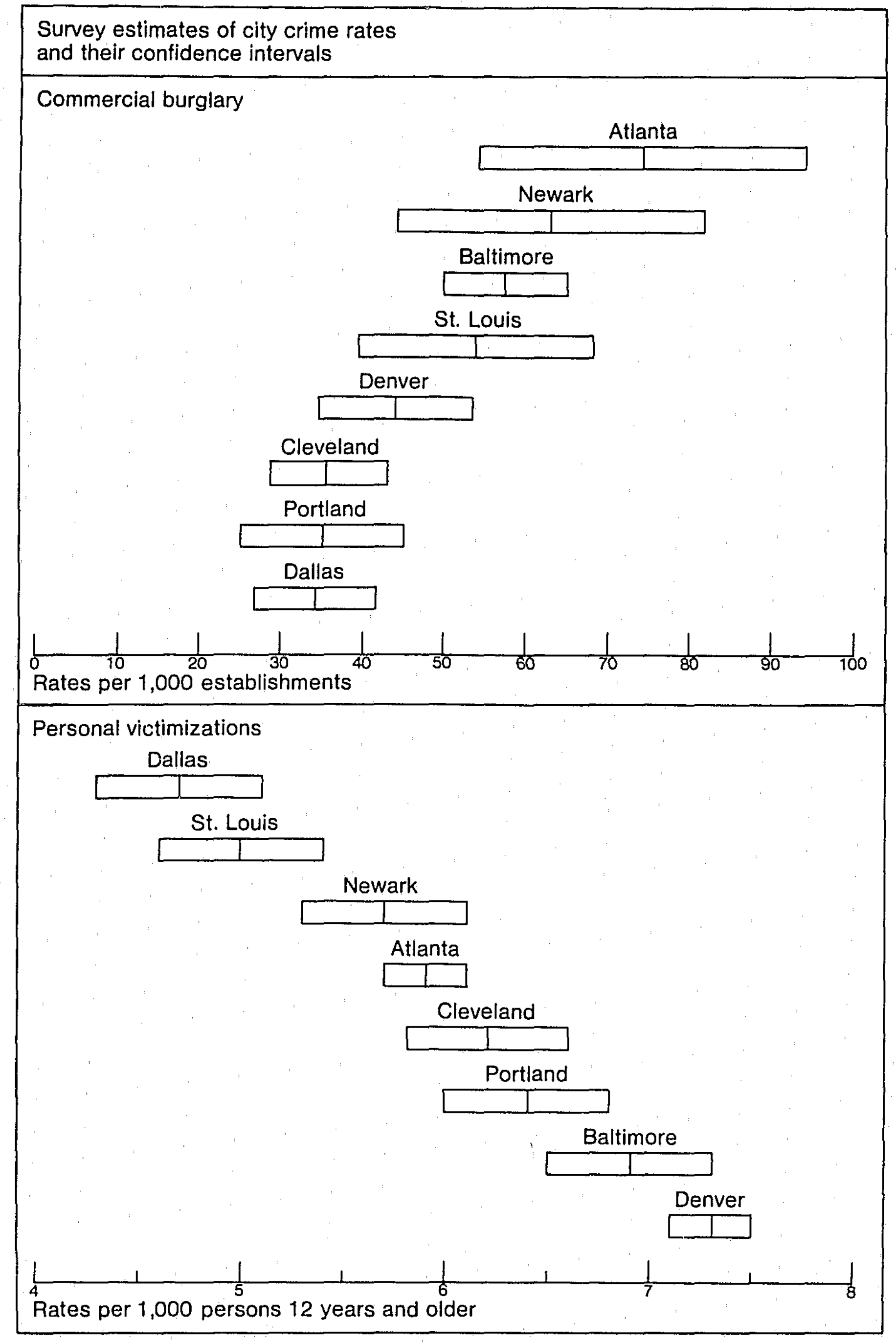

Figure 1

Figure 1 presents victimization rate estimates and their standard errors for eight cities where interviews were conducted in 1972. It reports estimates of the commercial burglary rate and the personal crime victimization rate for each of the cities, and the range within which we are 95 percent certain the "true" value must fall. The size of these intervals represents only the potential effect of sampling error.

Cities such as Newark, Baltimore, St. Louis, and Denver have virtually indistinguishable commercial burglary victimization rates despite large differences in their estimated values (63 per 1,000 in Newark, 44 per 1,000 in Denver), because the confidence intervals around these figures largely overlap.

The situation is less problematic when we examine personal victimizations, because the samples used to gather these data were larger than those for the commercial surveys. While it is difficult to talk about the difference between the personal crime rates of any two adjacent cities represented in figure 1, across the group of eight cities some are clearly high-crime and some low-crime places.

These confidence intervals do not take into account other, nonsampling errors. As we shall see, Bailey et al. (1978) recommend that confidence intervals should be doubled to protect against errors in inference due to the very considerable effect of interinterviewer differences on victimization reports. Because of the frequency-of-events problem and the many other measurement issues involved here, it is extremely difficult to produce accurate estimates of the victimization rate even when large samples are brought to bear on the subject.

The relative infrequency of victimization by serious crime has significant implications for the utility of the data generated by the crime surveys and for the applicability of the method in criminal justice research. The National Crime Survey originally was designed to produce estimates of the crime rate on a quarterly basis. This, in large measure, shaped the methodological development of the survey, for it focused attention on the ability of victims to recall the correct dates of incidents so that crimes could be counted in the appropriate quarter. There was less attention given to other possible criteria, including the accuracy with which other aspects of crimes could be remembered. However, quarterly estimates are plagued by a substantial amount of sampling error, and to date the data have only been used to make yearly estimates of victimization rates. The substantial sampling errors associated with estimates of victimization rates for cities limit their utility for city-

The relative infrequency of victimization by serious crime has significant implications for the utility of the data generated by the crime surveys and for the applicability of the method in criminal justice research.

level analyses as well. Some researchers (cf. Booth et al., 1977; Shichor et al., 1979) have used published estimates of rates for cities as indicators in aggregate-level correlational studies of patterns of victimization; however, none has taken into account the extent of, and variation in, error in those estimates.

Finally, the large sampling error associated with estimates of rates of victimization made from reasonably large samples greatly limits the utility of the technique for individual researchers or even local governments. For example, surveys conducted to evaluate the Seattle Community Crime Prevention Program produced "before and after" victimization rates for experimental areas which indicated a 36 percent reduction in burglary—but that difference was not large enough to be statistically significant. This was true in spite of the fact that (1) almost 1,500 households were questioned in the first round of interviewing and 1,200 in the second; (2) these interviews all were concentrated in only five census tracts; and (3) the survey examined the incidence of the most frequent major crime (Cirel et al., 1977). Studies that focus on personal crimes or small subgroups in the population are limited to an even greater extent by the laws of sampling.

Table 1.
Assault and robbery victimization, by number of times victimized

Number of times victimized in past 6 months	Assault		Robbery	
0	134,713	98.8%	135,943	99.7%
1	1,327	1.0%	384	0.3%
2	141	0.1%	16	—*
3-4	98	—	10	—
5-6	44	—	7	—
7-12	34	—	4	—
13 or more	8	—	1	—
	136,365	100%	136,365	100%

* "—" indicates percentage less than 0.1%.
Source: Author's computation. Note that this data was weighted to reflect noninterviews, sampling considerations, and other aspects of NCS weighting not related to producing population estimates. The frequency of series incidents was estimated using the mid-points of their frequency categorization.

Distribution of crime

Not everyone shares equally the burden of crime, and the highest risk groups often are relatively small. They also can be the most difficult to locate. One goal of victimization surveys is to identify high-risk subpopulations and their particular problems. Because these groups contribute disproportionately to the overall victimization rate, the payoff from these data for law enforcement officials could be substantial. Researchers interested in the effects of crime on fear and behavior also could benefit from detailed data on such groups as minority-group males, senior citizens, and middle-class families moving back into America's central cities. Because these groups all are relatively small, general samples of the population will not yield sufficient numbers for analysis. Further, ordinary survey techniques often fail to represent everyone in the population proportionately. The U.S. Population Census of 1970, for example, appears to have undercounted black males aged 30 to 34 by 18 percent, and the National Crime Survey for 1974 reached only 68 percent of the targeted figure for that group (National Research Council, 1976:table 6).

While crime is relatively infrequent in the general population, this is not the case among certain subgroups. Crime is spatially segregated. For example, in 1970 two-thirds of the reported robberies in the United States were concentrated in 32 cities which housed only 16 percent of the nation's population (Skogan, 1979). Within those cities crime was also heavily concentrated in a few places. In general, differences in the distribution of crime within cities are even greater than differences among cities. As a result, a relatively small proportion of the population is exposed to extremely high levels of risk, especially from violent crime. People from those places contribute quite disproportionately to the total count of victims.

The relatively extreme spatial concentration of victims, especially victims of violent crime, presents a challenge to samplers. Most surveys involve area probability samples in which successively smaller subsets of geographical areas are selected until a requisite number of sample points are chosen for interviewing clusters of respondents. If crime is clustered on the basis of demographic, economic, and physical features of individuals and neighborhoods and is disproportionately concentrated in certain small areas, probability samples reflecting only the distribution of people in the population across space will very often miss the mark. Those who have reports to make about a large proportion of all criminal victimizations may be only sparsely represented in such samples.

Turner and Dodge (1972) point out that high crime areas may be characterized by multiple victimization as much as (or even instead of) widespread victimization. Perhaps the most difficult group to study in any population is that made up of multiple victims. They are not uncovered very often in victimization surveys, although they contribute disproportionately to the resulting estimates of victimization rates for the population as a whole. Table 1 examines the frequency of reports of multiple victimization in the National Crime Survey. It reports the distribution of victims of two personal crimes—robbery and assault—in the data from the first 6 months of the National Crime Survey in 1977.

This analysis defines a "multiple victim" in the narrowest possible fashion as one who suffers twice or more from the same type of crime within a single 6-month reference period. Note that despite the very large samples involved, very few persons recalled being victimized more than once by the same type of crime in a 6-month period. On the other hand, this group contributed 20 percent of all the assault victimizations (but fewer of the robbery incidents) reported in the survey. Table 1 also should make it clear that general population surveys are a very cumbersome and expensive way to study this very small group, and the sampling and measurement errors associated with their identification make it very difficult to generalize about the results.

The inability of interviewers to locate representative samples of certain subgroups in the population also is troublesome. The difficulty is that factors associated with the victimization rate frequently are related to nonresponse. U.S. Census Bureau interviewers have been extremely successful in making contact with sample households and persons living there. Response rates for the National Crime Survey remain high (above 95 percent), even while responses to surveys conducted by academic and commercial polling units have slumped considerably. However, as Martin (1978) and others have pointed out, geographic location, lifestyle factors, and other correlates of respondent inaccessibility and problems in the management and administration of surveys often seem to go together with crime. Victimization rates are higher among transients, lower income persons, young males, city dwellers, those who recently have moved, and persons (like taxi drivers or bartenders) in occupations which often make them difficult to find.

Attempts to locate samples of victims selected from police files indicate that this group is particularly hard to cover, for completion rates in such studies have ranged from only 63 to 68 percent (Yost and Dodge, 1970; Turner, 1972a). This point is quite important, for there is a strong tendency for individuals who are victimized during one time period to report victimizations during later waves of the National Crime Survey (Lehnen and Reiss, 1978). However, changing residence (and thus moving out of the sample) also may be a common reaction to serious and multiple victimization (Reiss, 1977c).

Furtive nature of crime

Successful criminals elude detection or commit crimes with such speed and skill that they cannot be identified or described. Thus victims often cannot reliably describe attackers or their methods. In the crime surveys almost one-half of all victims of purse snatchings say that they cannot identify the race or estimate the age of their attackers. In the case of burglary, many cannot specify (even roughly) the time the crime took place. This severely limits our ability to use surveys to identify crime patterns or to use victims' descriptions to study offenders independently of police statistics. Many aspects of crime will remain hidden from sight regardless of the methods employed to examine them.

The extent of this inaccessible information about crime is documented in table 2. In that table, data from the National

Table 2.
Distribution of "don't know" responses about crimes, by type of crime

Crime attribute	Interpersonal violence	Robbery	Personal theft	Burglary	Larceny	Auto theft
	Type of crime					
Percent cannot identify offenders						
Sex	3.2	3.9	46.6	94.6	95.7	92.4
Race	4.0	6.1	47.5	95.2	95.8	93.0
Age	9.5	7.9	49.0	95.2	95.9	92.9
Percent who do not know relation to offender	3.2	3.6	46.0	90.5	92.9	91.8
(N)	(3,777)	(1,023)	(512)	(5,789)	(19,601)	(1,198)

Source: Computed by the author from all regular and series incidents from the National Crime Survey for 1973. Interpersonal violence category combines rape and assault; personal theft category combines purse snatching and pocket picking.

Crime Survey for 1973 are employed to illustrate the frequency with which victims who were questioned responded that they did not know about selected details concerning crime incidents. The ability of victims to report in detail about their experiences varies by type of crime. In general, the amount of information which they have to share varies in direct contrast to the frequency of crimes. The highest rate of "don't know" responses is among victims of larceny and burglary, by far the most common of these crimes. Victims are most able to recall the characteristics of offenders in crimes involving direct contact of some sort between them and criminals. Hindelang (1978) has taken advantage of these data to produce profiles of offenders in robbery and assault which are independent of (but strikingly similar to) those available from the police on persons arrested. However, events with fleeting victim-offender contact, including purse snatchings, yielded surprisingly little information on the apparent age or race of the offender. For property crimes, there was little information available at all about offenders.

This problem becomes even more intractable when we consider using survey techniques to gather data on other common victimizations, including shoplifting, fraud, and crimes of stealth and deception. They have low rates of detection, and there is no simple means to account for their frequency in the absence of shopclerks spotting offenders, or consumers becoming aware that they have been cheated. It seems that there are far more offenses of this type than are known to their victims.

Chapter 3

Conceptual issues

The form taken by victimization surveys reflects decisions which were made concerning the nature of crime and the utility of various ways of knowing about it. In the National Crime Survey, victimizations are conceptualized as discrete incidents, with a beginning and an end, and sharply bounded in space and time. As a result, the survey does not measure well continuous processes which are not so clearly delineated and which resemble enduring conditions more than discrete events. Further, victim surveys usually are oriented toward incidents rather than victims. They only measure events that can be uniquely described, thus ignoring classes of crimes for which victimization is quite prevalent even though the frequency of individual incidents is unknown. Finally, victimization surveys abstract events from their social context and define criminality without reference to the assessments of those directly involved in the incident. It is not clear that this is desirable, and there is evidence that participants in the surveys impose such criteria on the data in any event. All of these conceptual positions are reflected in the manner in which the concept of victimization is operationalized in surveys.

Operationalization of victimization

Operationalization is the translation of concepts being measured into terms workable in light of the measurement technology to be employed. In the victimization surveys, this takes the form of specific questionnaire items which are designed to elicit reports of crimes from their victims. Those questions define the concepts being measured in concrete terms. BJS's survey efforts rely on the criminal code as the source for the definition of criminal events. The questionnaire items parallel guidelines developed by the FBI for its crime reporting system in identifying components of events which are used to signal the occurrence of a crime.

Respondents in the survey are not simply asked if they have been victimized. If we ask people what bothers them, we will generate a great deal of data on the incidence of street urchins, demonstrators, noisy neighbors, and other things which lie outside the purview of the criminal law and beyond the capacity of the police to handle even in an informal manner. Rather, respondents are asked to report on their participation in specific events. The occurrence of a criminal event is indicated when they recall that they experienced or observed things resembling key elements of crimes. At the heart of the survey instrument is the "incident screen." A series of questions is asked each respondent, including:

> *Did anyone beat you up, attack you, or hit you with something such as a rock or bottle?*
>
> *Were you knifed, shot at, or attacked with some other weapon by anyone at all?*

Property crimes are probed by questions concerning the actual theft of property, or the observation of specific evidence that someone had attempted to steal something.

These items serve as "memory jogs." Each is designed to assist respondents in scanning their memories for crimes occurring within the survey's reference period. In the National Crime Survey, 17 items are employed to elicit "yes" responses from anyone who was a victim of one of those incidents. Eleven questions which are asked of all respondents are reproduced in figure 2. An additional six questions about household crimes such as burglary are administered to an adult informant in each household. At the end of the screening questionnaire, two "catch-all" questions (items 47 and 48 here) are included to stimulate the recall of any incident that has been overlooked. Respondents are asked if they had called the police about anything else that they thought was a crime, or if something had happened to them which they could have reported to the police. However, only reports of incidents which could be classified as rape, robbery, assault, burglary, or theft are retained for analysis.

This procedure for operationalizing victimization implies at least three conceptual decisions concerning the nature of crime: That crimes are discrete events which are bounded in space and time, that they are knowable only as distinct individual incidents, and that they can be understood apart from their social context.

Discrete events versus continuous processes

The fundamental unit of analysis in crime surveys is the victimization: an incident involving a victim(s) and an offender(s), which has a beginning, some characteristic activity, and an end. Events that resemble this ideal can be firmly placed in space and time, enabling us to examine day-and-night, public-and-private, and seasonal cycles of criminality. This undoubtedly is a useful way to describe many criminal incidents, including street robberies, store break-ins, and simple thefts. But there are many other kinds of crime (even by the definition employed in the crime surveys) which more accurately may be thought of as continuous processes rather than discrete events. What observers count as discrete incidents may be instances of ongoing disputes, conflicts, or predations. Several crimes which recently have come to the attention of the American public fall into this category, including child abuse, spouse abuse, and robberies of children in school. A commercial crime in this category of "continuous" criminality is price fixing. Because these are more or less enduring conditions rather than discrete events, they are difficult to count in conventional fashion. Incidents in this category share all of the barriers to reliable measurement to be discussed here, in addition to their intractability with regard to conventional accounting practices.

For example, consider a family in which the father comes home drunk every night, regularly beats his wife, and threatens his

Individual screen questions

INDIVIDUAL SCREEN QUESTIONS

36. The following questions refer only to things that happened to you during the last 6 months – between ____1, 197__ and ____, 197____. Did you have your (pocket picked/purse snatched)? ☐ Yes – How many times? ☐ No ____

37. Did anyone take something (else) directly from you by using force, such as by a stickup, mugging or threat? ☐ Yes – How many times? ☐ No ____

38. Did anyone TRY to rob you by using force or threatening to harm you? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No ____

39. Did anyone beat you up, attack you or hit you with something, such as a rock or bottle? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No ____

40. Were you knifed, shot at, or attacked with some other weapon by anyone at all? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No ____

41. Did anyone THREATEN to beat you up or THREATEN you with a knife, gun, or some other weapon, NOT including telephone threats? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No ____

42. Did anyone TRY to attack you in some other way? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No ____

43. During the last 6 months, did anyone steal things that belonged to you from inside any car or truck, such as packages or clothing? ☐ Yes – How many times? ☐ No ____

44. Was anything stolen from you while you were away from home, for instance at work, in a theater or restaurant, or while traveling? ☐ Yes – How many times? ☐ No ____

45. (Other than any incidents you've already mentioned) Was anything (else) at all stolen from you during the last 6 months? ☐ Yes – How many times? ☐ No ____

46. Did you find any evidence that someone ATTEMPTED to steal something that belonged to you? (other than any incidents already mentioned) ☐ Yes – How many times? ☐ No ____

47. Did you call the police during the last 6 months to report something that happened to you which you thought was a crime? (Do not count any calls made to the police concerning the incidents you have just told me about.)
(058)
☐ No – *SKIP to 48*
☐ Yes – What happened? ____________

CHECK ITEM C — Look at 47 – Was HH member 12+ attacked or threatened, or was something stolen or an attempt made to steal something that belonged to him? ☐ Yes – How many times? ☐ No ____

48. Did anything happen to you during the last 6 months which you thought was a crime, but did NOT report to the police? (other than any incidents already mentioned)
(059)
☐ No – *SKIP to Check Item E*
☐ Yes – What happened? ____________

CHECK ITEM D — Look at 48 – Was HH member 12+ attacked or threatened, or was something stolen or an attempt made to steal something that belonged to him? ☐ Yes – How many times? ☐ No ____

CHECK ITEM E — Do any of the screen questions contain any entries for "How many times?"
☐ No – *Interview next HH member. End interview if last respondent, and fill item 13 on cover page.*
☐ Yes – *Fill Crime Incident Reports.*

Figure 2

children, who may in turn be protodelinquents in their own right. Occasionally the family may generate an official statistic, as when the wife defends herself with a kitchen knife, or when the collective noise level reaches such heights that the neighbors call the police. When a crime survey interviewer enters the scene, a different set of statistics might be generated. Neither recordkeeping system adequately captures the situation; each samples ongoing activities in selected slices of time. What is being observed is a condition rather than an event. Another example would be a business concern whose employees regularly carry off merchandise. Is each distinct exit from the store a separate crime, or is the victimization rather one which is carried out over time? Are thefts by each employee separate victimizations?

This problem is most severe in the case of "series offenses." Occasionally respondents report that a particular kind of event occurred "many times" or "every week." In an in-depth study of these reports, Dodge and Lentzner (1978) found that they could be placed in four categories. The largest group was occupationally related incidents. It included victims who were law enforcement officials, bartenders, or bus drivers. All of these incidents were assaults. The next largest group was of related-party violence cases. These incidents involved family members, neighbors, and friends. In the bulk of those reports, the same offenders seemed to be involved in repeated incidents. Next were crimes involving violence against children. Most of these incidents were school related and involved victims and offenders of roughly the same age and sex. The smallest group was the ubiquitous "other" category. Overall, series incidents were disproportionately violent crimes rather than property offenses.

In the National Crime Survey, series incidents are those that are so frequent, similar in character, or otherwise unmemorable that their victims cannot disentangle them for an interviewer. In particular, if victims cannot recall the specific months in which similar incidents occurred, they are classified as series crimes. About 100 series incidents are recorded every month in the survey, and they make up about 3 percent of all incident reports (Dodge, 1975).

It is difficult to use the data that currently are being recovered on series incidents to estimate their frequency as discrete events. By definition, respondents have difficulty recounting details about those incidents which are sufficient to place them individually in space and time. In addition, it is hard to unambiguously classify them in analytic categories. Robberies and purse snatchings are difficult to differentiate even when a great deal of information about them is available. As a

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result, series incidents currently are excluded completely when National Crime Survey data are processed to produce national estimates of victimization rates. This has a substantial impact on the magnitude of those estimates. Reiss (1978) calculates that including series incidents would increase the estimated number of crimes in the United States by 18 percent. The irony is that those victims who in some ways have the worst crime problems, partially because of the intense difficulty of their condition, are not counted as victims at all.

Events orientation

The series-incident problem is illustrative of a larger conceptual issue: whether crimes can be recorded in the victim surveys only if they are known as discrete events. There are other ways to discern crime, and there are indicators which are revealing of its presence and magnitude even in the absence of data on individual events. Often these indicators are the only way to detect its occurrence, and by following incident-based counting rules we thus exclude from consideration crimes which are known to occur and have measurable consequences.

Most crimes are undertaken covertly. They are committed by persons who want to reveal as little as possible regarding their identity and behavior. The most successful crime is one which is never discovered or which leaves such a limited or confusing residue of clues that it is difficult to discern what in fact happened. Thus, under current crime-measurement procedures (and this includes official police figures as well as those based upon surveys) a businessman who conducts an audit or an inventory and discovers a shortage of cash or merchandise can make an insurance claim but cannot report anything that becomes a crime statistic. On the other hand, if an employee is observed stealing something or if a shoplifter is apprehended, those incidents are eligible to be recorded by the police and interviewers. The difficulty in the first case is that no discrete event was observed, only indicators of its magnitude—dollar losses. Limited by the event-counting approach to crime measurement, we ignore a great deal of crime about which we could gather useful information.

An alternative approach to estimating the magnitude of crime problems of this sort would be to turn from incidence to prevalence measures of victimization. The BJS's victimization surveys focus on discrete events and yield rates of the incidence of crime. A prevalence orientation to studying victimization focuses instead on victims, and yields data on the proportion of individuals or households which have been victimized. The unit of analysis thus shifts from crimes to the targets of crime. For example, in 1968 the Small Business Administration conducted a nationwide victimization survey of business establishments. Businesses were asked to report whether or not they had suffered any financial loss from shoplifting and employee theft and if they had apprehended anyone committing those crimes (Small Business Administration, 1969). This survey found that two-thirds of all retail businesses suffered shoplifting losses and that 31 percent had apprehended an offender during the previous year (Reiss, 1969). Using as its analytic focus the "victim or not" dichotomy, the study then explored the correlates of victimization by these two types of crime. Thus, even in the absence of information on the frequency of specific incidents, a survey such as this could yield important insights into victimization.

Social meaning of incidents

The major conceptual position implied by the incident screen is that we can talk about crime apart from its social meaning, the interpretation applied to an activity by those directly affected by it. Social meanings differentiate many objectively similar events. In general, when civilians kill policemen it is a crime, but when policemen kill civilians it is not; parents who strike their children "discipline" them, while children who strike their parents are committing a crime (in common-sense terms) only if they are grown up. Teachers, on the other hand, cannot strike anyone—but a decade ago many kept paddles on their desks. The problem of social meaning is not important when we examine street robbery or anonymous assault, but physical aggression within family and friendship circles, and thefts and robberies in which the offender personally was known to the victim, are more difficult to interpret. The "criminality" of an incident in such circumstances may depend very much on the expectations of those involved. Victimization surveys find that "it was not a police matter" is the explanation most often offered for not reporting most of those incidents to the police. The use of an incident screen which calls for reports of behavior may elicit information about activities which appear to fall within a standard crime category, but this does not always mean that it is appropriate to label them "criminal," especially if those directly concerned would not.

The meaning of an event also revolves around the issue of intent. Criminality is a concept which, in law, is not strictly behaviorally defined. Many people are injured in ways difficult to distinguish from criminal violence except through interpretations of those events supplied by participants. In a methodological study involving interviews about physical injury, Biderman (1975) quizzed over 600 people about their aches and pains. He found that 18 percent of those who were in pain or suffering from a handicap attributed their conditions to a criminal event; many more were victims of trauma inflicted by others, but those actions were interpreted as carelessness, error, or the result of inaction. Criminality is a label which may be officially conferred only after intent is established: this is what distinguishes manslaughter from murder. One may argue that from the victim's perspective the effect is the same. However, it is often necessary to establish intent in order to determine what kind of victimization an event was. For example, consider a shopkeeper who arrives at his store in the morning and discovers that his store window (still largely in place) has been smashed by a brick. What happened? It may have been an attempted burglary, a serious crime, or it may have been vandalism, which is not defined as a serious crime. The definition of the event will be determined by the interests of those involved and the procedures of those doing the measuring. The police will want to call it vandalism, for that is a Part 2 offense. Crimes in that category are rarely discussed when the FBI's statistics are announced. The motives of the shopkeeper

That we cannot agree upon what a crime is or how it may be isolated for measurement and analysis is indicative of how far victimological research has yet to proceed.

will be mixed, and his interpretation of the facts probably will be determined by how he wants to deal with his insurance company. The commercial victimization survey, on the other hand, could have recorded the incident as an attempted burglary, especially if the story gained some detail in retelling. Note that every crime measurement technique confronts the same cloudy information base. In that circumstance, social processes will determine the outcome of the investigation.

The importance of the social interpretation of events may be reflected in one of the major puzzles presented by data now produced by the National Crime Survey: the apparently inflated number of reports of victimization from assaultive violence contributed by higher status respondents. Victimologists always have assumed that the bulk of victims of assault comes from the lower reaches of the social ladder. Lower status persons are heavily overrepresented among victims of such crimes that are recorded in police files. However, one of the most universal findings of victimization surveys is that education is frequently only weakly and sometimes even positively correlated with reports of victimization by assault. Illustrative data from the National Crime Survey are presented in table 3. In this table, rates per thousand for assault are contrasted for groups reporting differing levels of educational attainment. Note that for 1976 those with college degrees recalled three times as many assaults as those with only an elementary school certificate.

This puzzling state of affairs seems ubiquitous. It characterized earlier surveys in the United States (Dodge et al., 1976), as well as surveys in Germany (Stephan, 1976), the Netherlands (Steinmetz, 1979), Norway, Sweden, Finland, and Denmark (Wolf, 1976a). If it is a methodological artifact, rather than a reflection of the "true" assault rate, it is an extremely robust one.

There are at least two competing explanations for this phenomenon. One is that it is an artifact of differential productivity in interview situations. This will be discussed in detail below. The other explanation is that respondents of different classes apply differing interpretations to

Table 3.
Rate of assaultive violence, by educational attainment

Education	Total assault rate per thousand
Elementary school	
0-4 years	9.8
5-7 years	9.2
8 years	7.2
High school	
1-3 years	14.3
4 years	14.8
College	
1-3 years	24.8
4 years	21.5

Source: U.S. Department of Justice, 1979a, table 15. Note that this table only includes persons 25 years of age and older. The data is for reference year 1976.

certain encounters. If the "true" distribution of violence is as victimologists traditionally have assumed, those who are unaccustomed to physical abuse may find it more memorable. What to some may be a condition of daily life may seem to others a brush with criminal violence. Because crime by its nature involves imputed motives and the imposition of definitions upon events by observers, differences in what respondents remember or think interviewers are asking may greatly affect the apparent victimization rate for selected groups. To explain such findings from his study in Stuttgart, Germany, Stephan (no date) speculated that they reflected differences in "sensitiveness toward criminality" on the part of upper and lower status persons.

Evidence internal to victimization surveys does not always support this interpretation of the data. For example, if differences in assault rates by social class were strongly influenced by differential interpretations or reconstructions of encounters, we would expect the positive education-assault relationship to be stronger for more trivial events; they presumably are more amenable to differential interpretation. It is not. Likewise, we would expect alternative measures of social standing, such as family income, to evidence similarly puzzling features. They do not: assault reports drop in frequency with increasing income (U.S. Department of Justice, 1979a:table 13). On the other hand, Stephan (1975) reasoned that "sensitivity to crime" should be greater among residents of Stuttgart than those of large American cities because crime is much less common in Germany. Therefore, Germans should remember more trivial events. He tested this hypothesis by examining the ratio of unsuccessful to successful crimes recalled in victimization surveys conducted in identical fashion in the United States and Germany. He found that Germans were more likely than Americans to recall unsuccessful or uncompleted crimes. While not as convincing as evidence in the other direction, this explanation might account for puzzling differences among American cities. For example, assault rates were higher for residents of San Diego than New York City, even controlling for their personal characteristics (Skogan and Klecka, 1977). Individual differences in the saliency of events also might explain some perplexing racial differences in the distribution of victimization by assault. Most notably, white residents of Washington, D.C., recalled 2½ times as many assaults as did black residents in the City Victimization Survey conducted there in 1974, and all of that difference was in the "simple assault" category (U.S. Department of Justice, 1975:table 3, p. 247).

It should be underscored that these conceptual controversies and their sometimes unsatisfactory resolution are not confined to the National Crime Survey; they reflect, rather, underlying, unresolved controversies within the field of criminology itself. That we are not agreed upon what a crime is or how it may be isolated for measurement and analysis is indicative of how far victimological research has yet to proceed.

Chapter 4

Measurement issues

Victimization surveys collect data on criminal incidents through interviews with participants. This use of self-reports of past events raises important measurement issues. Participants in a victimization survey are more akin to observers than to respondents in traditional opinion surveys. We assume that victims have been involved in events which have intersubjective meaning about which independent observers could agree. The task of interviewers is to elicit accurate reports of those occurrences.

Surveys tapping the hopes and fears or voting intentions of the citizenry strive for reliability in measurement. Because those surveys are probing internal states, researchers primarily are concerned that their readings of those states are stable and not highly dependent on a particular survey question. Victimization surveys strive for an additional goal, that of validity. Because the survey gathers data on events external to the individual, and those events presumably have a reality apart from their description to an interviewer, the standard of accuracy in victimization research is the match between the reality of an incident and its description.

This match is problematic under the best of circumstances. The problem is exacerbated by the nature of crime, conceptual disagreement surrounding the definition of criminal incidents, and a host of human processes affecting the accurate recall and description of things which occurred in the past. As a result, data on victimization may seem extraordinarily fragile, overly dependent upon subtle variations in the manner in which we gather it. In 1966 the Bureau of Social Science Research conducted the first investigation of victimization survey techniques. Its report concluded:

> Our survey method is heavily dependent upon the ability and motivation of the respondent to remember events and report them in the interview situation. In our pretest and survey experience, we have found that the quality of the reports of victimization that are elicited by our interviews depends to a considerable degree upon how the task of remembering and reporting is structured by the interview schedule and, presumably, by the way in which the interviewer uses it (Biderman et al., 1967:52).

Like the data on crime gathered by police, reports of victimization reflect both the distribution of events and our procedures for eliciting those reports.

Beginning with the work of the Crime Commission, there has been a great deal of research on specific techniques and strategies for improving the quality of victimization data. In addition, we can call on a substantial body of methodological research in related fields which confronts problems similar to those plaguing victimization studies. These include investigations of the quality of data gathered in surveys of unemployment, household expenditures, and health.

These studies suggest that retrospective reports of experiences with crime are clouded by four kinds of error. Each reflects fundamental human processes, and affects social measurements of all kinds. Errors in the measurement of victimization may be due to (1) ignorance of events, (2) forgetting (or not telling), (3) inaccurate or incomplete recall (or lying), and (4) differential interview productivity. In addition, there are a host of procedural problems and decisions about survey techniques, which seriously affect the data; they will be considered in the next section. Respondents sometimes do not know of things about which we quiz them. They also might have forgotten about them, a fallibility which in practice we cannot distinguish from their deliberately not telling us about them. Respondents may also either inadvertently or malevolently tell us something that is incorrect. Finally, some people are better respondents than others: they more readily grasp the nature of the task presented them; they work harder at it; and they tire of the demands of the survey less rapidly. All of these factors conspire to shape the volume and character of reports of victimization, sometimes independently and sometimes in conjunction with the true distribution of criminal incidents.

Methodological research techniques

Most of what we know about measurement problems in victimization surveys comes from three kinds of research. The first methodological research technique is analytic; it involves carefully examining the results of a victimization survey to infer the impact of various methodological features of the study on the data. The second technique is experimental; it involves varying specific survey methods across parallel samples and then comparing the resulting estimates of victimization rates or other aspects of the data. The third method is criterion validation; it depends on the existence of some alternative record of a crime which we can assume is accurate and we can compare to the results of an interview with the victim. Each of these techniques has made an important contribution to our understanding of the nature of error in the National Crime Survey and related efforts. The substance of those contributions will be discussed in later sections. Here we examine briefly the strengths and weaknesses of these research tools.

Analytic methods

A simple examination of data gathered in a victimization survey often is revealing of substantial methodological problems. For example, comparison of victimization rate estimates by month or quarter across the length of the recall period of a survey always reveals that reports of offenses are more frequent in months closer in time to the date of the interview. In January 1971 a segment of the sample for the U.S. Census Bureau's Quarterly Household Survey was asked about crime experiences for 1970. Eighty percent of

. . . the [victimization] survey gathers data on events external to the individual, and those events presumably have a reality apart from their description to an interviewer . . .

the personal crimes that were recalled by victims took place (as they were remembered) during the last 6 months of the period (Turner, 1972b). The same "bunching" of events in more recent months has been observed in Germany (Schwind et al., 1975) and England (Sparks et al., 1977). This does not seem to reflect the true distribution of crime.

Internal analyses of victimization data also point to weaknesses in reports gathered through proxy interviews. In early surveys an "informed adult" often was quizzed to gather data about the victimization experiences of all members of a household. However, in studies for the Crime Commission both Biderman et al. (1967) and Ennis (1967) found that respondents recalled many more incidents which involved themselves than they did incidents which involved others. Finally, reports of victimization may not be related to other important measures, like race and education, in ways that seem credible. During their early stages of development we often have less confidence in our measures than in our theories, and such findings indicate that further methodological research may be called for.

As this suggests, perhaps the greatest failing of inferential methodological exegesis is that we can only guess what might be error and we can only infer what the causes of error are. We recognize the excessive clustering of crime because we generally understand the seasonal pattern of offenses, and we presume that it occurs because victims forget more easily incidents which happened further in the past (actually, that is only partially responsible for the observed clustering). Many people have been puzzled by the positive relationship between education and assault rates revealed by the National Crime Survey. The data seem wrong; on the other hand, we collected the data because we thought that the base of our knowledge of victimization was inadequate. We often do not know enough about crime to recognize to what extent an observed distribution is affected by methodological factors. For example, in an analysis of National Crime Survey data over time, Lehnen and Reiss (1978) examined the extent to which respondents who in past waves of the survey had reported crimes were likely to report them in subsequent waves. Their hypothesis was that there would be a negative effect of respondents' past experiences in completing the detailed incident report questionnaire used in the survey. When they found that victims were more likely to report crimes in subsequent interviews, they argued for a substantive finding that some people are "victimization prone."

Experiments

The second widely used technique for exploring methodological quirks in victimization data is the parallel sample experiment. Alternative forms of a questionnaire can be administered, or different procedures can be utilized across groups, to explore the consequences for the data that are collected. Strictly speaking, this is an experiment only when assignment of membership in those groups is random; this condition has been met in most of the research reviewed in this monograph.

Perhaps the best example of an experiment supporting the development of the National Crime Survey was that conducted in 1971 in Dayton and San Jose. The issue was the relative advantage of gathering victimization data through interviews with every member of a sample household as opposed to interviewing a household informant. In each city half of a household sample was completely interviewed, while in the remainder only an informant was questioned. Not unexpectedly, the former procedure produced more reports of victimization (Kalish, 1974).

Experiments using randomized assignment have been used to examine other issues as well. Cowan (1976) investigated the impact of the current practice of interviewing parents about the experiences of their children. Victimization rates were higher for youths who were interviewed personally than they were for those represented by proxy. Two supplements containing additional attitude and judgmental questions have been added to some city and national survey questionnaires. Because this was done at random, the impact of those additional questions on estimates of the victimization rate could be analyzed. In each case, presenting additional items about crime and the seriousness of victimization before the main questionnaire seems to have stimulated the recall of more criminal incidents (Cowan et al., 1979; Balvanz, 1979).

Several investigations of the impact of survey procedures have focused on the issue of temporal telescoping. Telescoping is the tendency of respondents in retrospective surveys to "bring forward" events in time. This systematic-recall bias is a threat to victimization research because it may inflate the apparent victimization rate for the calendar period of interest, the reference period of the survey. This is controlled in the National Crime Survey by "bounding" each 6-month reference period by a previous interview; data are to be collected only on events which have occurred since the previous reference period and interview. Because people are randomly assigned to panels in the survey, it is possible to compare the reports of those who are being interviewed in the same month for the first time (thus there is no bounding interview) and for the second time (a bounded interview). These two random samples are reporting on events which occurred in the same 6-month period, so it is presumed (perhaps falsely, as we shall see) that differences between their reported rates of victimization can be attributed to the effect of that initial bounding interview (Turner, 1972b; Murphy and Cowan, 1976).

The experimental nature of these investigations lends a great deal of credibility to their findings, due to the power of random assignment. There are weaknesses in this experimental approach, however. The criterion by which the "better" method or procedure is to be chosen as a result of these studies is unclear, and in the end the decision always depends on an argument based upon other information. It has usually been assumed that "more is better," that the procedure which produces the largest number of victimizations is more accurate. Tuchfarber and Klecka (1976) argued that their telephone survey procedure was better than parallel personal interviews because they uncovered more reports of victimization. However, this is not an unambiguous criterion.

. . . the standard of accuracy in victimization research is the match between the reality of an incident and its description.

There may be reasons to believe that "more" is not necessarily more accurate. Interviews have demand characteristics which shape how diligently respondents work to complete their task, and conceivably certain techniques could encourage over-reporting. It seems clear that respondents could be encouraged to telescope forward more incidents from the past, or to reinterpret trivial and perhaps marginally criminal events, or even to lie in an attempt to please an interviewer.

In addition to the absence of a criterion, these studies are limited by the large number of interviews which are required to test conclusively the effects of methods or procedures. The relative infrequency of crime means that either very large samples or very large method effects must be involved if a cross-sample difference is to be significant. In the proxy interview study, which was conducted as part of the San Francisco City Victimization Survey, only 570 persons 12 or 13 years of age lived in the 9,778 households in the sample (Cowan, 1976). The interviewing experiment was far from definitive, due to the large sampling error involved.

Record checks

A third procedure for identifying methodological weaknesses in reports of victimization is the record check. The U.S. Census Bureau conducted three record checks while developing the procedures employed in the National Crime Survey (Yost and Dodge, 1970; Dodge, 1970; Turner, 1972a). In each study, samples of incidents were drawn from police files, and interviewers were dispatched to quiz victims named therein. The data gathered in these interviews were compared to the official records. Two questions were examined: "Did the victim recall the incident for the interviewer?" and, "Did the victim accurately identify the month in which it occurred?" Record checks thus document the recovery power of the survey and the validity of the dating of incidents. They were used to test successive improvements in the survey's instruments and the length of the recall period that respondents could be expected to report on accurately. (These record checks are reviewed in detail in Sparks et al., 1977, and Hindelang, 1976.)

In addition, there has been one major record check which reversed this process. It began with victims' reports gathered in a survey and attempted to match those with records from their local police department (Schneider, 1977). While the U.S. Census Bureau's investigations were largely confined to the power and accuracy of incident reporting, Schneider's study was concerned with the interview-record match of descriptions of offenders, victim-offender relationships, reports of self-protective measures taken by victims, perceptions of response time, as well as other data elements.

The record-check approach to the validation of reports of victimization is potentially a powerful tool for methodological research. However, the credibility of the findings depends in large measure on three assumptions: That the record employed in the comparison contains the correct view of the event, that the findings of record-check studies can be extended to cases in which no record was generated, and that problems in fielding such studies do not influence their findings.

The first difficulty with record-check validation is the assumption that a police file is a useful criterion for judging the veracity of victims' reports of their experiences. It has been assumed that the detail employed in the U.S. Census Bureau's validation studies—the month in which the incident occurred—was correctly reflected in police records. While this seems to be reasonable, the assumption that the police and victim necessarily would classify an incident into the same analytic category, or would interpret the event in similar fashion when making their respective reports, probably is not. As we have seen above, there can be pressure on both the police and the victim to recast events. The record check in San Jose suggests that attempted rapes and assaults were particularly prone to differential classification (Turner, 1972a). Further, Schneider's (1977) record comparison found a great deal of disagreement between the record of the Portland police and victims' descriptions of key elements of events. For example, on the question of whether or not the victim and offender were known to one another prior to the incident, the two reports agreed only 56 percent of the time. From the victim's point of view it also seemed that the Portland police were prone to classify assaults in less serious, Part 2 categories. In neither case is it clear that the police were correct in recording the crime. It should be noted as well that data from police records and police decisions regarding an incident may reflect information gathered from a variety of sources other than the victim including their own observations and reports of witnesses. This is another reason why details about events drawn from the two sources may not always be in agreement.

A second difficulty with the record-check approach to validation of incident reports is that it is limited to incidents which somehow came to the attention of a recordkeeper. The victimization surveys themselves suggest that only 50 percent of all serious crimes are reported to the police. Reported crimes are systematically different from unreported incidents, principally in terms of their seriousness (Skogan, 1976a). In general, record checks have been conducted only on crimes which are more serious and which are reported, investigated, and recorded by the police. Those crimes certainly should be the most vividly remembered by victims. Biderman (1971:4) has noted that we should expect:

> . . . poor recall of victimization for the type of unreported incident where the victim sees nothing whatever he can do about it (except cry over spilt milk). No pattern of actions follow upon the event that reinforce its psychological impact and provide additional concrete anchors in experience for recalling it.

Woltman and Cadek (1977) report that the "memory decay" curve apparent in data from the National Crime Survey is not as steep for reported as for unreported incidents or for those which are more serious. All of this suggests that record checks conducted to date probably overestimate the aggregate accuracy of the reports of victimization gathered in the surveys.

There do not seem to have been any record-check validations of victimization reports which have utilized reports other

The problems of respondents not knowing things which a measurement technique assumes they have knowledge of can have significant implications for the design of victimization surveys.

than those on file with the police. Thus we have no reading of the accuracy with which unreported crime is recalled in the National Crime Survey, despite the fact that the gathering of such data is one of the major goals of the project. Reiss (1977b) has suggested interviewing people who talked with the victims about their experiences and comparing those recollections with descriptions by victims who did not call the police, in an attempt to validate the recall of unreported crimes. Samples could also be drawn of households to which the police were dispatched, but where they did not write up an incident report. Record-check studies have the advantage of knowledge of the "true score" under investigation, which lends them great analytic power. There has not been enough critical or innovative research with regard to that criterion, however.

The final problem with record checks is the apparently universal tendency for victims to be hard to reach for interviewing. In every study of this type a substantial proportion of victims sampled from police files cannot be found or refuse to be interviewed. As a result, we are uncertain of the generalizability of the findings of these studies to the larger and apparently more transient victim population.

None of the record checks conducted by the U.S. Census Bureau has matched its usual standard for interview completions. In the city of San Jose a victimization survey of the general population enjoyed a 97 percent completion rate. As part of that project, a sample of victims was selected from police files, and their addresses were imbedded in the general sample. In that special victim group interviews were completed for only 63.5 percent. The bulk of the noninterviews (76 percent) was with people who simply could not be located; an additional 11 percent of victims moved from the city, and 13 percent refused to be interviewed or were never available (Turner, 1972a). This completion rate was the lowest of all the record checks conducted by the U.S. Census Bureau, although the figures were only slightly higher (about 68 percent) in studies conducted earlier in Baltimore and Washington, D.C. In London, Sparks et al. (1977) had even worse luck; they could find only 43 percent of their known victim sample, and 8 percent refused to cooperate.

These low completion rates are not surprising. It is prosecutor's lore that the first response of many victims of crime is to arrange an unlisted telephone number or to move to a new address. In the National Crime Survey people who recently have moved report higher rates of victimization than those who have not, and a substantial proportion of those reporting multiple or series victimizations moves to another address prior to the next wave of interviews (Reiss, 1978; Lehnen and Reiss, 1978). In addition, many victims (and witnesses) give false addresses to the police in order to avoid further involvement in a case or retaliation by their antagonist. It is unclear how generalizable the findings based on those who remain accessible are to all crime victims.

Knowledge of incidents

The problem of respondents not knowing things which a measurement technique assumes they have knowledge of can have significant implications for the design of victimization surveys. The commercial surveys conducted for LEAA were limited in scope to burglary and robbery, deliberately avoiding the difficulties involved in gathering incidence figures for shoplifting and employee theft. Individuals also may not recognize that an incident is a crime; this has limited the utility of surveys for studying offenses such as fraud. People also seem to exclude broad ranges of their experience as lying outside of the purview of the criminal law. Respondents in the national survey conducted for the Crime Commission were encouraged to volunteer reports of victimization for crimes not explicitly covered in the interview. However, Ennis (1967) notes that few respondents mentioned ordinance violations, housing discrimination, illegal treatment by government agencies, or other such offenses.

The bulk of the research concerning the problem of lack of knowledge on the part of respondents has focused on proxy interview procedures. In early surveys it was assumed that crimes were salient events that would be widely discussed, at least among members of a victim's household. Therefore it was assumed that it would be possible to conduct a victimization survey by interviewing just one adult in a household, asking him or her about the experiences of each household member. This procedure would seem to generate victimization data for a large number of individuals at low cost.

Subsequent analysis of data gathered in this fashion indicates that the method is inadequate. Biderman et al. (1967:45) found a 2-to-1 discrepancy in the frequency of reports of incidents personally involving informants and those affecting other household members. A difference of the same magnitude was found by Ennis (1967:102) in his national survey. In Biderman's Washington, D.C., study the correlation between the size of a household and the number of incidents reported there was even negative, rather than positive, in sign. In the national survey proxy problems especially biased victimization rate estimates for younger blacks, who rarely were the household member interviewed and whose experiences thus were underrepresented. The same pattern of underrecall for persons other than the respondent has been found in surveys overseas. In a survey in Stuttgart, Stephan (1976) questioned residents of 741 households. In some he interviewed all members of the family directly, while in others he interviewed only heads of households and asked them to report on victimization of other members of their family. Direct personal interviews proved to be almost 50 percent more productive of victimization reports.

Because no careful analysis of any of these data has been reported, we can only speculate about why the household informant technique seemed unsuccessful. Perhaps many crimes which went unreported using this technique were so minor they were not generally discussed. Others may have been so embarrassing that they were edited out of family conversation. This may be particularly true of crimes involving some culpability on the part of the victim, or of sex crimes. As part of its data quality control program, the U.S.

Census Bureau conducts reinterviews with small samples of respondents each month. Those who gave inconsistent responses in the two interviews are questioned about the difference. One respondent confronted with an inconsistency replied:

> *Mother was present when I was interviewed. I didn't dare tell (the) interviewer in front of mother. I didn't know if I really should tell about it anyway. (Graham, 1974:4)*

Researchers in the past apparently overestimated the extent of communication which goes on within families, especially across age lines. As noted above, experimental personal interviews with 12- and 13-year-olds indicated that self-reporting by young respondents rather than proxy reporting by their parents yields information about more incidents, most notably assaultive violence (Cowan, 1976).

The apparent unreliability of household informants as sources of data about the experiences of others led LEAA to fund the experiment in San Jose and Dayton that was described above. In half of the households interviewed there (and the sample was 11,000 households in each city) a "chance respondent" was interviewed, and in the other half every resident 16 years of age and older was quizzed. Differences between the estimates of victimization rates produced by the two methods were substantial; the ratio was 1.7 to 1 for rape, 2.1 to 1 for strong-armed robbery, and 2.2 to 1 for attempted robbery, in favor of the self-response technique (Kalish, 1974:37). As a result of this experiment LEAA decided to adopt complete-enumeration samples for city and national surveys, despite the substantially greater cost this entails.

The unreliability of household informants continues to be illustrated in National Crime Survey reports of property victimization. In addition to being quizzed about the standard litany of personal crimes, one respondent in each household is asked to supply details about such incidents as burglary which presumably affect the entire family. This supplemental household questionnaire is thus given to only one informant at each address. However, an examination of all of the incidents revealed by the survey indicates that a substantial proportion of the crimes which should have turned up in the household interview (13 percent for burglary and 30 percent for household larceny) were in fact reported by someone else in response to screen questions which were not designed to stimulate the recall of household crimes (Dodge, 1977a). An informant cannot be relied upon to supply details even about burglary or auto theft. This raises the question of how many additional household incidents would have been uncovered in the surveys if appropriate memory cues were supplied for every respondent.

Forgetting and not telling of incidents

There is a tendency for victim-respondents to fail to report information about incidents which have occurred and about which they should have been knowledgeable. We can observe examples of nonrecall in methodological studies employing each of the three research techniques described in the previous section. For instance, the way in which an interview is structured affects the frequency with which instances of criminal victimization are recalled. Experiments reveal that when respondents have to work harder at their assigned recall task, or when the task is organized so that they easily can learn how to reduce their workload, they will respond by restricting the amount of information they contribute to the survey. Second, record checks indicate that victim recall can be highly selective. Respondents seem to edit incidents which may be embarrassing or may be considered "none of the government's business," even when they previously were reported to the police. Finally, victimization rates analyzed monthly or quarterly over the length of the survey's recall period typically indicate that few incidents occur in the most distant months, although other evidence suggests that crime was just as frequent then.

In each case the observed variations in victimization rates are artifacts of the method employed to gather the data rather than reflections of the distribution of the true rate of crime. There are three general sources of nonresponse which correspond to the examples given above: Respondent load and fatigue, purposeful suppression of valid responses, and forgetting. Because crime surveys employ verbal interviews to elicit victims' reports, it is not possible to distinguish directly between these sources of nonresponse. Physiological measurement techniques, including the use of lie detectors and devices to measure galvanic skin response, potentially could identify outright lying, but these have never been used to validate responses in victimization surveys.

Load and fatigue

The effects of workload factors were first noted in the Bureau of Social Science Research's pretest of victimization survey methods. It experimented with two procedures for conducting interviews. In the first, respondents were given flash cards describing criminal incidents. If they indicated that they had been involved in such an event, a detailed incident report form was completed for it at that time. The other procedure involved asking respondents to give "yes or no" answers to a complete checklist of offense descriptions before filling out incident report forms for each positive response. The first procedure clearly linked a positive response with a lengthy respondent task, while the latter did not allow the respondent to become test wise until it was too late. Not surprisingly, the second mode elicited 2½ times as many reports of incidents as the first (Biderman et al., 1967).

The current screening procedure used in the National Crime Survey reflects this experience. By deferring the introduction of incident forms until the completion of the incident checklist, it may encourage more complete recall. However, there may still be a tendency for respondents to suppress reports of victimization in order to speed the interview, a disposition that presumably would be greater in surveys with 12- rather than 6-month reference periods.

Surveys that employ a household informant entail a considerably heavier respondent burden. Biderman (1973) speculates that once respondents have manifested their cooperativeness by recalling a victimization, there is less pressure in the interview situation to remember others, because the interviewer has been "satisfied." Personal interviews are social interactions. Interviewers ask for people's time, and they can offer little in return. Respondents may reciprocate by offering a little to the interviewer and then stopping. This may explain the surprisingly slight incidence of multiple victimization documented above. Given the average number of victimizations in the population, statistically we should find fewer nonvictims and more multiple victims than currently are uncovered in surveys (Sparks et al., 1977). Fatigue, impatience with the repetitiveness of the incident screen, and other factors may account in part for the observed distribution. This is likely to be more common among poorly motivated respondents, those who find interviews taxing or incomprehensible, and those who find few social rewards in chatting with someone from the U.S. Census Bureau. Biderman (1973) speculates that such persons may be more likely to be victimized by crime as well.

Lying and not telling

The evidence that respondents may be lying, or deliberately suppressing reports of events of which they have full knowledge, is quite inferential. It comes primarily from record checks based on reports of incidents sampled from police files. In the San Jose methodological study described above, evidence emerged that respondents who were known victims were neglecting to describe particular events. The relationship between the victim and the offender as recorded by the police seemed to play an important role in the recall of those events in subsequent interviews. As indicated in table 4, incidents in which the victim and the offender were related to one another were reported in the survey only 22.2 percent of the time. That recall rate rose sharply when the relationship between the parties was more tenuous. For events involving strangers the recall rate was 76 percent. Two-thirds of the personal victimizations that were not recalled involved at least an acquaintance between the parties, while three-quarters of all "stranger" crimes were recalled. Eleven of the fifteen rapes which went unmentioned involved nonstrangers.

Table 4.
Patterns of record-check response

Incident characteristics	Percent recalled	Number of cases
Offender a stranger	76.3	99
Offender known	56.9	78
Offender related	22.2	18
(Total cases)*	(63.7)	(206)
Assaults—total	48	81
Assaults by strangers	54	24
Rapes—total	67	45
Rapes by strangers	84	19

*Includes other subcategories.
Source: Turner, 1972a:9.

Almost an identical pattern was uncovered in a record-check study of the validity of survey reports of assault conducted by Statistics Canada. They found that 71 percent of stranger assaults were recalled, but only 56 percent of "known party" assaults and 29 percent of related-party assaults were recalled (Catlin and Murray, 1979:table R). Those figures are extraordinarily similar to findings from the San Jose record check.

There are competing explanations for this phenomenon. Victims may not remember disputes which arise within kinship or friendship circles as readily as they remember events involving strangers—the data in table 4 may reflect true forgetting. Or, such disputes may not register as the kind of incidents that the interviewer is looking for—they may not be construed as crimes. People may think that to be a "crime" violence must involve strangers. However, these alternatives seem unlikely, for these incidents all were "founded" by the San Jose police.

It may be that persons who have been victimized by someone they know frequently may not think it is any of the interviewer's business. Or, the survey may raise again the memory of a painful situation, one which victims may not wish to recall. Although these all were incidents which came to the attention of the police, the victim may not have been the party who called them; many crimes are reported to police by friends, relatives, and bystanders, and the offended party may not wish to spread the story even further. Finally, in related-party cases the question of who is to blame and who is the real victim is not always clear, and the role of the person being interviewed might not always withstand close scrutiny. It is possible that an interview with any of the participants in these affairs could have recorded what appeared to be a victimization.

Victims who are themselves culpable may also be motivated to suppress information about criminal incidents. Research on crime indicates that "victim precipitation" is a common phenomenon in violent crime and in incidents where the victim knows the offender. In those incidents it is the eventual victim, rather than apparent offender, who first initiated the event. Other crimes may be encouraged or facilitated, if not caused, by citizen behavior. Biderman's (1967) survey in Washington, D.C., dealt in passing with this problem. There, 25 percent of all victims agreed that they were negligent or had done something foolish which contributed to their plight. Victims who feel culpable may be less likely to report their experiences later in an interview.

Forgetting

Most research on nonrecall has focused on what is assumed to be true forgetting. The problem has been described variously as "time-dependent error" and "memory decay," for it appears that the difficulty is one of remembering incidents from the more distant past. At one time it was assumed that crimes were very memorable events; it was planned to use retrospective surveys of the general population to reconstruct an historical time series for victimization rates, using interviews with a life-long reference period. Pretests quickly demonstrated the futility of that enterprise. Rather than being readily memorable, Biderman et al. (1967:31) found:

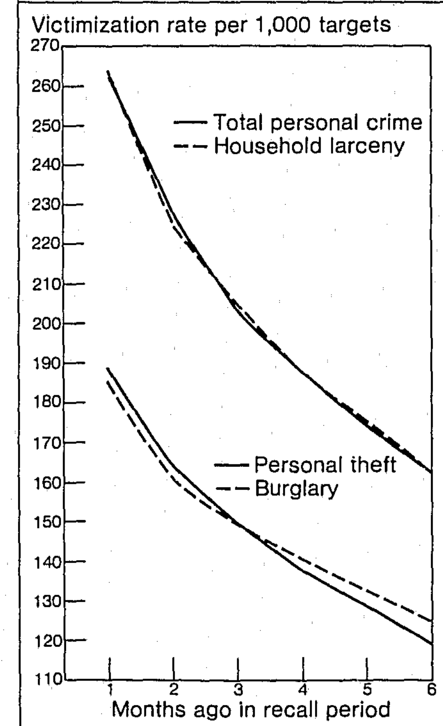
> In practice, most respondents seemed to find it difficult to remember incidents of victimization other than recent cases. . . . People reported hours, days, and even weeks later that incidents they had not remembered at the time they were interviewed had come to mind subsequently.

In the Washington, D.C., survey, respondents were asked to recall the "worst crime that has ever happened to you." They recalled a total of 260 incidents in response, only 108 of which occurred more than 2 years previously, and only 60 of which happened 6 or more years in the past (Biderman et al., 1967:41). Biderman et al. (1967:40) noted:

> Respondents have to do a great deal of thinking and slow reflection before they can remember even fairly serious crimes of which they were victims some time ago—even when these older incidents are far more consequential than recent ones.

The National Crime Survey now inquires only about what has happened "in the last 6 months."

Rates of victimization reported for months in recall period



Source: Data from Woltman, Bushery and Carstensen, 1975, Table 2

Figure 3

The fact that victims forget about their experiences with the passage of time also has serious implications for the accuracy of estimates of victimization rates based on surveys. This is illustrated in figure 3, which shows how different the estimates of victimization would be if they were calculated on the basis of crimes which were described as happening 1 month ago, 2 months ago, etc. If we used crimes described as occurring only 1 month ago, we would find that the national rate of victimization from personal theft was 189 per 1,000, and for all personal crimes 261 per 1,000. However, with increasing lengths of recall those estimates would have dropped sharply. Based on incidents recalled for the sixth month before the interview, the corresponding rates would drop to 119 per 1,000 for personal theft and 162 for all personal crimes.

Table 5.
Record-check recall,
by months of recall demanded

Months between interview and incident	Percent recalled	(N)
1–3	69	(101)
4–6	50	(100)
7–9	46	(103)
10–12	30	(90)

Source: Turner, 1972a:8.

As we shall see below, not all of this gradient can be attributed to the forgetting of past incidents. It is also shaped by forward telescoping. However, decreases in rates for personal crime and burglary of nearly 100 incidents per 1,000 over a 6-month recall period clearly signal trouble. As we saw with regard to the 1971 Quarterly Household Survey, the problem is even more extreme in 12- as opposed to 6-month recall periods, and this doubtless affects the yearly victimization estimates produced in the city surveys conducted between 1972 and 1975.

Similar declines in recall with the passage of time can be observed in data from record-check studies. In record checks, samples of cases of different "ages" are drawn from police files. Interviews with victims are employed to determine if those from the more distant past are less likely to be recalled. Record checks are more definitive studies of the forgetting problem because other factors which affect the distribution of data such as that in figure 3 are not present. Table 5 summarizes the findings of the San Jose record check. It

The fact that victims forget about their experiences with the passage of time also has serious implications for the accuracy of estimates of victimization rates based on surveys.

indicates the proportion of incidents that was remembered by victims in light of the number of months of recall that they required. As table 5 indicates, recall was relatively high for cases from 1 to 3 months in the past, but it hovered around only 50 percent for those from 4 to 9 months in the past, and then dropped below one-third for those from nearly 1 year in the past. As the author of the San Jose report noted, based on this criterion "... there is very little to choose from after the first three months" (Turner, 1972a:8).

Declining rates of recall with the pressure of time also were noted in earlier U.S. Census Bureau record checks in Washington, D.C., and Baltimore, although patterns in the Washington study were less clear-cut (Dodge, 1970). In Baltimore, levels of recall were much higher than in San Jose, averaging 81 percent, but evidenced a steady decline with passing months (Yost and Dodge, 1970). On the other hand, Sparks et al. (1977) found high rates of recall (averaging 92 percent) and only a slight decline in that rate over a 10-month period.

There is considerable interest in patterns of forgetting, for they have implications for how we interpret data with such error. For example, if incidents which are reported to the police are less likely than others to be forgotten, this will increase the apparent reliability of victimization reports, because record checks have all been based on police records. On the other hand, if forgetting is unrelated to the characteristics of incidents or the attributes of victims, it is much less likely to lead us to make false inferences from the data.

Conflicting evidence

Evidence about the relation between incident characteristics and rates of forgetting is mixed. One of the first investigations of the problem was done as part of a study on a subject not related to victimization. Neter and Waksberg (1964) examined reports in a survey of household repairs and concluded that less expensive projects were likely to be forgotten more rapidly with the passage of time. The results of studies of criminal victimization have not been consistent with this regard, however. The Washington, D.C., record check indicated that incidents involving smaller financial losses were not more likely to be forgotten (Turner, 1970). Looking at general crime categories, Ennis (1967) concluded that nonrecall in his survey was unrelated to incident seriousness. Comparing monthly official crime rates with victimization rates for eight cities, Gottfredson and Hindelang (1977) came to the same conclusion, but in a fourth study, Woltman and Cadek (1977) found that crimes involving weapons (which presumably are more serious) were less likely to be forgotten.

Similarly, studies have been inconsistent with regard to the relationship between whether or not victimizations are reported to the police and patterns of forgetting. Gottfredson and Hindelang (1977) found that monthly survey rates for reported and unreported crimes deviated from official figures for eight cities in the same fashion, indicating that there was no relationship between notifying the police and remembering the incident later in an interview. On the other hand, Woltman and Cadek (1977) noted that reported crimes evidence less of a time-dependent gradient than unreported crimes; this is similar to Schwind et al.'s (1975) data for Göttingen, Germany. Note, however, that these analyses necessarily involved the inspection of patterns of reports of victimization and not record checks.

Only record-check data can speak definitively about memory decay, for reports of victimization (and other events) in retrospective surveys are strongly influenced by telescoping as well. While telescoping can be both forward and backward in time, the net effect of these two forces often is strongly in the forward direction (Schneider, 1977). There is a significant tendency for respondents to "move" events around in time, falsely describing them as being more recent than they actually were. In a set of victimization data, forward telescoping (which shifts events into later months) and forgetting (which primarily affects earlier months) combine to produce memory error gradients like those in figure 3. In the end, forgetting is a much more significant threat to researchers than is telescoping, for in the latter case events are recalled with inaccuracy rather than being overlooked completely. Telescoping within a reference period will not affect data on victimization for groups, nor will it affect estimates of city or national rates. Forgetting, on the other hand, will certainly affect estimates of victimization rates, and it may lead us astray in fundamental ways if it is related to the attributes of crimes or victims.

The inextricable relationship between forgetting and telescoping in victimization data recommends further record-check research. With only two exceptions—dollar value of losses and victim-offender relationships—none of the U.S. Census Bureau's reverse record checks has examined correlates of nonrecall other than the passage of time. In England, Sparks et al. (1977) concluded that there were few attributes that differentiated between those who did and did not recall known victimizations. In general, nonrecall was not related to age, race, residential mobility, sex, social class, or the attributes of incidents. On the other hand, because most incidents in the London study were recalled, there was little variance to be investigated.

More recall research is necessary to inform decisions about the optimal length of the reference period to be employed in victim surveys. The National Crime Survey currently employs a 6-month recall period, although we have seen that both record checks and visual inspection of the resulting data suggest that substantial time-dependent memory error occurs even over that length of time. In Scandinavia, where relatively little methodological research has been done on these matters, victimization surveys typically employ reference periods of 2 years or even longer (Wolf, 1976b).

In terms of time-dependent forgetting, the best recall period is the shortest one. People doubtlessly can give the most accurate information about events which occurred "yesterday." Other factors also affect decisions about the length of a survey's reference period, however. Brief reference periods will generate relatively little data on victimization for most individuals, especially about serious crimes. As a result, there is a tradeoff between length of reference periods and the size of the sample required to make stable estimates of victimization rates. Also, errors in measurement due to factors such as telescoping will have a greater proportional effect on data gathered for shorter reference periods (Reiss, 1977b).

Inaccurate or incomplete recall of incidents

The failure of respondents to share information about events which apparently did involve them is not the only type of error encountered in data gathered in interviews with crime victims. Information which is volunteered may be incorrect or at least different from that gathered on the same incident from other, presumably more reliable, sources. Victims make mistakes: they may inaccurately recall the amount lost in a crime or the date of the incident. In their London study, Sparks et al. (1977) compared the month in which victims placed criterion incidents with police information on the same offenses. They found that only 55 percent recalled the month of the offense with accuracy. Victims may also deliberately misconstrue their role in a crime, the value of a stolen object, or the identity of an offender. This may be common in crimes that involve close victim-offender relationships, victim complicity, or victim precipitation. The police often suspect the motives of complainants, and so might survey interviewers.

Record-check comparisons of archival and interview data indicate that at least two types of recall error present serious methodological problems: temporal telescoping and misreporting. There can be a great deal of disagreement, some of which appears to be time dependent, about the characteristics of offenses and offenders between these two data sources.

Telescoping

The issue of temporal telescoping has received a great deal of attention, because it has profound implications for survey design and cost. In an early study, Gray (1955) conducted a record check of reports of sick leave by British civil servants. He found that few forgot completely that they had taken leave, but that there was a substantial tendency for them to err in recalling when they took it. Neter and Waksberg (1964) investigated telescoping problems in self-reports of household repairs. They found that recall error was predominantly in the forward direction, moving events closer to the date of the interview. They also discovered that major expenditures—which presumably would be more memorable— were more likely than minor expenses to be telescoped forward. Because minor expenditures also were more likely to be forgotten, the error structure of the U.S. Census Bureau's self-report data on household repairs was very complex.

Although telescoping is found in many retrospective surveys, it is not altogether clear why it takes place. Part of it may be due to the demand characteristics of a victimization survey. In this case, the "demand" to produce an incident occurs because most respondents have not been victims of most of the crimes covered in the interview. The long incident screen produces a succession of "no" responses, and respondents may feel the interviewers are "disappointed" by their lack of productivity. In this situation, the temptation to give the interviewer some false but apparently satisfying information may be overpowering when a familiar but slightly out-of-bounds incident comes to mind (Biderman, 1970). There is also some evidence that frequent and recurring events are telescoped more often, for there is a greater likelihood that the respondent will become confused about his or her dates (Sudman and Bradburn, 1974). The "interview demand" hypothesis does not explain, however, continued forward telescoping even within the reference period for a survey, a phenomenon noted by Neter and Waksberg (1964) and in all of the victimization record checks.

For purposes of making accurate estimates of victimization rates for a calendar period, any disposition by respondents to draw into the reference period events which took place before (or after) it is more threatening than errors in time placement within the period. The more threatening phenomenon is known as "external telescoping." Various survey techniques have been developed to deal with this problem. One solution to this has been to "bound" surveys conducted for estimation purposes by an earlier interview. The bounding interview, which takes place at the beginning of the reference period, gathers reports of prior incidents and serves as a benchmark for the ensuing timespan. Interviews conducted at the conclusion of the reference period presumably are then protected from forward telescoping. In addition, incident reports gathered in the initial interview can be used to screen later interviews to eliminate duplications. Another aid to recall is to shorten the length of the reference period and to locate its terminal point as close in time as possible to the date of the interview. This increases recall accuracy (the demand for details about temporally distant events is eliminated) and limits the scope for backward telescoping. The trade-off, of course, is cost. Finally, external telescoping can be reduced by "bounding" the beginning of the reference period with a salient date. During interview pretests for the National Crime Survey it became apparent that people had difficulty locating events in time because of the absence of salient reference points. They appeared to remember incidents which occurred in January more frequently than many other months because they "came just after the first of the year" (Yost and Dodge, 1970). Interviews which refer to reference periods with natural boundaries marking their beginning and end seem to be more satisfactory.

Controlling telescoping within a reference period is much more difficult than controlling external telescoping, and there is no effort to do so in the National Crime Survey. There are, however, procedures which serve to enhance recall accuracy in retrospective surveys which are applicable in this case. Even in the complete absence of a bounding interview it may be possible to increase the accuracy with which respondents pinpoint the timing of events by assisting them in the construction of their own mental benchmarks. In a victimization survey in England Sparks et al. (1977) opened their interviews with a series of questions about respondents' recent activities. Londoners were asked about "things which you did in the past year," including vacations, major illnesses, births, marriages, and job changes. Exact dates were solicited for each of these events, which are usually highly salient in the minds of the participants. When victimization information was gathered later in the interview, the incidents were put within the context of these events. It would be ascertained, for example, that an assault took place between a holiday to Brighton and the marriage of a daughter. This appeared to increase the accuracy with which the dates of criminal incidents were elicited, for the known victim subsample in this survey (selected in the same fashion as those in the record checks) exhibited less telescoping than American victim samples.

The effects of external telescoping on victimization rate estimates can be considerable. In the 1970 Washington, D.C., record check, for example, some individuals were selected for interviewing because police files indicated that they had been victimized 7 months before. They were asked only about their experiences during the "past 6 months." About 15 percent of those out-of-bounds incidents were pushed forward into the reference period. Over 20 percent of a sample of 13-month-old cases were incorrectly placed within a 12-month reference period by another group of victims (Dodge, 1970). In July 1971, the issue was investigated experimentally. A victimization instrument was administered to 18,000 participants in the Quarterly Household Survey,

Table 6.
Victimization rate estimates based on bounded and unbounded national samples

Type of victimization	Rate estimates per thousand: Bounded panel	Unbounded panel	Percent* difference
All personal crimes	64.3	84.0	31
All violent crimes	16.4	22.7	39
Personal thefts	47.9	61.2	28
All household crimes	103.2	137.6	33
Burglary	42.8	57.1	33
Vehicle theft	9.0	11.8	32

*Calculated unbounded minus bounded, divided by bounded. Calculations based on unrounded figures.
Source: Wollman, Bushery, and Carstensen, 1975, Table 3. Data are for January to June 1973 recall period.

12,000 of whom had been interviewed about crime in January of the same year. The survey asked about their experiences "in the past 6 months." In every crime category, the 6,000 respondents whose interviews were unbounded reported more incidents than those who had been questioned before. The ratio of unbounded to bounded reports ranged from 1.2 to 1 for burglary to 1.9 to 1 for robbery. This was roughly the same magnitude of error due to telescoping in Neter and Waksberg's (1964) comparison of bounded and unbounded reports of household repairs: their unbounded interviews yielded 40 percent more reports of expenditures.

The design of the National Crime Survey enables us to make comparisons between bounded and unbounded interviews on a continuous basis. Table 6 reports on differences between estimates of the national victimization rate for several crimes based on interviews conducted for repeat, bounded samples and new, unbounded samples for the reference period January to June, 1973. In every case, the new, unbounded households entering the survey reported more instances of victimization than those which were already part of the sample used for estimation purposes; in the aggregate, the difference in rates was about 33 percent, a very substantial discrepancy attributable to this single methodological difference.

It should be noted that the data presented in table 6 actually understate the impact of bounding on telescoping. Many interviews in the "bounded" column in table 6 were in fact unbounded. In theory, individuals who are interviewed for the first time are questioned to establish an initial reference period and to gather data for use later to eliminate duplicate reports of events. In practice things are more complex. The U.S. Census Bureau conceives of the survey sample as a sample of addresses. Thus it treats an address as bounded when at least one person there has been interviewed. As a result, many respondents are treated as bounded when they have not been interviewed before. This group includes others at the address who were not interviewed when it first entered the sample, new persons in that household, younger household members who "age" into the sample, panel participants who were missed on a prior round of interviews, and—most important—entire households who move into a sample

address at some point after the bounding interview (Jacob, 1975). In an exhaustive analysis of several years of data from the National Crime Survey, Reiss (1978) concluded that in most periods between 17 and 19 percent of those interviewed were falsely considered bounded—there had been no interview 6 months before. The bulk of these interviews was with members of "replacement households" which had recently established themselves at the address.

The impact of the inclusion of unbounded interviews in the data used for estimation purposes is considerable. Reiss (1978) reports that fully one-third of all victimizations uncovered in the survey are reported in unbounded interviews. This is partially due to external telescoping and partially to the fact that people who move generally report higher rates of prior victimization than those who do not. Although the bounding procedure in principle controls for the former, the failure to implement it fully plays a large role in determining the apparent level of victimization in the United States.

In this regard it is also important to note that the city victimization samples interviewed by the U.S. Census Bureau were unbounded. The interviews conducted in 26 cities between 1972 and 1975 employed 12-month unbounded reference periods. In eight of the cities, the reference period also did not refer to a calendar year (January through December), which probably further detracts from the quality of the data. We do not know enough about the consequences of this to predict its impact on other measures. If more serious incidents were telescoped into the reference period (Reiss, 1978) while less serious ones were more rapidly forgotten (Neter and Waksberg, 1964), the relative mix of crimes as well as rates of victimization were affected. On the other hand, external telescoping should have proportionally less of an impact on reports gathered for a 12-month period than it does on the unbounded components of the National Crime Survey with its 6-month reference period. Respondent fatigue and forgetting should be greater over the longer span, however.

In addition to reconceptualizing bounding procedures, research on the telescoping process should focus on internal telescoping effects and on the correlates of telescoping itself. We know little about why events are telescoped or about their differential misplacement in time. In her record check in Portland, Schneider (1977) examined the kinds of events which were most severely moved about in time. Her survey employed a 12-month reference period. On the average, matched incidents were pulled forward within that period by 2.2 months. Forty-nine percent of all incidents were placed in the wrong month by their victims. She found a weak tendency for more trivial incidents to be telescoped forward more often, and for events which occurred more distantly in the past to be pulled forward more frequently. Also, crimes in which the victim reported resisting the offender often were misplaced in time. However, the tendency to move events forward in time was not related to the age, race, sex, or educational level of respondents.

Telescoping within a reference period presents analytic difficulties, for it impedes our understanding of the timing and sequencing of criminal incidents. Even within the 6-month reference period currently employed in the National Crime Survey, survey incidents apparently are being pulled forward in dramatic fashion. Twenty-eight percent of all incidents now are being placed in the first month of any recall period, four times as many as in any last month (Reiss, 1978). This destroys the utility of the data for examining issues such as the sequencing of multiple victimizations or the impact of recent experiences with crime on a victim's willingness to resist another attack or to report ensuing incidents to the police. Without accurate data on the temporal placement of incidents we cannot link them in causal fashion to other events, such as quitting a job, moving to another address, installing a crime-prevention device, or getting a divorce. To document the causes and consequences of crime at the microlevel we need accurate data on the relative time placement of many events in people's lives, including victimization.

Other sources of measurement error

Research on inaccurate recall has focused almost exclusively on the time placement of individual incidents. However, there is reason to suspect that victims are likely to recall inaccurately other aspects of events. Research in experimental psychology indicates that errors in recall increase as a function of the logarithm of time (Sudman and Bradburn, 1974). Record checks which match significant characteristics of incidents across police files and victim interviews would shed a great deal of light on the general reliability of the data collected in the surveys. The only record check of the characteristics of incidents that has been made by the U.S. Census Bureau focused on differences in estimates of dollar losses between victims and the Washington, D.C., police. That comparison revealed that citizens had substantially higher estimates of the value of their stolen and damaged property than did the police. Three-fourths of the loss estimates gathered in interviews were higher than those recorded by the police, often by 50 to 100 percent. On the other hand, there was no indication that these differences were time dependent or that the dollar amount of a loss affected the accuracy of its recall (Turner, 1970).

In the Portland record check, Schneider (1977) compared police and interview data on a variety of incident attributes. She found that survey estimates of loss and seriousness consistently were higher than police figures. Victims were much more likely than police reports to mention that weapons were involved in a case. Police reports and victims also disagreed much of the time on the race of the offender and, as noted above, on victim-offender relationships. Victims also reported substantially longer response times by the police than official records indicated. On the other hand, there was a good match for such factors as the age and sex of suspects and the number of offenders involved in the incidents. Interestingly, these mismatches were not consistently related to the passage of time. Some of the incidents were from 12

Other factors contributing to the omission of offenses during an interview may not be time dependent. Record checks strongly suggest that incidents involving close relationships between the parties are withheld from interviewers.

months in the past, yet none of the error in those comparisons (scored as measures of the difference between victim and police reports) was time dependent. Also, the passage of time was not related to the tendency of the victims to give "don't know" responses to questions about their experiences. Only knowledge of the date of the incident seemed to fade with time. It would seem that the criterion of accuracy employed in the survey pretest record checks was the most stringent of choices.

Differential productivity of respondents

Research on general survey methodology indicates that respondents also differ in their willingness or ability to adopt a productive role during an interview (Sudman and Bradburn, 1974). In general, more highly educated respondents are more cooperative, more at ease in interview situations, and more able to recall the details of events. Those factors may affect the accuracy with which victimizations are recalled during interviews.

As we noted above, it is assumed that most forms of criminal victimization are more frequent among lower status persons. However, surveys conducted for the Crime Commission found victimization to be positively related to measures of social class. The strongest social class correlate of victimization was education. College-educated respondents recalled victimizations at a higher rate than did others. This surprising pattern may be due to differing definitions of victimization and attendant variations in the probability that events will be recalled in an interview. On the other hand, researchers suspect that significant negative associations between social position and victimization are masked in the survey findings by greater interview productivity among more highly educated and test-wise respondents. Higher levels of education (but not income) measure entry into a "test and measurement culture" in which surveys, questionnaires, and opinion polls are recognized features of life. In addition, more educated respondents may enjoy greater verbal fluency of the kind necessary for conducting a bureaucratic encounter, and they may generally be more inclined to trust the stated intentions of inquiring government agents. Interviews with such respondents should be less perfunctory, involve greater task comprehension, and elicit more effort in completing the task than those with less comprehending or less able respondents.

There is also little evidence of the dimensions of the problem or of the credibility of this explanation for observed variations in victimization reports. In England, Sparks et al. (1977) found that among upper class respondents victimizations which were recalled were more likely to be trivial ones, or attempted rather than successful crimes. Similar findings have been reported for Germany (Stephan, 1976) and the United States (Biderman et al., 1967). In the National Crime Survey those proportions fluctuate considerably among those with lower levels of educational attainment, but are by far the highest for those with college training. In data collected during the first 6 months of 1977, 63 percent of all college-educated assault victims fell in the "attempted assault without a weapon" category; for everyone else that figure was 49 percent (author's computation).

The only other evidence that differences in the ability of victims to complete the interview task are affected by education was reported by Reiss (1978). He found that less educated respondents were more likely to recall incidents that fell into the "series" category, which is composed of crimes for which discrete details could not be remembered. On the other hand, Schneider (1977) found in her record check that education was not related to any tendency for victims to give "don't know" responses or to systematic differences between police reports and interview data on incidents. Based on this evidence, it seems that productivity effects must be only of the "recalled or not" variety and thus at work only in the screen section of the survey instrument. It remains unclear why nonrecall error ever should be distinct from errors in the detailed incident descriptions gathered in the incident report section of the instrument.

Summary

Conceptual and measurement decisions have had a substantial impact on the volume and nature of crime in America revealed by victimization surveys. Between 1973 and 1978 trendlines for the major categories of offenses measured by the National Crime Survey were flat, revealing little increase in crime over that 6-year period (U.S. Department of Justice, 1979b). This stability highlights the importance of conceptual and measurement problems, for they have had more impact on the apparent level of crime than all the events of the 1970's.

The use of proxy respondents serves to depress the apparent rate of victimization. When people are asked to recall events for others as well as for themselves, their own experiences predominate. In addition, using a household informant places a taxing burden on a respondent, and sheer fatigue may become an important factor shaping his or her productivity. Proxy effects also may account for the sharp increase in victimization rates currently recorded between the ages of 13 and 14, the point when the National Crime Survey shifts from proxy to self-responses for youths. In addition, proxy respondents are questioned to gather data about people whom U.S. Census Bureau interviewers cannot arrange to interview individually. Under many circumstances these hard-to-reach household members may be persons whose lifestyle would lead us to expect more frequent self-reports of victimization (Hindelang et al., 1978).

There is also evidence of considerable memory bias in the surveys, part of which is reflected in nonresponse and part in misresponse. It is useful to distinguish between the two, for there is some evidence that they reflect different recall problems. However, in a set of victimization data the two are inextricably linked. Without additional information it is impossible to disentangle time-dependent forgetting from internal forward telescoping, the two processes which conspire to produce apparently decreasing rates of victimization over the length of a reference period.

Sources of method variance in victimization data

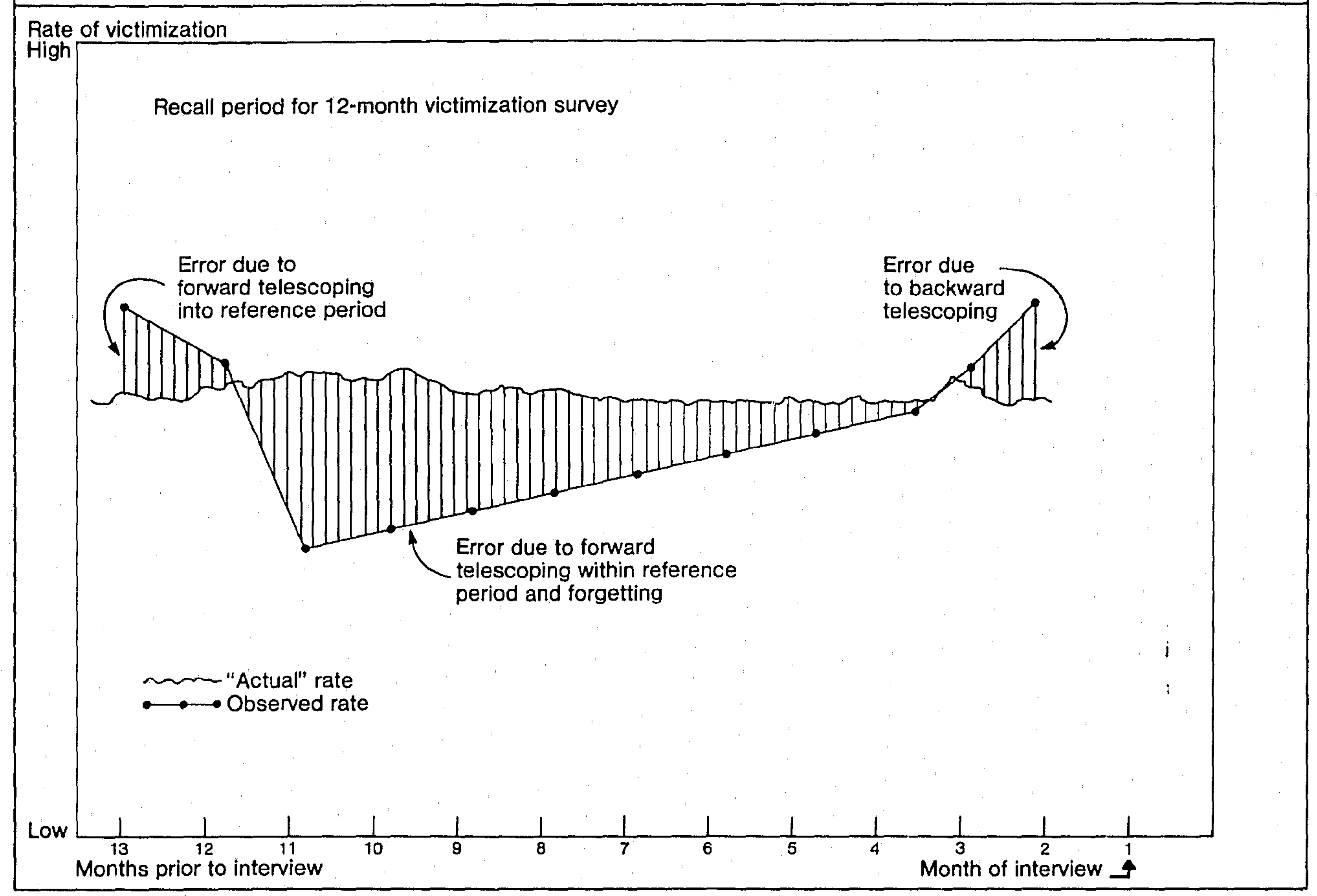


Figure 4

A summary of the impact of forgetting and telescoping is presented in figure 4. It plots a hypothetical "actual" victimization rate and one observed in a typical victimization survey. If a survey is unbounded, we expect to see an upward "tail" at the beginning of the reference period reflecting incidents which occurred before it began and are telescoped into it. The tendency of victims to forget events more distant in the past and to move them forward erroneously in time when they do recall them then predominates. Only in the recent past, within 3 months of the date of the interview (Turner, 1970), does the ability of victims to place properly criminal offenses in time approach 90 percent. Finally, the National Crime Survey and other surveys inevitably collect a number of interviews some time after the close of the reference period. This allows for the shifting of some events into the period which in fact occurred after its conclusion.

Other factors contributing to the omission of offenses during an interview may not be time dependent. Record checks strongly suggest that incidents involving close relationships between the parties are withheld from interviewers. However, the definition of those events as noncriminal may be an important problem as well, especially when they were not reported to the police. Experiments with various interviewing techniques indicate that respondent load and fatigue factors are quite consequential in retrospective studies and should be greater in surveys with longer reference periods or greater coverage of categories of victimization.

Evidence on inaccurate recall of the details of incidents is based on one record-check study. In it, temporal telescoping was again found to be a major problem, but error in the reporting of other aspects of offenses varied considerably with magnitude. Except for the date of the event, none of that misrecall was time dependent. Police and victims disagreed on the seriousness of crimes, the value of losses, the identity of offenders, and other aspects of what happened for other reasons, most of which are not clear. However, the independence of that disagreement from the personal attributes of victims performing the recall task should be heartening to the analyst.

Our very sparse data on differential respondent productivity are quite puzzling. On the one hand, education seems to be related to self-reports of victimization in unexpected fashion. On the other hand, education is not related to inaccurate recall of the detail of events or the tendency to give "don't know" responses to questions about that topic. This is consistent with most research on the quality of reporting in surveys, which indicates that measurement error is not strongly associated with demographic characteristics of respondents. Less educated respondents are more likely to generate reports of series incidents, which is congruent with the productivity hypothesis. But otherwise the presumed productivity effect seems confined to "remembering or not." It does not seem to lead to errors or failures of recall in events that are dredged up from the past.

Chapter 5

Procedural issues

The results of victimization surveys also are affected by procedural factors. These are difficulties inherent in fielding national retrospective surveys, the manner in which surveys are carried out on a day-to-day basis, and variations in survey procedures which arise in different places. In this chapter we consider three of the most important of these factors. First, we will examine some of the consequences of the panel design of the National Crime Survey, including panel bias and attrition. Second, we will explore the implications of variations in modes of interviewing, comparing telephone and in-person methods. Finally, we will review evidence concerning interviewer bias in victimization surveys. There are considerable gaps in our knowledge of some of these topics, but enough data are at hand to suggest that the manner in which the surveys are being conducted has considerable consequences for the picture of crime in America which emerges from the end product.

Panel bias and attrition

The National Crime Survey is a panel study. As a result, the findings are affected by a variety of panel effects that lead to systematic biases in victimization data. Panel biases are artifacts in data attributable to the fact that respondents are visited again and again in a survey. These biases may arise because respondents tire of the task and either suppress reports of things the interviewer wants to know or fail to exert the effort necessary to succeed at it. Panel bias also arises when participation in a survey affects the attitudes or behavior of respondents. In one-time, cross-sectional surveys this presents no difficulty if those effects come later, but in panel efforts it will affect subsequent readings of the sample.

Panel bias

Panel effects seem ubiquitous. In a review of the topic, Bailar (1975) reports that participation in prior interviews has depressed subsequent reports of household expenditures, repairs, and alterations. In addition, when the reports of new respondents to a survey are compared with those who are being reinterviewed at the same time, it was found that the recall of recent illness and of unemployment is lower among experienced survey participants. All of these effects seem to be attributable to panel fatigue. In Bailar's (1975) study incoming panel members reported rates of unemployment which were 20 percent higher than those reported by experienced respondents. (However, it may be that these differences are attributable to telescoping, for a new panel member is also "unbounded.")

In the National Crime Survey, respondents are scheduled to be interviewed seven times, at 6-month intervals. This is comparable to the Current Population Survey, in which respondents are to be quizzed eight times. Woltman and Bushery (1977a) took advantage of this survey design to compare victimization reports for groups with differing degrees of panel experience who were being interviewed in the same month. They found generally declining rates of reported victimization as the number of times that respondents had been interviewed increased. The largest drop was between the second and third interview, when reports of various personal crimes declined between 4 and 10 percent.

Lehnen and Reiss (1978) tackled the problem using special tabulations for individuals which merged their responses over a number of waves of the panel. They argued that panel participants could have contradictory effects: participants might become fatigued by continued interviewing and restrict their output as a result, or their ability to recall events could be enhanced by this unexpected continuing interest in their experiences on the part of the Government. Their data did not speak unambiguously to these alternatives, however. They found that the number of times respondents were interviewed was negatively correlated with reports of victimization. But they included the results of bounding interviews in this analysis and external telescoping doubtlessly greatly affected the outcome. They also found that reporting past victimizations—and thus learning of the connection between a "yes" response on the incident screen section of the interview and a difficult respondent task—was positively related to reporting victimizations in later interviews. We have seen that this could have several interpretations, but it does argue against strong test-wise task avoidance effects in the panel.

There has been much less research on the issue of how participation in a survey affects the subsequent behavior of respondents. Surveys which employ a panel design are particularly threatened by this form of bias. The issue is not that of accurate measurement but that panel participants are no longer representative of the population from which they are drawn if such effects are present. The Center for Political Studies at the University of Michigan conducts a biyearly election study using such a panel, and it has found to its distress that repeated interviewing has increased the proportion of panel members who are registered to vote (Traugott and Katosh, 1979). It confirmed that effect by a record check of voter registration lists. The only evidence on this issue available for the National Crime Survey relies entirely on the reports of participants. Those data indicate a substantial panel effect on one aspect of victim behavior. Experienced respondents are much more likely than first-time respondents to recall that they reported crimes to the police (Murphy and Cowan, 1976).

An important unanswered question is whether or not panel participation affects rates of victimization among panel members. The linkage between participation and victimization would be indirect, through the possible impact of panel membership on the adoption of self-protective measures. There is considerable correlational evidence that certain risk-reduction tactics—like staying home—can reduce individual and household victimization rates (cf. Skogan and Maxfield, 1980). If repeated questioning about crime, or interviews inquiring about the seriousness of various offenses, has had an impact on how people comport themselves during the following 6 months, the surveys may be underestimating the true level of victimization in the Nation.

Panel attrition

Biases in panel data also may reflect selective attrition in panel membership. Any panel study which extends over a 3½-year period inevitably will be threatened by the loss of participants. (Twenty percent of the American population moves each year.) Even panel studies which vigorously attempt to follow relocating families are hard pressed to preserve their original sample. The National Crime Survey panel currently is organized as a study of residents of sample addresses. No attempt is made to retain contact with departing households or individuals. As a result, a considerable proportion of the people initially interviewed at those addresses does not participate throughout the life of their panel. Those who replace them at that address are treated as "replacement households" and represent their dwelling units (in an initially unbounded state) in ensuing rounds of interviewing.

A problem arises because this residential mobility and the subsequent "replacement" procedure is far from random. In particular, it appears that moving to another address is a common response to victimization. Reiss (1978) first noted that a substantial proportion of victims interviewed as part of the National Crime Survey did not reappear in data collected at their address 6 months later. Less than two-thirds of those reporting a victimization remained in place, a figure substantially lower than that for nonvictims. The more victimizations one reported, the higher the chance that he or she would move or refuse to be interviewed later.

After reporting one victimization, 24 percent of all respondents "disappeared" 6 months later, most of them along with their entire household. For those reporting three or more victimizations, 35 percent of their households subsequently vanished from the panel sample. The data indicate that victimization rates decline with continued participation in the panel (Lehnen and Reiss, 1978). In light of selective attrition it is likely that this reflects the consequences of crime for movement out of the sample rather than crime's true distribution in the population.

Telephone versus personal interviews

While the National Crime Survey and the victimization surveys conducted in 26 cities are personal-interview studies, in each case a substantial proportion of interviews was conducted via the telephone. In the National Crime Survey, contact with a sample household is initially established by the personal visit of an interviewer. During this visit the interviewer lists each household member; at that time he or she also interviews all available respondents. However, the interviewer exercises discretion about whether to complete the remaining interrogations by other personal visits or by telephone, and is to choose the easiest and most cost-effective method (U.S. Census Bureau, 1979).

We do not have a reliable reading of the consequences of this procedure. Some comparisons of the results of interviews conducted personally and by telephone indicate that there are few differences between them. Comparisons of parallel surveys that have been conducted using the two methods sometimes indicate a similar equivalency, but sometimes favor one of the interviewing modes. No truly definitive experiment has been conducted detailing the consequences of mode of interview for data on victimization. Research on related topics also provides no clear lesson for the victimization surveys. There are reasons to suspect that telephone interviews may be less productive than those conducted in person, and there are counterarguments which support the use of the telephone. The best research on the topic can be read to support both conclusions.

It is widely argued that surveys of the general population achieve higher completion rates when the interviews are conducted in person. Because the U.S. Census Bureau pursues a mixed-mode data collection strategy to pursue individual noncompletions, it is impossible to talk about the relative effectiveness of each in the crime surveys. In its Health Interview experiments, the Survey Research Center of the University of Michigan found a 10-percent difference between both completion rates and refusal rates which favored the in-person strategy (Cannell et al., 1979). On the other hand, the Center achieved virtually identical results in another study of the two techniques (Groves, 1979). Those who favor personal interviews also argue that "the data are better" when collected in that way because of the greater rapport that can develop between interviewer and respondent. Also, in intimate settings, interviewers can supply more verbal and nonverbal cues to shape respondent behavior, and both parties may be more satisfied with the emotional rewards of face-to-face contact. Comparisons by mode of interview indicate that respondents and interviewers are less satisfied with telephone interviews (Groves, 1979; Cannell et al., 1979). Respondents tend to supply less detail in response to open-ended questions given over the telephone (Groves, 1979). They also are more likely to evidence response-set bias, using the same verbal category in answer to a string of questions more frequently when interviews are conducted by telephone (Groves, 1979). It also seems that respondents in telephone surveys are less certain of the sponsorship of those studies or of the use to which the data will be put. Rodgers (1976) and Groves (1979) both found they were less likely than those being interviewed in person to supply sensitive personal information such as family income. In the Groves study, telephone respondents also were more likely to report that they felt "uneasy" discussing selected topics.

. . . enough data are at hand to suggest that the manner in which the surveys are being conducted has considerable consequences for the picture of crime in America which emerges from the end product.

Vigorous arguments can be made in support of telephone surveys as well. Some have argued that telephone interviews may be more productive because they are anonymous. On the telephone it may be possible to be more candid and matter-of-fact about embarrassing issues, and it may be easier for respondents to admit less desirable behavior. In a record check of the two modes of interviewing, Rodgers (1976) found that telephone reports of whether respondents were registered to vote were more accurate. On the other hand, Groves (1979) found no difference between the two approaches in terms of the social desirability of responses to various measures. Telephone interviews may often be more discrete, for other members of a household usually cannot hear the questions asked. Because the work is conducted at a central site, telephone interviewing can be better supervised than can field visits. As a result, interviews may be more standardized. Rodgers (1976) found that interviewer styles were more uniform and interviewer effects on data were less pronounced over the telephone, and that across two waves of interviews responses by members of her telephone panel were more consistent. Interviewers are undeniably safer in telephone surveys, often a significant concern. Groves also found that completion rates for urban areas were higher for telephone than in-person surveys.

Evidence on the relative validity of data gathered in each way is important, for the mixed-mode data collection strategy employed in the crime surveys is distributed in a decidedly nonrandom fashion. During the first few years of the National Crime Survey about 25 percent of all interviews were conducted over the telephone (National Research Council, 1976), and persons who are interviewed by telephone are more likely than others to be young, male, and black. All other evidence suggests that these are among the most likely groups to be victimized. If telephone interviews are not as productive of reports of victimization as those conducted in person, the resulting rate estimates will be severely affected.

Research results

There have been four major studies of the problem in the context of measuring victimization. While they came to somewhat different conclusions, the best of them supports the use of the telephone. In the first study, the results of interviews with households in which maximum effort was made to employ only personal visits were compared with those in which telephones were used whenever feasible. No major differences between victimization reports from the two samples were apparent (Turner, 1977; Woltman and Bushery, 1977b). In another analysis, Tuchfarber and Klecka (1976) contrasted the results of parallel victimization surveys. They uncovered more victimizations over the telephone than the U.S. Census Bureau measured in person. However, Reiss (1978) analyzed several years of national data organized in panel fashion and—controlling for a host of other factors—found that telephones were 50 percent less productive than personal interviews.

The results of this research can only be labeled inconclusive. Rodgers (1976), Groves (1979), Klecka and Tuchfarber (1974), and others agree that there were few differences in relationships between variables gathered in differing fashion. The problem seems to be one of threats to the precision of estimates of the number of victimizations and other objects of interest. In 1966 Biderman et al. (1967) attempted to use the telephone to gather victimization data, but quickly abandoned the technique as inadequate. Since most recent and systematic evidence on the issue is ambiguous, additional research should be conducted if only because of the very large contingent of telephone respondents in the current crime panel. In the U.S. Census Bureau project, many persons in each "experimental" condition actually were interviewed by another mode. In the Klecka and Tuchfarber study two different organizations had conducted the parallel surveys and different sampling frames were employed for each, leaving room for a host of differences between the surveys in addition to the way in which interviews were conducted. Reiss' data are correlational and suffer from a lack of random assignment of mode of interview. Clearly an experiment is called for in which samples of individuals would be randomly assigned to groups and interviewed in different ways, in conjunction with a record check to provide an independent reading of what responses "should be."

This design was employed by Statistics Canada in a major methodological study of the validity of survey reports of victimization (Catlin and Murray, 1979). They sampled 1,525 crime victims from the files of the Edmonton, Alberta, police department. Victims were randomly assigned into two groups, one to be interviewed only by telephone and the other only in-person. An additional random sample of adults was drawn from the city directory and mingled with the two victim samples in order to disguise the true purpose of the survey from the interviewers. This insured that respondents would often have no victimizations to report. Parallel surveys were then conducted to assess the completion rates, costs, and accuracy of recall associated with each survey method. Statistics Canada found that telephone interviews were significantly less expensive to conduct—including an allocation of administrative expenses and other overhead costs, telephone interviews cost 70 percent less than in-person interviews. Telephone interviews also were as successful as in-person efforts at reaching respondents; the completion rates of the two parallel surveys were quite similar. Finally, there was virtually no difference between the two surveys in the proportion of known victimizations which were successfully registered in the interviews. The in-person interviews recovered 64 percent of the criterion incidents, and the telephone interviews 63 percent. As a result of this experiment, Statistics Canada employed telephone interviews in its large-scale 1979 study of criminal victimization in Vancouver, British Columbia.

Interviewer effects

In addition to panel artifacts and biases related to mode of interviewing, differences among survey interviewers in the way they carry out their task also shape the resulting data. Interviewer effects are but one of several sources of "correlated response variance" (Bailar, 1976). These effects manifest themselves as variance on indicators which is shared among respondents who were quizzed by the same interviewer. The effects of interviewers can be quite substantial, especially when survey personnel have had comparatively little training and are only minimally supervised. Interviewer effects reached epidemic proportions in the 1960 Census of Population, a technical rationale for accepting a cost-cutting move to self-enumeration by use of a mail survey in the 1970 Census. In the city victimization surveys conducted by the U.S. Census Bureau, interviewer effects were comparable in magnitude to sampling error. For example, for Baltimore it is necessary to multiply estimates of sampling variance by 1.60 to calculate confidence intervals which take account of both sampling and interviewer variance. The rate for all victimizations there was 110 per 1,000, with a sampling-error range (with 95 percent confidence) around that estimate from 40 to 180 per 1,000. Taking into account the effects of correlated response variance extended that range to from 20 to 200 per 1,000. Those differences become even more extreme when we examine particular crimes (Bailar et al., 1977).

The sources of interviewer effects are numerous. Interviewers differ in how they interpret individual survey items and in their understanding of the purpose of the enterprise. Some probe for detailed comments more vigorously than do others, and some interviewers readily accept "don't know" and other nonresponses. Interviewers also differ in how they interpret and record responses to questions and how they explain individual items to respondents who do not understand them. Often they do not link the verbal and nonverbal cues they give respondents to any productive effort on the respondent's part, thus rewarding unacceptable task behavior (Cannell et al., 1979).

Examination of the types of incidents for which interviewer effects are most substantial suggests that they involve the particularly sensitive topics probed by the victimization surveys. The most systematic analysis of those effects indicates that they are greatest for crimes in the "assaultive violence without theft" category—that is, for rapes, intrafamilial disputes, and public brawling (Bailey et al., 1978). Dodge and Lentzner (1978) noted that reports of series incidents often are first recorded when a new interviewer takes responsibility for a household. Presumably some interviewers are better than others at eliciting reports of "conditions" rather than events, while others more quickly tire of attempting to untangle vague or complex incidents.

Precise estimates of the magnitude of interviewer artifacts in the data are based on "interpenetrated sample" research. In each of the eight cities studied by the U.S. Census Bureau in 1975, interviewers were assigned batches of 80 sample households. A portion of these were randomly assigned from a pool of households distributed between pairs of interviewers. Then, comparisons were made in the data collected from these households, examining contrasts among interviewers. The analytic question was, How much of the observed variance in reports of victimization could be attributed to interviewers rather than to sampling variance and the true distribution of crime (Bailey et al., 1978)?

There was considerable disparity in reports of victimization among interviewers, between interviewers assigned to the same supervisor, and across cities. Interviewer effects were most extreme in Newark, where it is necessary to multiply estimates of sampling variance by 2.4 to take this additional source of error into account. Interviewer effects were most extreme for assaults and petty theft. Hearteningly, they were not linked to the attributes of the respondents themselves (Bailey et al., 1978). As a result, such effects will have fewer consequences for tabulations of relationships in the data. Also, the impact of interviewer variance on a set of data goes down as the number of interviewers in a study increases and the average number of respondents each one deals with decreases. Thus interviewer effects are much less significant in the National Crime Survey (Bailar et al., 1977). Conversely, telephone surveys typically employ only a few centralized interviewers, and the impact of differences among them will thus be more substantial.

Chapter 6

Assessment

Current state of the art

The National Crime Survey shoulders a difficult task, that of measuring the extent of a complex social process. Most sample surveys confine themselves to more manageable topics: they elicit information about the attributes of respondents, or they inquire about simple behaviors. The victim surveys examine confrontations between persons. Those interactions are complex and may take numerous forms. They are subject to various interpretations by their participants, and those interpretations are often commingled with the willingness of the actors involved to remember or report them to an interviewer. The experience of the Bureau of Labor Statistics in devising a measure of a seemingly straightforward concept such as unemployment suggests the difficulty of probing such phenomena. It took decades of research effort to arrive at the current (and still controversial) procedure for examining labor force participation, and in the process researchers were forced to drop all efforts to tie the concept to interpretations by respondents of their own status. People's assessments of their own willingness to work have been replaced by inquiries about specific job-seeking behaviors, an approach not unlike that adopted in the crime surveys (De Neufville, 1975).

The varieties of human experience are endless. The National Crime Survey has chosen one reasonable approach to imposing some order upon that rich empirical domain. Methodological criticisms of the effort impose a strict standard upon the data, that of criterion validity. Validity is a question of the relationship between two distinct measures of the same variable; if different nonsurvey measurement procedures identify (in this case) the same events or victims, we are more confident that the data are not artifactual, generated by the measurement process itself. However, few of the measures commonly employed in survey research have any known validity. Survey data which are good by standards of the profession usually display, at most, internal consistency, and are related in expected ways to benchmark attributes or attitudes of the respondents. (This is known as construct validity.) The validity of self-reports even of simple behaviors is often quite low: claims about having voted often are inflated by 10 or 20 percent (Traugott and Katosh, 1979; Clausen, 1968; Weiss, 1968). In one study 47 percent of the sample misrecalled whether or not they gave money to a Community Chest drive (Cahalan, 1968). Any socially approved behavior will be claimed by more persons than actually practice it. Biderman and Reiss (1967) summarized a study which reported that 30 percent of a sample of persons known to have visited a doctor within 2 weeks prior to the interview failed to report the event, and that 7 percent of a sample of recently hospitalized persons exhibited similar lapses in memory. Forward and backward telescoping affect the reporting of vacation and sick leave and self-reports of household expenditures.

The purpose of this review is not to expound on the difficulties of conducting survey research, but to suggest that a bounded survey using an interview schedule with an overall recovery power of 75 percent or better is well within the normal range of the instruments of social science.

This assessment reflects a more general position about data—they always contain errors. Data are indicators of the relative distribution of some conceptual variable across a population. The numbers themselves only partially reflect that distribution (their true score component). They also are partially artifacts of the measurement method (their method component), and they are clouded by random noise from a variety of sources. In dealing with data the issues always are: Is the error component of the data truly random with respect to the relationships I am investigating? Am I being led astray by misinterpretations encouraged by method effects? The more we know about a set of data, the more confident we can be when we answer these questions.

Victimization research has come a long way in this regard. As a result of the methodological research described in this volume we know a great deal about errors in victimization data. The measurement of predatory personal crime, stranger violence, and serious property crimes appears to be satisfactory. The recovery power of the survey instrument is relatively high for most crimes in these categories, and interrespondent differences in interview productivity and interviewer effects are a less serious problem in these areas as well. Thus, despite all of their difficulties, the data generated by the crime victimization surveys have enormous potential for clarifying many issues in criminology.

The largest problem area remains the data on assault. While the complexity of victimization survey data demands that we interpret all the data with care, the methodological shortcomings of the enterprise all seem most to affect reports of interpersonal violence. We have seen in record checks that many assaults are not picked up in personal interviews, even when they have already been reported to the police. In the Baltimore method test, only 36 percent of all assaults were recovered in the interviews, and less than one-half of those were placed in the correct month by their victims (Yost and Dodge, 1970). In San Jose, 48 percent of the victims of assault recalled the event, but that percentage dropped to 22 percent among those who were victimized by acquaintances or members of their own family (Turner, 1972a). It was apparent in the pretests that the interview process was not eliciting thorough accounts of interpersonal violence and that the problem was acute in the case of nonstranger assault.

We also have seen the unexpected relation between education and reports of assault victimization, a relationship which leads us to suspect that more educated respondents are most likely to remember such offenses, to define them as crimes, or to cooperate in their reconstruction in interviews. Series victimizations, which ordinarily are excluded when data from the National Crime Survey are analyzed, almost all involve assaultive violence. This leads to the severe undercounting of assaults and greatly reduces the apparent frequency of multiple violent victimization. Because series offenses are more likely to be reported by less educated respondents, they further cloud the relation between education and victimization violence. Interviewer effects also hit hardest at events in this category, overwhelmingly in the direction of undercounting them (Bailar et al., 1977; Graham, 1974). Finally, panel attrition also probably disproportionately reflects assault victimization. Especially when the incidents involve neighbors or related parties, violent assault should propel victims to seek refuge in other domiciles.

These assault-linked shortfalls in the data seem to be reflected in a number of puzzling aspects of victimization research. They undoubtedly account for the high proportion of assaults attributed to strangers in the national panel and in the city studies. Across the 26 cities where surveys have been conducted, an average of 70 percent of all interpersonal violence was attributed to strangers; in the 1973 national data the figure was 60 percent (author's computation).

Interpersonal violence

These figures do not correspond with what is known about the dynamics of interpersonal violence. The evidence from other sources is that a much higher proportion of assaults, and even rapes, takes place within friendship and family circles. Numerous studies of police homicide files suggest that strangers account for only about 25 percent of all urban murders. Homicide and assault are similar in origin and process, differing primarily in their outcome—which is often a function of such factors as the caliber of gun employed or the availability of a doctor (Zimring, 1972). Curtis' (1974) survey of official records in 17 major cities found that only 21 percent of all assaults in 1966 were attributed by investigating officers to strangers. These proportions are similar to those revealed in numerous crime-specific studies of police file data. They render the survey data even more suspect because we also believe that violence between friends and relatives is less likely than stranger violence to be reported to the police. That police files contain approximately 3½ times more acquaintance violence than revealed in interviews does not add to our confidence in the validity of the survey findings.

Another bit of evidence relating to the reliability of the data on violence is more qualitative, but equally persuasive: the data do not square with what the police do. On a Friday night in any big city a large proportion of squad car dispatches are made in response to complaints about domestic disturbances. Frequently these do not lead to an arrest, and rarely do the participants end up in court, but the police often are called on to defuse potentially dangerous confrontations within families and to restore peace in the immediate environment.

There are several other method artifacts which shape data on aspects of assault victimization. Method artifacts probably account for the fact that in the National Crime Survey data victim-offender relationships do not appear to affect the rate at which victimizations are reported to the police. This is a surprising—and puzzling—finding. Given the low proportion of violence within close interpersonal networks which surfaces in the survey data, that which does is probably of such a character that it is also readily reported to the police. Second, the relative dearth of nonstranger offenses in the data undoubtedly increases the proportion of assaults and rapes which was reported to have involved victims and offenders of different races. To the extent to which people are likely to know or live near persons of the same race, the underestimation of violence among acquaintances will skew the data in favor of interracial crime. Given the potentially unsettling social consequences of high levels of interracial crime in America, artificially high reports of the rate are unwelcome.

Error in the measurement of interpersonal violence which is related to the differential productivity of respondents may account for the observation that blacks recall far fewer reports of minor assault than do whites. The most trivial form of violence in the crime survey is "attempted assault without a weapon," which includes incidents which resulted in no injury and in which no weapon was brandished. That a crime even occurred is inferential, and most of these events may better be described as threatening encounters. There is no particular reason to expect blacks to experience fewer of these episodes than whites; in fact, given what we know about class- and culture-linked youthful exuberance it is more plausible to expect the opposite. Yet in the 1973 data, fully 47 percent of all the assaultive violence recalled by whites fell into this category, while only 31 percent of the assaults reported by blacks were trivial. Almost 60 percent of all black assault victimizations were categorized as aggravated assault (involving serious injury or the use of a weapon) while only 37 percent of all attacks on whites were aggravated. This is highly unlikely. Much more plausible is the hypothesis that blacks reported fewer of their less threatening encounters, while whites dredged up everything in memory. Further investigations of the correlates of respondent productivity are required before we can make any confident statements based upon much of the data on assault.

The hypothesis that the experiences of blacks are substantially undercounted in the victim data accounts for one of the most anomalous patterns appearing in the city data when the data are used in aggregate form. At the city level, using the 26 surveyed communities as the units of analysis, we find that one of the best predictors of rates of violence is "percent white." The higher the proportion of the population which is white, the higher the rate of interpersonal violence. Because data on race are highly correlated at the city level with other social indicators, including those measuring the extent of poverty, educational failure, and the

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quality of life, we also find that high rates of interpersonal violence are positively associated with good housing, low population density, high income, and high levels of formal education. This is quite unlikely. It seems rather that white communities were represented by samples of white respondents, and that they produced more exhaustive reports of events and a more thorough recounting of essentially trivial events.

Future developments

This review of the methodological development of the National Crime Survey suggests a number of specific research tasks. Some of them will require broad-gauged experimental trials of alternative data collection techniques. Others will require record checks of the reports of victims and witnesses of crime. These studies are necessary if the findings of victimization surveys are to achieve the same general acceptance as other social indicators, such as the Current Population Survey (unemployment, household composition) and the Health Interview Survey (medical expenses and demands on health resources). These surveys have a long history of research and development. The Health Interview Survey, for instance, began in 1957 and has achieved a maturity which the National Crime Survey can as yet only aspire to.

Some research should focus on response validity. Record checks can be used to validate improved techniques for gathering reports of dollar losses for determining the attributes of offenders and calculating indicators of crime seriousness. Some efforts also must be devoted to the validity of the data concerning whether or not incidents are reported to the police. These data frequently are used to "correct" official crime reports in local jurisdictions (cf. Schneider, 1976; Skogan, 1976b), but we have no knowledge of the accuracy of reports of police notification.

Significant amounts of response error seem to have social sources. We need to examine more closely the underreporting of related-party crimes. If this underreporting can be traced to the differential definition of events, then appropriate changes on how the survey's task is described to respondents and in the memory jogs supplied in the incident screen may lessen the problem. If the difficulty turns out to be that victims recall such incidents but refuse to share them with interviewers, then we may experiment with random response techniques and other strategies for granting respondents greater anonymity and confidence in the confidentiality of the survey. Finally, the issue of differential interview productivity is an important one. The positive relationship between education and victimization clouds the analysis of the race and social class correlates of experience with crime. Improved interview techniques may assist less educated respondents to perform properly the difficult recall task the survey now demands.

One of the major components of the future research agenda for the National Crime Survey concerns the optimal length of the recall period. The shorter that period the larger the size of the sample which is required for the survey. The survey now is very large, yet there is evidence that there is significant nonrecall during months 4 to 6 of the reference period. One response to the problem could be to shorten the recall period, and thus further decrease the dimensions of respondent burden. The cost implications of this are substantial. This lends even greater importance to the development of alternative techniques for facilitating more thorough recall in retrospective surveys. In particular, using respondent-defined anchoring events scattered throughout the recall period seems to enhance the ability of respondents to remember crimes, as well as to more accurately place them in time (Sparks et al., 1977). This technique also would enable us to gather better information on the timing and sequencing of victimizations and other significant events during the recall period. Because some of those events may be read as consequences of crime, this would increase the general analytic utility of the data.

The coverage of the survey (in its broadest sense) also is a lively issue. Currently the National Crime Survey confines its coverage only to selected predatory crimes and some forms of assaultive violence. Many other forms of harm or potential risk do not fall within the scope of the survey, including obscene and threatening telephone calls and vandalism. Each of these, of course, presents added definitional problems, but surveys in other nations have indicated that both are widespread problems. In Australia obscene and threatening calls present substantial multiple victimization problems (Australian Bureau of Statistics, 1975), while in Vancouver, British Columbia, vandalism was 37 percent more frequent than burglary (Corrado et al., 1979). Expanding the coverage of the survey would not only improve the utility of the data, but it doubtless would enhance the overall recovery power of the questionnaire. As Biderman (1970a) has noted, "people experience incidents, not offenses," and "neither nature nor the minds of respondents packages events neatly in accordance with Uniform Crime Report offense classes." The more respondents are encouraged to ruminate about their experiences, the more offenses of all kinds they are likely to recall, based on linkages that may not be logical from a bureaucratic perspective. We see evidence of this in the lack of correspondence between screen-questionnaire items and the kinds of crime they currently stimulate victims to remember. It also is reflected in studies of the impact of adding new crime-related questions to the basic survey instrument; more stimuli concerning crime served to enhance recall almost regardless of their specific content.

Series incidents

The current treatment of series incidents is indefensible. By definition series offenses occurred at least three times, and the description of the latest of them must be clear enough to classify it as falling within the purview of the National Crime Survey. Not to count them at all when generating estimates of victimization rates is difficult to justify. In addition, there are many important questions about patterns of multiple victimization which cannot be resolved without better data on events of this type. For example, whether or not offenders are the same across series incidents would be extraordinarily revealing of the character of multiple victimization (cf. Reiss, 1977a, 1978).

By their nature we will never be able to distinguish clearly between many of the incidents these series reports represent. It appears that vigorous interviewer training and supervision can reduce the frequency with which they are recorded. Series incidents declined in number during the first years of the National Crime Survey, probably due to increasing interviewer experience with the survey (Reiss, 1978).

The inadequate representation of series offenses in the data used for estimation purposes partially explains the apparent paucity of multiple victimization in the population. Better handling of the data on such incidents would greatly increase our understanding of victim proneness. However, multiple victimizations do not necessarily take place within neat 6-month packages, and a thorough portrait of the sequential connection of events requires linking reports from individuals in the National Crime Survey across successive reference periods. This is beyond the current capacity of the U.S. Census Bureau's data system for the National Crime Survey.

Procedures for measuring multiple victimization also mitigate against finding it. Currently we discern that a type of crime has affected an individual more than once via responses to the question "how many times?" which is posed following a positive response in the incident screen. This is a very weak operationalization of the concept of crime-specific victim proneness, one which probably encourages overreporting of series events. Further, the bulk of the research done on multiple victimization has employed data from the city victimization surveys. The 1-year reference period for those surveys seems more productive for studying multiple events. However, with longer reference periods the problem of respondent fatigue also should affect the apparent frequency of multiple victimization, biasing estimates in a downward direction.

The issue of panel attrition is intimately linked to the fundamental conceptualization of the National Crime Survey. If the survey were organized around individuals, the movement of households and individual household members would trigger followup efforts. The current address orientation of the survey precludes following mobile respondents for a reasonable period of time, greatly limiting the utility of the panel data which presumably it is designed to collect. It also leads to the current definition of a "bounded interview," which has little to do with the social-psychological process of telescoping which it was designed to protect against. Selective panel attrition appears to affect estimates of the level of victimization made from the survey, and limits the utility of the data for studying one extreme reaction to crime, namely, moving elsewhere.

Finally, there needs to be a great deal of research on the implications of telephone interviews for the National Crime Survey, both for the data as they are currently collected and for the future organization of the program. As the cost of conducting the survey mounts, there inevitably will be pressure to convert to a telephone survey. Many local victimization surveys are now conducted by telephone (see Abt Associates, 1977; Skogan, 1978; Statistics Canada, 1979). We know little about the implications of the use of the telephone for undercoverage, nonresponse, and response error. We should be prepared to speak to the costs and benefits of the use of telephone interviews at the national level.

These and many other issues are now being considered by the U.S. Census Bureau and the Bureau of Justice Statistics. A large-scale project to redesign much of the National Crime Survey has been underway since 1979, directed by Albert Biderman of the Bureau of Social Science Research, Inc., in conjunction with researchers and users of victimization data from throughout the nation. The research to be conducted by this group should illuminate many aspects of the data which already have been collected in the national and city surveys, as well as serve as a model for conducting future victimization surveys.

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