

Summer 1981

## Methodological Problems in Victim Surveys and Their Implications for Research in Victimology

Anne L. Schneider

Follow this and additional works at: <https://scholarlycommons.law.northwestern.edu/jclc>

 Part of the [Criminal Law Commons](#), [Criminology Commons](#), and the [Criminology and Criminal Justice Commons](#)

---

### Recommended Citation

Anne L. Schneider, Methodological Problems in Victim Surveys and Their Implications for Research in Victimology, 72 J. Crim. L. & Criminology 818 (1981)

This Symposium is brought to you for free and open access by Northwestern University School of Law Scholarly Commons. It has been accepted for inclusion in Journal of Criminal Law and Criminology by an authorized editor of Northwestern University School of Law Scholarly Commons.

# METHODOLOGICAL PROBLEMS IN VICTIM SURVEYS AND THEIR IMPLICATIONS FOR RESEARCH IN VICTIMOLOGY\*

ANNE L. SCHNEIDER\*\*

The purpose of this paper is to examine several of the more serious methodological problems in victimization surveying, with particular attention to the implications of certain measurement problems for basic research in victimology. Most of the paper deals with three aspects of measurement error: the amount of error contained in survey-generated estimates of victimization; the net direction of that error; and the correlates of error. Errors in survey data concerning the identification of persons as victims will be the primary focus.

## OVERVIEW OF THE MAJOR METHODOLOGICAL PROBLEMS

As in any kind of survey approach, regardless of the specific topic under consideration, most of the methodological problems in victimization surveying fall into one of three categories: problems of sampling, problems in measurement, and problems of inference.

A fundamental methodological problem in victimization research is that surveys of the general population are not productive. Crime, especially serious personal crime, is a relatively rare event. Only samples of considerable size yield enough victimization incidents of any particular type to permit detailed and meaningful study. Alternative methods of sampling, such as beginning with known victims from police files or from victim programs of some type, are more efficient in generating victims, but suffer from other kinds of problems. These samples contain only known victims, those who reported their victimization to the authorities or the program. The lack of representativeness of these victims vis-a-vis the total population of victims is further increased by difficul-

---

\* This article benefited greatly from insightful reviews and comments by Michael Gottfredson, Al Biderman, James Garofalo, and David Griswold. All remaining errors are the author's responsibility.

\*\* Institute of Policy Analysis, Eugene, Oregon.

ties in locating them for the survey interview.<sup>1</sup> Research results based on these samples may not be applicable to the full population of victims.

Another fundamental methodological problem with surveys of victims is that researchers often attempt to develop explanatory or predictive models, or they seek to test propositions derived from causal theories, using data from a survey of a single point in time rather than a panel design. The designation of certain variables as independent or as dependent may be arbitrary and the direction of causality impossible to ascertain. This problem is particularly acute for studies in which victimization is the dependent variable and the respondent's attitudes or behaviors are used as explanatory variables. The behaviors and attitudes are measured at the current point in time, whereas the assumed victimization occurred prior to the interview. When victimization experiences are the independent variables, however, the problem is more tractable.

The third broad area of methodological problems, which is the central focus of this paper, concerns the amount of variance in the victimization variable that is true variance and the amount that is error. Whether the error is produced by a lack of reliability or by a lack of validity is not particularly important; what is important is that measurement error can influence the conclusions drawn from research studies. Unless the investigator is aware of the nature of the error and its implications, erroneous inferences can occur.

#### IMPLICATIONS OF MEASUREMENT ERROR

The implication of error for the research depends on whether it is random or directional, and whether it is correlated or uncorrelated with other variables of interest to the investigator. The primary impact of random error, that is, error which is not correlated with other variables of interest to the investigator and which has a mean of zero, is that it reduces the likelihood of finding significant differences between variables when, in fact, such differences exist.

In a similar way, random error reduces the strength of measures of association such as the correlation coefficient, regression coefficient, non-

---

<sup>1</sup> The lack of representativeness of the sample, beginning with victims known to authorities or known to victim-oriented programs, depends on the proportion of all victims known to these authorities, the response rate of persons contacted for the purpose of interviewing, and the extent to which persons actually interviewed differ from both the non-respondent and nonreported groups. The reverse records checks, especially London and San Jose, contain information on characteristics of victims who could not be located for interviews. *See* R. SPARKS, H. GENN & D. DODD, *SURVEYING VICTIMS* (1977); A. Turner, *San Jose Methods Test of Known Crime Victims* (Nat'l Inst. of Law Enforcement & Crim. Just. Statistics Div. Rpt. No. 1, 1972).

parametric measures of association (such as gamma, sommer's d, lambda, etc.), and other similar statistics. For example, the maximum correlation coefficient that can be obtained between two variables is estimated to be the square root of the product of the non-error variance (reliability) of the variables:<sup>2</sup>

$$r_{\max_{ab}} = \sqrt{(\text{rel}_a) (\text{rel}_b)}$$

The principle is straightforward; measures of association are based on the extent to which one variable can explain the variance in another. If part of the variance is random error, then by definition, this portion of the variance cannot be explained by any other variable. Thus, the maximum variance available to covary with some other variable is reduced. The practical effect is that when the amount of error is high, even though randomly distributed, the researcher's measures of association and tests of significance are too conservative, and biased toward finding no relationships even if they exist.<sup>3</sup>

A second problem pertains to directional error, that is, the mean of the error is either positive or negative. If this error is not correlated with other variables, the implication is that the investigator's description of the concept measured by the variable will be distorted. For example, there is evidence that the amount of loss estimated in victimization surveys may be exaggerated. The mean of the error, then, would be positive, and one of the implications of the error is an overestimate in the amount that victim compensation programs would cost.

Correlated error particularly concerns researchers who are examining relationships among variables. Two kinds of correlated error should be distinguished. First, the absolute amount of error in a variable can be correlated with other variables of interest to the investigator. For example, certain types of victims may make more errors in the recall of the crime than do other types of victims. Consequently, the amount of error differs, and the investigator is likely to find that relationships which hold for one type of victim may not hold for the other. Although this phenomenon could be produced by real differences, it is also produced by different validity of the data for different types of victims.

---

<sup>2</sup> See H. WALKER & J. LEV, *STATISTICAL INFERENCE* 303-05 (1953), for a discussion of this coefficient.

<sup>3</sup> The principle can be extended to multivariate models, and in general, variables with greater error will show lower regression coefficients than variables with less error. The significance of this finding is particularly important in studies where the researcher is attempting to compare the relative impact of variables with different error variances. For example, measures of attitudes, opinions, and perceptions contain more error than do factual attributes of victims such as race, sex, and employment, and more error than factual characteristics of offenses.

Other practical problems are introduced when the absolute amount of error is correlated with other variables. For example, attempts to replicate results, or to find consistent results in several different data sets, may be thwarted because of different amounts of error in the data being used. Attempts to demonstrate consistent patterns of relationships may be confounded for the same reason.

A second, and perhaps even more troublesome, type of correlated error exists when the direction of error in one variable is related to another variable being used by the researcher. Suppose, for example, that the problem of under-reporting of crime is related to age in such a way that older persons tend to forget incidents more than younger victims do. The result would be that the relationship between age and frequency of victimization is confounded with the relationship between age and memory decay.

#### IDENTIFICATION OF VICTIMS IN GENERAL POPULATION SURVEYS

Of all the methodological problems confronted by the field of victimology, none is more critical than a proper determination of who has been a victim of crime. Even assuming that the investigator can settle such issues as which behaviors or events constitute victimization, there still are problems in developing adequate measures. The problems of non-recall and telescoping have been recognized for years as major contributors to the misidentification of victims as nonvictims and vice versa, but the enormous difficulties in studying these problems have generally thwarted efforts to develop estimates of validity for the categorization of persons.<sup>4</sup> If researchers obtained a true measure of victimization, then the data from surveys and police records could be compared directly to the true measure and the extent of error could be determined. Figure 1

---

<sup>4</sup> The early pilot studies, P. ENNIS, *CRIMINAL VICTIMIZATION IN THE UNITED STATES: A REPORT OF A NATIONAL SURVEY* (Nat'l Opinion Research Center 1967); Reiss, *Public Perceptions and Recollections About Crime, Law Enforcement, and Criminal Justice*, in 1 *STUDIES IN CRIME AND LAW ENFORCEMENT IN MAJOR METROPOLITAN AREAS* § 2 (1967); A. Biderman, (A Pilot Study of) *Public Survey Approaches to Crime Phenomena—Report on a Design for National Study* (BSSR 382, April 1966), identified most of the methodological problems in victim surveys. At this time, there have been four reverse records checks and one forward records check of crime victims. A reverse records check begins with a sample of known victims and measures the efficiency of the survey technique in capturing the events and information about them. A forward records check begins with the general population. Persons who say they were victims are tracked through the official records. The reverse records checks were done by R. SPARKS, H. GENN & D. DODD, *supra* note 1, in three areas of London, and A. Turner, *supra* note 1, for the LEAA in San Jose. Two additional reverse records checks were done by the LEAA in Washington, D.C. and Baltimore. Very little information is available about the latter two. The forward records check was conducted by A. Schneider, *The Portland Forward Records Check of Crime Victims—Final Report* (Ore. Research Inst., April 1977), in Portland, Oregon, from victimization survey data that had been collected earlier for different purposes.

**FIGURE 1**  
 TYPES OF ERRORS IN SURVEY AND POLICE DATA

		TRUE CATEGORIZATION	
		Victim in Reference Period	Not a Victim in Reference Period
<b>SURVEY ESTIMATE</b>			
victim in reference period	(a)		1. external forward telescoping  2. exaggeration or lying
not a victim in refer- ence period		3. non-recall, lying, underestimate of situation	(c)
		4. external backward telescoping	
		TRUE CATEGORIZATION	
		Victim in Reference period	Not a Victim in Reference Period
<b>POLICE ESTIMATE</b>			
victim in reference period	(a)		5. exaggeration of situation, lying
not a victim in refer- ence period		6. non-reporting by victim	(c)
		7. non-recording by police	

displays different kinds of misidentification problems that occur in survey data, and for comparison purposes, in police data.

In the first two-by-two table, the cases falling on the main diagonal (cells *a* and *c*) have been correctly classified, and those on the off-diagonal are incorrect. The sources of error for the incorrect categorization are shown in Figure 1. In the lower part of the figure, the two-by-two table shows the sources of error in police estimates. Again, cases falling in the main diagonal are correctly categorized, whereas those in the off-

diagonal are incorrect. In addition to the types of error shown in Figure 1, there are some victims who do not report the crime either to the police or to the interviewer. These individuals would be categorized incorrectly in both the police and survey data.

Table A contains information from four reverse records checks, one forward records check, and other methodological studies that can be used to make rough judgments about the magnitude of error in the victimization surveys and in the police data. The amount of error in survey data depends, in part, on the survey methodology, such as the quality of interviewing, questioning procedures, length of reference period, and sampling frame. Thus, the four reverse records checks are not directly comparable to one another, and the forward records check is not comparable to any of the reverse records checks.<sup>5</sup> Nevertheless, the figures provide rough ideas of the amount of error in studies using victimization as an independent or dependent variable.

#### EXTERNAL FORWARD TELESCOPING

External forward telescoping occurs when respondents place an event forward in time, in the reference period, when in fact it occurred prior to the reference period. Estimates of the magnitude of external forward telescoping, measured as the proportion of persons categorized as victims who actually were victims prior to the reference period in

---

<sup>5</sup> The reverse records checks are not comparable to one another because: the length of recall period differs; the questioning procedure was different; the length of the interview varied; and the interviewing contact procedures differed. Perhaps most important, the surveys differed in the types of crimes covered. The San Jose results were weighted so that each offense contributed to the overall scores for the survey in relation to its contribution to the initial sample, but in none of the other studies was the sample weighted so that it reflected the original sample, correcting for non-response, or so that it reflected offenses as represented in official data. See R. SPARKS, H. GENN & D. DODD, *supra* note 1; A. Schneider, *supra* note 4; A. Turner, *supra* note 1. Since the amount and type of error differ by offense, this lack of equivalency is especially important but virtually impossible to correct in secondary analysis. The Baltimore and Washington studies were the first, and were not as accurate as the San Jose or London studies. For information on the NCS study comparing bounded and unbounded surveys, see H. Woltman & J. Bushery, A Panel Bias Study in the National Crime Survey (paper presented at the Am. Statistical Ass'n Meetings, Aug. 25-28, 1975); H. Woltman, J. Bushery & L. Carstensen, Recall Bias and Telescoping in the National Crime Survey (Census Bureau Statistical Methods Div. Memo, Sept. 23, 1975). The technique used to estimate external forward telescoping in this study was to compare the victimization rates of the bounded part of the sample with the unbounded portion. The difference, presumably, would represent the extent of external forward telescoping into the reference period. Of course, there are other operative factors in this comparison, such as population mobility rates of the bounded and unbounded portions with the corresponding likelihood of different rates of victimization. The Census Bureau report does not discuss how these other possible contaminating factors were dealt with. See Lehren & Reiss, *Response Effects in the National Crime Survey*, 3 VICTIMOLOGY 110 (1978).

unbounded surveys, range to 25% (see Table A) for reference periods of six months, and to 11% for a twelve-month reference period.<sup>6</sup>

**TABLE A**  
ESTIMATES OF THE PERCENT OF ERROR IN SURVEY AND  
POLICE VICTIMIZATION DATA

TYPE OF ERROR	Portland FRC		London RRC		San Jose RRC		Washington RRC		Baltimore RRC	NCS Experiment
	6 mo.	12 mo.	6 mo.	12 mo.	6 mo.	12 mo.	6 mo.	12 mo.	6 mo.	6 mo.
<b>SURVEY</b>										
<b>A. Non-victim identified as victim</b>										
1. External forward telescoping	18%	11%	(>4%)	13%	-	-	(>4%)	-	-	24%
2. Exaggeration or lying		(32%) ( 3 )								
<b>B. Victim identifies as non-victim</b>										
3. Forgetting, understatement, lying to interviewer	-	-	4%	8%	32%	33%	18%	30%	33%	
4. External backward telescoping	6%	3%	3%	5%						
<b>POLICE</b>										
<b>A. Non-victim identified as victim</b>										
5. Exaggeration or lying	-	-								
<b>B. Victim identified as non-victim</b>										
6. Non-reporting		51%								60-70%
6a. Victim claimed to have reported, but did not		(32%) ( 3 )								
7. Non-recording		(32%) ( 3 )								

#### NON-RECALL

The reverse records checks show that the proportion who fail to recall a known crime to the interviewer has ranged from 4% in Sparks'

<sup>6</sup> The 12-month external forward telescoping estimates for the London and Washington studies are not comparable to the other estimates because, in each case, the external forward telescoping was estimated by drawing a sample of known victims within 13 to 15 months prior to the interview date. The 4% estimate is the proportion of the 15-month sample base which were pulled into the 12-month part of the time period. Forward telescoping, however, can be more extreme than this example. If the sample had included incidents 16 to 20 months in the past, some of these incidents also would have been pulled in. Thus, the London and Washington information of 12-month external forward telescoping is an underestimate if the researcher is interested in determining the proportion of incidents actually recalled in a time period that do not belong in that time period.



London study<sup>7</sup> for the six-month time period to 33% in the Baltimore study.<sup>8</sup> Sparks reports that only 8% of his respondents failed to recall the incident during the twelve-month reference period.<sup>9</sup> This remarkably better recall rate, in comparison with American efforts, probably is due to the improved questioning procedures used in the London study,<sup>10</sup> and the extensive efforts to assist respondents in remembering key dates during the previous year.<sup>11</sup>

#### EXTERNAL BACKWARD TELESCOPING

A third source of error in victim survey estimates is produced by external backward telescoping, in which the respondent places the incident earlier, out of the reference period. The procedures currently followed in almost all victimization survey work indicate that these incidents would not be counted, and in some surveys, would not even be entered with the computerized data. Although these persons are victims, the usual assumption is that the investigator wishes to identify the persons who have been victims within a particular period.

The Portland Forward Records Check<sup>12</sup> and the London Reverse Records Check<sup>13</sup> both showed that 3% of the incidents which actually occurred during the twelve-month reference period were telescoped backward out of it. The Portland estimate for external backward telescoping in a six-month reference period was 6%,<sup>14</sup> and the San Jose data show 5% external backward telescoping for a six-month reference period.<sup>15</sup>

#### NOT REPORTING TO THE POLICE

Information in the lower portion of Table A shows that the major source of error in police data involves an undercounting of victims attributable to victims not reporting incidents. The extent of not reporting is 60-70% according to the National Crime Survey.<sup>16</sup>

#### THE SURVEY-POLICE GAP

Estimates are given in Table A for three other sources of error, all

---

<sup>7</sup> R. SPARKS, H. GENN & D. DODD, *supra* note 1.

<sup>8</sup> *See* notes 4-5 *supra*.

<sup>9</sup> Sparks, Genn & Dodd, *Crimes and Victims in London*, in *SAMPLE SURVEYS OF THE VICTIMS OF CRIME 43-72* (W. Skogan ed. 1976).

<sup>10</sup> *Id.* at 1-15.

<sup>11</sup> *Id.* at 16-34.

<sup>12</sup> A. Schneider, *supra* note 4.

<sup>13</sup> Sparks, Genn & Dodd, *supra* note 9.

<sup>14</sup> A. Schneider, *supra* note 4.

<sup>15</sup> A. Turner, *supra* note 1, at 6-11.

<sup>16</sup> Lehren & Reiss, *supra* note 5.

of which are related to the common gap between survey and police estimates of crime.<sup>17</sup> The major contributor to the difference between survey and police estimates is lack of reporting, but even when only the incidents that survey respondents said were reported to the police are examined, the survey data often show a higher victimization rate than police records.

In the Portland Forward Records Check, 212 out of the original 972 incidents (22%) were found in the police records. Of the 760 which could not be found, 65% were not found due to the respondents' failure to report the incident. Of those which the respondent said were reported to the police, and for which a search was undertaken, 53% were located. An estimated additional 15% had not been located due to methodological problems or the importance of protecting the victim's identity.

Thus, approximately 68% of the victims identified in the survey were accounted for, leaving 32% who apparently were miscategorized either by the survey as victims or by the police data which said they were not. Three sources of error could account for the estimated 32% missing: (1) respondent's exaggeration of a situation as a crime when legally it would not qualify, or outright fabrication of incidents; (2) respondent's telling the interviewer that the incident was reported when, in fact, it was not; (3) the police not recording the incident because it did not have the elements of an offense or for other reasons.

#### ESTIMATING THE ERROR

Unfortunately, the data shown in Table A cannot be used to develop estimates of the amount of error to be expected in survey-generated identification of victims and non-victims. However, hypothetical populations with known distributions of victims and non-victims can be constructed. By applying various combinations of the error estimates to this distribution, and by choosing among the various assumptions, esti-

---

<sup>17</sup> The gap in victimization and official records is not found in all cities nor for all crimes. In Portland, Oregon, for example, the forward records check found one in five of the offenses and accounted for about two-thirds of all the incidents that respondents claimed to have reported. A. Schneider, *supra* note 4. Sparks estimates that only one in 14 of the incidents uncovered in the London survey made it into police records. R. SPARKS, H. GENN & D. DODD, *supra* note 1. For other cities, the survey may contain far too many reported incidents, whereas in some cities there is actually a negative gap, probably produced by the serious problems in calculation of rates for the official data with denominators that do not reflect the same population as counted in the numerator. See Skogan, *Comparing Measures of Crime Police Statistics and Survey Estimates of Citizen Victimization in American Cities*, 1974 PROC. SOC. STATISTICS SECTION AM. STATISTICAL ASS'N 44-52; Skogan, *Measurement Problems in Official and Survey Crime Rates*, 3 J. CRIM. JUST. 17-32 (1975); W. Skogan, Key Issues in the Measurement of Crime (paper presented to the Victimology Section, Congress of the Int'l Soc'y of Criminology, Lisbon, Sept. 6, 1978), for discussion of these problems.

mates of the validity of the data can be generated. Corresponding estimates of the maximum strength of association to be expected when using the victimization variable can be generated.

For example, consider an unbounded survey with a twelve-month recall period, utilizing questioning procedures similar to San Jose and NCS, which identifies 30% of the sample as victims and 70% as nonvictims.<sup>18</sup> Disregarding all other sources of error for now, what proportion in each category have been misidentified due to the problems shown in Table A? Of the 30% identified as victims, 11% of the victims may have telescoped the incident forward into the recall period, and a similar proportion exaggerated or lied to the interviewer (see Table B). Thus, 22% of the 30% (7%) are incorrectly identified as victims. The estimate of actual victims could be obtained by assuming a 33% forgetting rate as shown in the San Jose study, and by assuming external backward telescoping for 3% of the victims as estimated in the Portland and London studies. The survey estimate of victims, minus those misidentified and shifted to the non-victim category, should be increased to account for those missed. The results of these calculations, shown in Table B, indicate that the survey underestimated victimization (30% versus 36%) but, overall, 80% of the respondents were placed in the correct categories (the main diagonal), and only 20% are in the incorrect categories (the off-diagonal). The index of inconsistency is .34, the correlation coefficient (which also is phi) is .55, and the maximum correlation coefficient to be expected when using this hypothetical variable would be .74, assuming that the variable contains no other error and that the variables with which it is correlated contain all true variance and no error variance.<sup>19</sup>

Table C contains similar types of estimates for a variety of other conditions and assumptions. The calculations in Table C are based on estimates of the major measurement errors in victimization surveys, but the accuracy of the estimates used to generate the figures in Table C are not known. Furthermore, other assumptions could change the esti-

---

<sup>18</sup> A bounded interview, as that term is used here, refers to an interview that is bounded by a prior interview in that it is conducted at the beginning of the reference period, such as the procedure used in the NCS.

<sup>19</sup> The index of inconsistency is used in the San Jose Study. A. Turner, *supra* note 1. Hindelang indicates that .20 or below is considered very good; .21 to .50 indicates some problems with the data; and above .50 is an indication of serious problems. M. HINDELANG, M. GOTTFREDSON & J. GAROFALO, *VICTIMS OF PERSONAL CRIME: AN EMPIRICAL FOUNDATION FOR A THEORY OF PERSONAL VICTIMIZATION* (1978). It is a measure of association similar to phi and r, but reversed in its direction, and is asymmetrical rather than symmetrical. Thus, it shows the degree of association between the "standard" and the measure to be validated. The index can vary between zero and +1 with higher scores indicating more inconsistency. All of these statistics, except the simple percentage agreement, are influenced by the marginal distribution and, unless the marginals are equal, the statistics cannot achieve their maximum.

mates. Thus, the coefficients in the table should be used only as rough indications of the amount of error variance in the data. Before summarizing the implications of these figures, the critical assumptions underlying the particular calculations in Table C should be reviewed.

**TABLE B**  
VALIDITY ESTIMATES FOR HYPOTHETICAL SURVEY DATA

	TRUE CATEGORIZATION		Totals
	Victim	Non-Victim	
<b>SURVEY RESULTS</b>			
Victim	(a)	(b)	(a+b)
	23	07	30
Non-Victim	(d)	(c)	(d+c)
	13	57	70
Totals	(a+d)	(b+c)	
	36	64	100

**ERROR ESTIMATES:**

$$\left. \begin{array}{l}
 \text{Cell b - External forward telescoping} = 11\% \text{ of } a+b \\
 \quad - \text{ Exaggeration or lying} = 11\% \text{ of } a+b \\
 \text{Cell d - Forgetting} = 33\% \text{ of } a+d \\
 \quad - \text{ External backward telescoping} = 3\% \text{ of } a+d
 \end{array} \right\} \begin{array}{l} \\ \\ \\ \\ \end{array} = e_b \\
 \left. \begin{array}{l} \\ \\ \\ \\ \end{array} \right\} = e_d$$

**TRUE SCORE ESTIMATES:**

$$\begin{aligned}
 \text{Cell b} &= (a+b)(e_b) = 30 \times .22 = .07 \\
 \text{Cell a} &= (a+b) - [(a+b)(e_b)] = .30 - [(.30)(.22)] = .23 \\
 \text{Cell a+d} &= a/(1.0-e_d) = 23/64 = .36^{20}
 \end{aligned}$$

**DEGREE OF FIT:**

$$\begin{aligned}
 \text{Percentage Agreement} &= a+c = 80\% \\
 \text{Correlation (r) (and phi)} &= ac-bd / \sqrt{(a+b)(a+d)(d+c)(b+c)} = \\
 &.55
 \end{aligned}$$

$$\begin{aligned}
 \text{Index of Inconsistency} &= N \frac{N-(a+c)}{N^2 - [(a+d)^2 + (b+c)^2]} = .34 \\
 r \text{ Max} &= .74
 \end{aligned}$$

<sup>20</sup> Cell a + d is found as follows: If 33% of the true victims fail to recall the incident and 3% place it outside the reference period, then the actual number of victims in the survey is 100% - 36% = 64%. Thus, the number of victims identified by the survey (cell a) is 64% of the total. The number of true victims is .23/.64 = .36.

1. The amount of non-recall in a twelve-month time period is the same as that in a six-month time period for survey procedures such as those used in the American pre-tests and the NCS (see Table B).<sup>21</sup>

2. The amount of non-recall could be reduced substantially if questioning procedures were improved, which probably accounts for Sparks' improved recall rate, but telescoping will not be altered by improved questioning.

3. Bounding of interviews with a prior interview completely eliminates external forward and backward telescoping.

4. The non-recall and telescoping error for reported and unreported offenses are the same. There are studies, reviewed below, which show that memory bias is accentuated for incidents that were not reported to the police, but the differences are not particularly great. No adjustment has been made in Table C.

5. For comparison purposes, the survey data showed a 30-70 split of victims and non-victims and the police data showed a 10-90 split.

Readers, of course, are free to make other assumptions, and by using the data in Table A would be able to generate other estimates of error in the survey or police data. With these caveats, the implications of the calculations shown in Table C include:

1. For unbounded surveys, a twelve-month recall period may be better than a six-month recall period, because telescoping is more strongly related to the length of the recall period than is forgetting, at least for recall periods of six to twelve months.<sup>22</sup>

2. Surveys using six-month recall periods that are not bounded by a prior interview, and do not use sophisticated methods of memory improvement may contain substantially more error than any of the other options. The maximum correlation coefficient obtainable for these surveys might be as low as .44.

3. Police data in a community that only reports 30% of its crimes, and in which there is a 10% rate of not recording, is less valid than most

---

<sup>21</sup> See A. Turner, *supra* note 1; notes 4-5 *supra*.

<sup>22</sup> The 12-month recall period would still be superior to the six-month in unbounded surveys even if the rate of forgetting dropped to 18%, which is the estimate obtained from the Washington study, and the 12-month data stayed the same as in example A of Table C. A six-month, unbounded survey, with external forward telescoping of 24%, exaggeration of 11%, forgetting of 18%, and external backward telescoping of 6% would show a percentage agreement of 83.5;  $\phi = .59$ ; and the index of inconsistency would be .43. In addition, of course, a 12-month recall period is more productive than a six-month survey in terms of the sample size of victims, especially the less common ones.

**TABLE C**  
**COMPARISON OF VALIDITY ESTIMATES UNDER DIFFERENT**  
**ASSUMPTIONS AND CONDITIONS<sup>23</sup>**

RELATIONSHIP TO "TRUE" DATA					
CONDITION	Percent Agreement	Index of Inconsistency	phi(r)	Maximum r Obtainable in Analysis	
<b>SURVEY DATA (12-MONTH)</b>					
A. EFT = 11% Exaggeration = 11% Non-Recall = 33% EBT = 3%	80%	.34	.55	.74	
B. Same, except Non-Recall = 8%	90%	.26	.76	.87	
<b>SURVEY DATA (6-MONTH)</b>					
C. EFT = 24% Exaggeration = 11% Non-Recall = 33% EBT = 6%	59%	.82	.20	.44	
D. Same, except Non-Recall = 4%	88%	.38	.71	.84	
<b>BOUNDED SURVEY (6-MONTH)</b>					
E. Exaggeration = 11% Non-Recall = 33%	84%	.33	.67	.82	
F. Exaggeration = 11% Non-Recall = 4%	96%	.10	.90	.95	
<b>POLICE DATA</b>					
A. Non-reporting = 70% Non-recording = 10%	60%	.80	.31	.55	
B. Non-reporting = 60% Non-recording = 10%	77%	.52	.47	.69	
C. Non-reporting = 50% Non-recording = 10%	85%	.40	.57	.76	

<sup>23</sup> The coefficients shown in the table are very rough indications of the estimated error in categorization of respondents as victims or nonvictims. EFT refers to external forward telescoping; EBT refers to external backward telescoping. For each situation described in the rows

of the survey data, but may be more valid than the six-month unbounded interview using no special recall devices to minimize non-recall.<sup>24</sup>

4. With the possible exception of surveys using six-month unbounded reference periods and no special memory aides, data produced by surveys using six or twelve-month recall periods appear to be within the range of acceptable validity. The correlation coefficients tend to be at .70 or more, the index of inconsistency is in the .30s, and the maximum obtainable correlation coefficient is .75 or better.

5. Bounded surveys using the procedures adopted by the NCS with a six-month reference period can be expected to have a high degree of validity.

#### AMOUNT OF ERROR BY OFFENSE

If the error in victimization data were random and uncorrelated with all other variables of interest to the researcher, then its primary impact is attenuation of the estimates of the strength of association between variables and in the tests of significance. In other words, conclusions are biased against findings.

The errors discussed thus far, however, are correlated with the type of offense under consideration, and for that reason, introduce several additional problems. Offenses that contain substantial amounts of error, such as assaults, will be more susceptible to unnecessarily conservative conclusions than will offenses which contain less error, such as burglary. Theories of victimization that seem to work for one crime may not work for another simply because of differences in the error between offenses.

---

of the table, the initial set of estimates uses the highest error figures from Table A and the last situation uses the lowest set of error figures from Table A. The estimates for surveys are based on a 30-70 distribution of victims and nonvictims. Police estimates are based on the assumption that 10% of the population has reported an offense.

<sup>24</sup> This article does not focus on the utility of official data for victimology research, but the interested reader might notice that if the sample of known victims, such as police records or program files, covers 60% of the actual victims, and if this sample is combined with a sample from the general population, with corresponding re-weighting if needed in later phases of the study, then the validity of the victim to nonvictim variable might approach that of a 12-month unbounded survey—provided that there were no expected differences between reporting and nonreporting victims as well as victims who participate and those who do not. Sixty percent coverage from official files is difficult to obtain, and for incidents in which 60% coverage is obtained, the 10-90 split used in Table C is too low. If so, then the validity estimates would change and worsen if the proportion who are victims increases. For rare offenses with high coverage in official data, the costs of general population surveys may not be worth the marginal improvement in accuracy, especially since error also is a function of sample size and is not taken into account in the tables. To illustrate, an offense with a true victimization rate of 2% and a reporting rate of 50% would have a percentage agreement of 98%; phi of .70; and an index of inconsistency of .51.

**TABLE D**  
**OFFENSE-SPECIFIC ERROR IN VICTIM IDENTIFICATION**

	Burglary	Larceny/ Theft	Robbery	Assault	Rape
1. External Forward Telescoping					
NCS (6 months)	17%	28%	47%	30%	
San Jose (6 months)	6%	17%	22%	16%	0
Portland (6 months)	9%	17%	-	-	-
Portland (12 months)	6%	21%	-	-	-
2. Non-Recall					
Washington (12 months)	12%	23%	9%	35%	-
Baltimore (6 months)	14%	25%	24%	64%	-
San Jose (6 months)	5%	22%	24%	49%	53%
San Jose (12 months)	10%	9%	24%	52%	33%
London (12 months)	4%	11%	-----11%-----		
3. External Backward Telescoping					
San Jose (6 months)	5%	4%	10%	6%	0
Portland (6 months)	4%	7%	-	-	-
Portland (12 months)	3%	6%	-	-	-
4. Nonreporting by victim to police (NCS)					
	52%	73%	47%	53%	47%

Table D contains estimates of the amount of external forward telescoping, non-recall, and external backward telescoping for burglary, larceny, robbery, assault, and rape. These errors all influence the accuracy of a survey's categorization of persons as victims of these offenses. In addition, the proportion of these offenses not reported to the police, based on NCS data, is shown in Table D.

The data show that survey identification of persons whose homes have been burglarized probably is more accurate than identification of any other offenses. The non-recall rate for burglaries is less than 15%. The amount of external forward telescoping is estimated to be less than 10% except for the NCS estimate. The amount of external backward telescoping is 5% or less. Assaults appear to suffer from the greatest amount of error. The non-recall rate is exceptionally high, and the rates of external forward and backward telescoping both are substantial. Data are far less accurate on incidents of rape, but the recall rate for rape appears as poor as that for assaults, although the telescoping may not be as severe. This finding could indicate that non-recall for the rape incidents is attributable not to actual lack of memory but to unwilling-



ness to report the incident to the interviewer. Larcenies and theft seem to have about the same amount of error as robberies. Both have more error than burglaries but less than assault and rape.

The proportion of incidents reported to the police, according to survey respondents, varies substantially among the different offenses, with larcenies and thefts being especially underreported. As indicated by the last row of Table D, police data should be expected to omit about half the incidents of burglary (52%), robbery and rape (47%), and assault (53%), but to omit 73% of the larcenies and thefts. Because larcenies are particularly subject to external forward telescoping combined with relatively good recall in the surveys, and because they are not likely to be reported to the police, survey data often suggest that a much greater proportion of all incidents are larcenies than would be shown in the police data.

The major implications of the information in Table D can be summarized as follows: First, because of different errors in the data, survey information will not show the same patterns of offenses as police data. In particular, police data will indicate that a smaller proportion of all incidents are larcenies, whereas survey data will show that a larger proportion are larcenies.

Second, the strength of relationships between burglary and other variables should be closer to the true magnitude of the relationship, although still underestimated. The strength of relationships between the other crimes and other variables of interest would be seriously underestimated.

#### OTHER CORRELATES OF MEMORY RECALL BIASES

If certain types of victims tend to telescope forward more than others, then survey data will overestimate the victimization rates of these persons. Likewise, if certain types of victims telescope incidents out of the reference period to a greater extent than others, then these persons would be underrepresented in the survey data.

In the Portland Forward Records Check, several characteristics of victims were examined in order to determine whether some are more inclined to telescope than others. As shown in Table E, the age, race, sex, and educational level of the victims were not correlated significantly with the extent of forward telescoping. Two general tendencies, however, did not reach statistical significance. More serious crimes tended to be telescoped forward less than trivial incidents. There is a slight indication that men telescope forward less than women.

The absolute amount of either forward or backward telescoping appears to have weak, but statistically significant, relationships with some

characteristics of victims and offenses (see Table F). More errors appear to be made by younger respondents than by older ones, and by women rather than men. The information suggests that errors are more likely to be made in reference to trivial incidents than to serious ones.

**TABLE E**  
CORRELATES OF FORWARD TELESCOPING BY CRIME TYPE  
FOR MATCHED CASES<sup>25</sup>  
(Pearson Correlations)

Characteristic	All Crimes (n=203)	Property Crimes (n=181)	Personal Crimes (n=16)
Time between incident and interview	.68**	.70**	.03
Positive attitude toward police	.00	.02	-.31
Age	-.06	-.06	.33
Race (0=black; 1=white)	-.08	.11	‡
Sex (0=female; 1=male)	-.10	-.13*	-.21
Education	-.01	.04	-.08
Seriousness	-.11	-.08	.03
*P < .05			
**P < .001			
‡ Only one black respondent			

Even though these relationships reach statistical significance, they are not very substantial. For instance, correlations of less than .15 explain less than 3% of the variance in telescoping. The period between the date of the incident and the interview correlates at .64 with the absolute amount of error. Of considerable interest is the fact that Sparks found only a .14 correlation between this period and the absolute amount of error in placement of the date.<sup>26</sup> The additional emphasis on accuracy of recall used in his questioning procedures might account for this substantial difference in results of the two studies. Moreover, Sparks did not find correlations between the absolute amount of error in recall of the date and age, race, sex, or other similar variables. This lack of correlation could be produced by differences in questioning proce-

<sup>25</sup> Positive correlations mean that higher scores on the characteristic are related to forward telescoping; negative correlations mean that lower scores on the characteristic are related to forward telescoping. For example, for all crimes longer time between the incident and the interview is strongly related to forward telescoping.

<sup>26</sup> Sparks, Genn & Dodd, *supra* note 9.

**TABLE F**  
**CORRELATES OF ERROR IN RECALL OF INCIDENT DATE**  
**(TELESCOPING) FOR MATCHED CASES<sup>27</sup>**  
**(Pearson Correlations)**

Characteristic	All Crimes (n=203)	Property Crimes (n=181)	Personal Crimes (n=16)
Time between incident and interview	.64**	.65**	-.02
Positive attitude toward police	.07	.08	.10
Age	-.12*	-.11	.22
Race (0=black; 1=white)	-.04	-.03	‡
Sex (0=female; 1=male)	-.14*	-.16*	-.30
Education	-.04	-.04	-.03
Seriousness	-.12*	-.08	-.02

\*P < .05  
\*\*P < .001  
‡ Only one black respondent

dures, if such procedures are most effective on persons who otherwise would be most likely to err. Thus, the improved surveying technique could not only reduce error, but might result in the error being more evenly distributed across different kinds of respondents.

Perhaps the most widely known error in the victimization surveys is the relationship between failure to recall incidents of assaultive violence and the relationship of the victim to the offender. The San Jose study showed that incidents in which the victim knew the offender were far less likely to be reported during the interview. The Portland Forward Records Check showed the same pattern of bias for official data. Interview victimizations which involved family members, persons who knew each other, or juveniles were not as likely to be found in the records or, if found, were more likely to have been classified as a reduced crime type, e.g., malicious mischief rather than assault.

Sparks' study is the only one of the reverse records checks that reports whether the tendency not to recall the incident to the interviewer was related to characteristics of the victim. His conclusion was that

<sup>27</sup> Positive correlations mean that higher scores on the characteristic are related to greater error in recalling the incident date; negative correlations mean that lower scores on the characteristic are related to greater error. For example, for all crimes lower seriousness is related to greater error in recalling the incident date.

non-recall was not related to sex, age, race, migration patterns, employment, attitudes, perceptions about crime seriousness, or social class of the victim. Another technique that has been used to correlate memory decay in the form of either telescoping or forgetting is to examine the pattern of recall during the months covered in the reference period. The usual procedure is to assume that if there were no memory decay, each month in the recall period would contain an equal share of the total incidents recalled in the study. In some studies, the official data have been used to correct for actual trend, but in most instances these corrections have not been needed.

Two studies have examined the relationship between victim characteristics of age, race, sex, and education, and memory decay.<sup>28</sup> Both concluded that there were no significant relationships. A National Crime Survey methodological study found two statistically significant relationships. Incidents with weapons were less subject to memory bias than incidents without weapons. Incidents in which the suspect was a stranger were less subject to memory biases. These findings indicate that less salient incidents show a sharper memory decay due either to more forward telescoping or to more forgetting in the distant months.

Several investigations have been undertaken to determine whether incidents that respondents said were not reported to the police are more likely to be forward telescoped or forgotten than are incidents which were reported.<sup>29</sup> Although the evidence is not substantial, the nonreported incidents are subject to more memory bias than are the reported incidents. Therefore, the unreported incidents either are telescoped forward more than the reported ones, or they are forgotten easier, or both. If forward telescoping is the primary problem, then in unbounded surveys estimates of the proportion of incidents not reported will be inflated. If non-recall is the primary problem, then survey estimates of incidents not reported will be too low.

A further implication of different error patterns for incidents reported and not reported is that error estimates which rely on police data as the standard cannot be used without adjustments to estimate the error in survey data.

---

<sup>28</sup> M. HINDELANG, CRIMINAL VICTIMIZATION IN EIGHT AMERICAN CITIES—A DESCRIPTION ANALYSIS OF COMMON THEFT AND ASSAULT ch. 2 (1976); A. Schneider & D. Sumi, Patterns of Forgetting and Telescoping in LEAA Survey Victimization Data (Inst. Pol'y Analysis, Nov. 1977).

<sup>29</sup> See Gottfredson & Hindelang, *Victims of Personal Crimes—A Methodological Disquisition*, PROC. SOC. STATISTICS SECTION AM. STATISTICAL ASS'N (1975); A. Schneider & D. Sumi, *supra* note 28; H. Woltman & G. Cadek, Are Memory Biases in the NCS Associated with the Characteristics of the Criminal Incident (Census Bureau Memo, April 4, 1977).

## DISCUSSION AND CONCLUSIONS

The implications of measurement error depend on both the amount and the nature of the error. The key consequence of random error is an attenuation of the strength of relationship and tests of significance which, in turn, produce no findings even when relationships exist. Probably the most important consequence of directional error, that is, error not correlated with other variables but which has a non-zero mean, is inaccuracy in descriptive studies of the phenomenon being measured. If the error is correlated with other variables used in the study, then the results may contain serious distortions, or even reversals, in the direction of the relationship among the variables.

Presuming that the estimates of error obtained from the reverse record checks, forward record check, and the NCS methodological studies are relatively accurate, the following conclusions are warranted:

1. Surveys using six-month reference periods, no bounding to eliminate telescoping, and no special memory aides other than the usual screening questions may contain considerable misidentification of respondents as victims during the reference period. The total amount of error in the six-month reference period appears to exceed that contained in the twelve-month surveys.

2. Sampling from files of known victims, such as police records of victim programs, and subsequent use of the information in conjunction with general population surveys to identify non-victims can be expected to produce data with accuracy approaching that of the surveys only if their coverage of all reported and unreported victims is considerably higher than indicated by the nonreporting rates shown in the NCS. The extent of coverage is open to speculation, but an unbounded survey using a twelve-month recall period should produce data that are as valid as police or program data which capture 40-50% of all the actual incidents. The choice of a data set is not the subject of this paper, and of course, should be guided by several additional considerations, such as size of the sample that can be generated, which is a major contributor to error, and the cost of the data.

3. The accuracy of survey data in categorizing respondents as victims varies by offense. Burglary victims are better-identified than any other type of personal, rather than commercial, victims. Victims of personal assault, especially if the offender is known to the victim, are identified with the least amount of accuracy. The implication here is clear that theories of assaultive violence may be more difficult to support from the data than are theories of property offenses such as burglaries. Although surveys seem not to be a particularly efficient way of recovering incidents of personal violence, especially between persons who are

known to one another, the same may be true for police data. The Portland Forward Records Check study indicated that these offenses, even though reported to the interviewer, were more likely not to be found in police files than were property offenses such as burglaries.

4. Evidence is accumulating that different amounts and patterns of errors may be found for reported crimes than for unreported ones. Of particular concern, then, is the fact that all estimates of non-recall, and most estimates of telescoping, are based on reverse records checks which began with reported crimes. In addition, no victimization studies using truly short reference periods such as one day or one week have been undertaken, hence the amount of non-recall found in the reverse records studies is seriously underestimated.