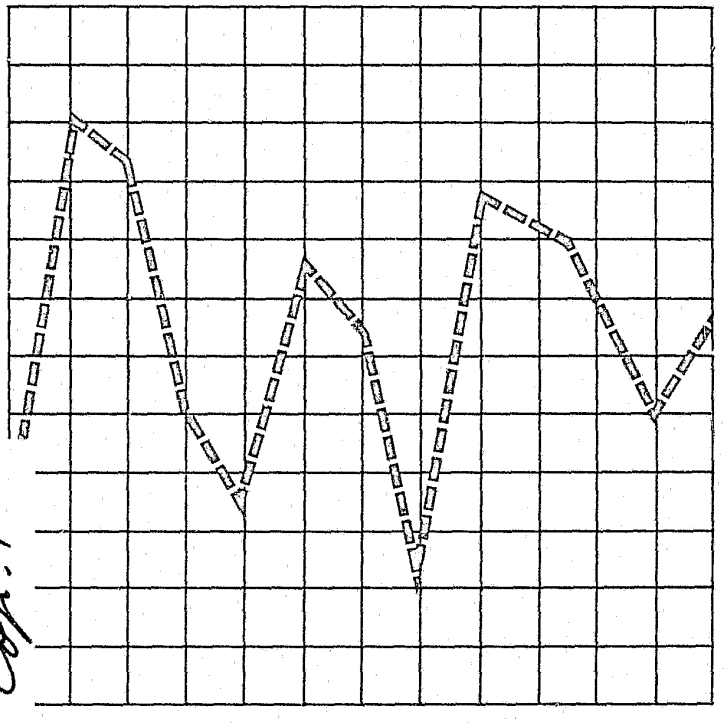
Portland Forward Records Check of Crime Victims



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Law Enforcement Assistance Administration
U. S. Department of Justice

Portland Forward Records Check of Crime Victims

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December 1978

National Institute of Law Enforcement and Criminal Justice
Law Enforcement Assistance Administration
U. S. Department of Justice

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402

Stock No. 027-000-00709-1

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This project was supported by Grant Number 76-NI-99-0084, awarded to the Oregon Research Institute by the National Institute of Law Enforcement and Criminal Justice, Law Enforcement Assistance Administration, U. S. Department of Justice, under the Omnibus Crime Control and Safe Streets Act of 1968, as amended. Points of view or opinions stated in this document are those of the authors and do not necessarily represent the official position or policies of the U. S. Department of Justice.

ABSTRACT

Victimization survey data and official crime records never reflect exactly the same information. This raises a major question: are victimization survey data sufficiently reliable and valid to be used for research and evaluation purposes when official data are not appropriate? Among the issues to be resolved: does the victimization data provide an accurate portrayal of the types of crimes that occur, of the seriousness of the crimes, the characteristics of the suspects, and the patterns of victimization?

This study is based on an intensive analysis of 212 reports of crime incidents from the 1974 Portland, Oregon, victimization survey. These were matched with official crime reports of the same incidents. The record check described and analyzed the following: (1) differences between survey and police data in classification of these crimes, details of the events, seriousness of the offenses, characteristics of the offenders, and activities of police, victims, and witnesses during the crime; (2) the frequency of "don't know" responses in the data; (3) patterns of telescoping (i.e., in recalling the crime, the victim distorts the time of occurrence) and their relation to victim characteristics; (4) survey incidents not found in police data even though respondents said they were reported; and (5) implications regarding the utility of survey and police data and the methods used to collect victimization survey information.

In making recommendations for future research, the study drew these conclusions: (1) Information obtained through victimization surveys is sufficiently similar to that reported to police so that most crimes are classified the same way by the two sources of data; (2) the survey data contained higher estimates of the dollar loss from the crime; (3) the reliability or validity of the survey data depend upon the type of information considered; (4) for most of the types of information considered, accuracy or completeness did not decline as a function of the time lag between occurrence and interview; (5) the age of the victim was not related to the amount or type of error in the data.

Appendices provide a review of the difficult match/no-match decisions (i.e., matching survey to official reports); the seriousness scale used in the analysis; and tables comparing different categories of crimes reported to police and reported in the interview. A bibliography also is included.

ACKNOWLEDGEMENTS

We would like to thank several people who were of considerable help in the conduct of this research: Richard Rau, the LEAA project director; four reviewers whose comments were invaluable in preparation of the final report--Wesley Skogan, Leonard Bickman, Lois Mock, and Steve Schroffel; and Lois Newton and Karen Oatis for their typing and help in preparing the final report.

TABLE OF CONTENTS

PORTLAND FORWARD RECORDS CHECK OF CRIME VICTIMS:

EXECUTIVE SUMMARY

The past ten years of experience with victimization surveying have been marked by numerous debates concerning the merits of measuring crime through surveys. The debate often has been cast in terms of whether the survey-generated crime data are "better" or "worse" than official crime statistics. This approach to the issue fails to recognize that there are major differences between the two sets of data and that each has an important role in planning, evaluation, and research.

It is an oversimplification to believe that survey and police data are simply two measures of the same phenomenon and, therefore, only one or the other should be collected. Survey data contain reports of crime incidents that are not in police files. Most of the incidents that are not in the police data were never reported to authorities and others were reported but were not recorded as an official crime incident. For some types of planning, evaluation, and research purposes it is imperative that the data include unreported as well as reported crimes. Thus, the critical question is not whether survey-generated victimization data are needed, but rather whether survey data are sufficiently reliable and valid that they can be used in the types of studies for which the official data are inappropriate.

The Portland Forward Records Check of Crime Victims was designed to investigate a number of issues concerning the reliability and validity of survey-generated reports of crime incidents. The study is based on an intensive analysis of 212 survey-generated reports of crime incidents from the 1974 Portland, Oregon victimization survey that were matched with the official crime report of the same incident. The study cannot provide definitive answers to all questions because of the small sample size and the fact that all the data are from one city. Nevertheless, the Portland study is the first forward records check of crime victims and

one of only a few studies which have compared survey information with official data about the same crime event. The major conclusions and recommendations from the study are summarized below.

Major Conclusions

1. The information obtained through surveying is sufficiently similar to that given to the police at the time of the incident that most crimes are classified in the same way by the two sources of data.

Both the Portland study and the San Jose records check found that 97 percent of the burglaries were classified the same from survey and police data; both found that 82 percent of the larcenies were classified the same way; and there were only slight differences concerning classification of personal crimes. The Portland study indicated that information was sufficient to produce the same classification in 74 percent of the personal crime incidents, whereas the San Jose study (which had a larger sample of personal crimes) obtained the same classification in 85 percent of the incidents. The implication is that even though survey data might be criticized for a variety of reasons, there is accumulating evidence that criticisms directed toward the accuracy of information needed to classify crimes are not warranted.

2. Survey data from Portland and from the San Jose study contained higher estimates of the dollar loss from the crime.

The range of differences found in the San Jose data was from a 24 percent higher estimate to a 33 percent higher estimate, depending on the type of crime. The range of differences found in the Portland study was from a 24 percent higher estimate to a 48 percent higher estimate. Several propositions were tested with the Portland data concerning factors that might have produced higher estimates in the survey, but we were not able to identify reasons for the higher estimates in survey data compared with police estimates.

This problem with the data is most acute for researchers who wish to use survey information to estimate the total amount of monetary loss due to crime or the average loss per victim. Data of this type are of value in estimating the expected cost of crime compensation programs, the savings that could result from certain types of crime prevention programs, and cost-effectiveness evaluations. The survey data include estimates for unreported as well as reported crime, and, for this reason, might be considered superior to official data even if the error is contained mainly in the survey information. At this time, however, there is no evidence of whether the error was in the survey data or the police data (or both).

3. Telescoping crimes into the reference period that actually occurred prior to the most distant month included in the time span appears to be a major problem in unbounded interviews.

The Portland data showed that larcenies were more likely to be telescoped than other types of incidents. An average larceny was telescoped forward by 4.4 months in the Portland study and 22 percent of all the larcenies were incorrectly placed within the recall period when they actually occurred prior to it. These results are similar to previous studies in that incidents tend to be forward telescoped to a greater extent than they are backward telescoped.

The study confirms previous research which has shown that telescoping produces error in the surveys in relation to the victimization rate, the comparative frequency of different types of crimes, and the month-by-month trend within the recall period.

The analysis of why telescoping occurs showed that the major explanatory factor is the amount of time that elapsed between the incident and the interview. Incidents that occurred further in the past were telescoped to a greater extent than those which occurred recently. There was no indication from the analysis that certain types of victims were more inclined to telescope incidents than

were other types. The only characteristic of the crimes that was examined in relation to telescoping was the seriousness scale. Although the more serious incidents were telescoped less in the 212 cases examined, the strength of the relationship ($r=.11$) was not great enough to be statistically significant at the .05 level.

4. The reliability or validity of survey data depend upon the type of information being considered. The types of information that appear to be most accurate and to have the greatest validity are:

1. The details of what happened during the crime, including whether the victim was attacked, whether the victim was threatened, whether the offender had a weapon, whether there was physical injury, whether medical attention was needed, whether property was taken or damaged, whether the offender had a right to be there, whether the offender actually got in, and whether there was evidence of forcible entry.
2. The classification of the offense.
3. Age and sex of suspects.
4. Number of suspects.
5. Whether the victim undertook self-protective actions.
6. Whether there were witnesses present.

Differences between police and survey information were great enough to be of concern for the following:

1. Seriousness of the offense (Sellin-Wolfgang scale).
2. Dollar loss from the crime.
3. Race of suspects.
4. Whether the suspect was known to the victim or not.
5. Police response time.
6. Number of activities undertaken by the police at the scene.
7. Month during which the crime occurred.

The seriousness of the offense was measured with the Sellin-Wolfgang scale and included several indicators of seriousness. The higher estimates in the

survey data were produced mainly by higher dollar loss from the crime and by victim statements concerning whether a weapon was present or not. Efforts were made to determine why the survey data contained higher estimates, but there was no evidence that memory loss or memory distortion produced the differences and no evidence that certain types of victims contributed disproportionately to the higher survey estimates.

It is not possible to develop recommendations concerning how the accuracy of dollar loss and seriousness data could be improved, since we do not have any evidence about the source of the problem. Police record-keeping could produce lower estimates; survey methods in which the value attached to the items stolen is accepted without questioning could produce higher estimates in the survey; or, respondent errors could produce the differences. It is important, however, that some additional investigation be undertaken to identify the reasons for the differences and to develop better questioning procedures (perhaps for both the interviewers and the police) in order to insure that measures of crime seriousness and dollar loss are more accurate than indicated in the Portland data.

Police and survey data differed on a case-by-case basis concerning the race of the suspected offender and whether the offender was known to the victim. However, there were no systematic differences in that the survey data did not indicate more black (or white) suspects than the police data and did not suggest that there were more (or fewer) strangers than the police data. It might be noted that police and survey data were more similar in respect to age, sex, and number of suspects than they were for race or relationship of offender to victim. This result, if replicated in other studies, would suggest that the latter facts about the incident are more sensitive to the respondent. Improvement in the reliability of the data might be achieved through better questioning procedures (by interviewers and/or by the police). As with most of the other data which differed between police and survey records, no evidence could be found concerning

why the differences exist. Memory loss, memory distortion, and selective misperception by certain types of victims were tested as possible explanations, but none of these had statistically significant correlations with the amount or type or error.

The implications are that the reliability of racial data about offenders may be lower than some of the other information, making it more difficult to find statistically significant relationships between offender's race and other characteristics of the incident or characteristics of the victim. The same is true for the stranger/non-stranger variable. On the other hand, studies which use these variables to examine relationships between type of victim and type of offender, for example, should not contain systematic biases that could confound the conclusions because error in offender characteristic data appears to be unrelated to victim characteristics and unrelated to characteristics of the offense.

Survey data overestimated police response time, in comparison with police records, and underestimated the number of activities undertaken by the police at the scene. The most plausible explanation for survey estimates of police response time being higher than the police estimates is that persons, during times of crisis, tend to believe that more time has elapsed than actually is the case. The possibility that police underestimate the time cannot be entirely eliminated, but in Portland this possibility is very remote. The victim's call to the police is recorded, the dispatcher's call to the officer is recorded, and the officer's call that he has arrived on the scene is recorded. The time estimates are kept in seconds, not just in minutes, and even though the persons who copy from these logs onto the police form could alter the response time data, it does not seem likely that they would do so, since positive evidence of response time is available.

A plausible explanation for why the survey data underestimated the activities by the police at the scene is that this is an open-ended survey question and not one designed to jog the memory of respondents in the survey. Questions which specifically ask the victim to recall whether the police investigated, arrested someone, or took fingerprints, almost certainly would improve the survey data.

5. For most of the types of information elements examined in this study, there is no evidence that the accuracy or completeness of the information declines as a function of the time lag between when the crime occurred and when the interview was conducted.

There were, however, two exceptions. First, the accuracy of respondents' recall of the date declined as the time lag increased, and, second, there was a tendency for victims to forget that witnesses were present for events that occurred further in the past.

The implication of this finding is that a 12-month retrospective recall period may be just as good as shorter ones if the data are to be used for certain types of purposes. Previous studies have demonstrated without exception that respondents are more apt to forget crimes that occurred further in the past. The evidence in this study suggests that if they remember the incident at all, they tend to remember (accurately) most of the details about what happened. Thus, studies which use victimization surveying for the purpose of analyzing relationships within the data, rather than making population-level estimates of victimization rates, might be able to use longer recall periods--perhaps recall periods even longer than twelve months. The critical question, and one that has not been examined, is whether incidents that are forgotten differ from those recalled in terms of the patterns and relationships between victims and offenders, offenders and certain characteristics of the crime, and so on. Therefore, before definitive conclusions are drawn concerning the optimal recall period for surveys

focussing on patterns and relationships, the results in this research should be replicated and similar types of analyses should be conducted using reverse record check procedures so that forgotten incidents can be analyzed.

6. Preliminary evidence from the study indicates that survey data should provide accurate conclusions for studies of:

(a) the distribution of crimes among population subgroups;

(b) the distribution of crime seriousness among population subgroups;

(c) the relationships between victims' characteristics and certain characteristics of the offense; and

(d) the relationship between victim characteristics (age, race, educational level, sex) and the activities of the victim, police, and witnesses at the time the crime occurred.

Characteristics of victims were not related to the amount of error in the data nor to systematic misperceptions about the events. Furthermore, there was no evidence that certain types of victims forward telescope more than others. Forward telescoping results in an overestimation (in unbounded surveys) of the amount of crime committed against persons who forward telescope. Thus, the fact that victim characteristics were not related to forward telescoping is an important result from the study.

It should be emphasized, however, that if offenses which are forgotten are characterized by different patterns and relationships than those recalled, then the survey data would not produce reliable conclusions about such relationships. Thus, the results of the forward records check need to be replicated and reverse record checks should be designed to test bias in the forgotten incidents.

Although the survey data appear to be relatively free of systematic misperceptions by certain types of victims, there is a tentative indication that persons with negative attitudes toward the police projected these attitudes into their recollection about what the police did, how long it took the police to arrive, whether there were witnesses present, and the extent of the victim's own activities to

prevent the crime. Thus, studies that seek to explain victim attitudes toward the police as a function of police activities or response time should be cautious in interpreting the causal direction of observed correlations. The data presented here indicate that persons with negative attitudes may perceive these in a different way than persons with positive attitudes, even though the "facts" are the same.

7. Evidence from this study and others indicates that victimization survey data cannot be used to measure trends in the victimization rate within the retrospective recall period covered by the survey.

If telescoping and forgetting were distributed equally (or randomly) across the various months in the recall period, then one could use the data from a single survey to estimate monthly or quarterly victimization rates (provided, of course, that the size of the sample was sufficiently large). There is a considerable body of evidence, however, which demonstrates that telescoping is primarily forward rather than backward, and that forgetting increases with the length of the recall period. Even though the survey data contain information about the date of each crime event, a single survey yields an estimate only for the entire 12-month recall period (or six months), and not for individual months.

This problem greatly reduces the value of survey data for evaluation purposes. Survey data are needed for most crime prevention and deterrence programs as well as for other evaluations which require comparisons across cities, and programs that would alter citizen reporting rates or police discovery rates. Because these types of programs are focussed on entire geographic areas, it is usually impossible to have a true field experimental design, and the best procedure available to the evaluator is the quasi-experimental time-series design that requires twelve to fifteen pre-program estimates of monthly (or quarterly, or yearly) victimization rates and several post-program estimates. If the survey data could be disaggregated, then each survey using a 12-month recall

period would provide twelve estimates; two surveys would yield 24 estimates; and so on.

8. The analysis indicated that the age of the victim was not related to the amount or type of error in the data. Moreover, the study showed that persons who make errors in recalling the correct date are no more likely than others to have given different information to the interviewer than to the police. Both of these results were somewhat surprising, since age is generally presumed to influence memory loss, and since it is reasonable to believe that persons who make one type of error would be more inclined to make others. A partial explanation was revealed in the analysis of "don't know" responses. The frequency of these increased with respondent age and with the frequency of error in recalling the date of the incident. Thus, it is possible that older victims and those who guess (incorrectly) at the date of the incident tend to say "don't know" to other questions rather than provide erroneous information.

9. Many of the incidents that respondents said were reported to the police were not found in police files. Through a series of adjustments in the data, the best estimate is that approximately 32 percent of the survey incidents that presumably were reported could not be found either because they were not reported or because they were not recorded as a crime by the police.

Discussion and Recommendations

Victimization surveying has the potential for providing considerable information and new knowledge about crime which cannot be obtained from official crime statistics. Unreported crimes constitute a large proportion of all incidents that occur. The absence of unreported incidents in official data represents an inherent and uncorrectable problem with using the official statistics for a variety of research and evaluation purposes. Survey data should provide superior estimates of the amount, costs, and characteristics of criminal

victimization. Analysis of the data could, potentially, provide important new insights about crime causation, factors contributing to victimization, and the distribution of crime as well as its costs among different population subgroups.

The results of this study indicate that survey data are sufficiently reliable and valid to be used with confidence for some of these purposes, but doubts remain about others. Furthermore, the results of a single study, conducted in a single city, with a small sample, are not final answers to these questions, and all of the propositions tested in the Portland study need to be reexamined and replicated in other studies before final conclusions are drawn. Although the survey data appear to be quite good in many respects, the full potential of victimization surveying for generating information of the type mentioned above will not be realized unless these is a resumption of methodological research into the types of bias in survey-generated information about crime and the efficiency of various solutions for improving its reliability and validity.

A major recommendation from this study is:

A series of multi-purpose reverse record checks should be conducted in several different cities. The studies should be designed so that information can be obtained in relation to several propositions and the results compared across the different cities.

(a) The amount of telescoping, forgetting, and differences between police and survey data in factual information about the incident.

(b) The characteristic nature of the differences (higher or lower survey estimates in comparison with police data, for example).

(c) The extent to which telescoping, forgetting, and differences between police and survey data are correlated with characteristics of the victim, the offense, and the offender.

Ideally, the samples drawn for the studies should be large enough to permit at least a minimum amount of experimenting with different surveying methods, different questioning procedures, and/or different recall periods. The purpose

of these studies would be to test propositions such as those examined in this study about the types of bias in the survey data and to experiment with methods of reducing them.

One of the most important contributions that could be made by victimization surveying is in the improvement of program evaluation efforts. The survey data are needed for evaluating community-based crime prevention programs, crime deterrence programs, programs that alter citizen reporting rates and/or police discovery of crimes in progress, and programs or strategies that are being tested comparatively across different cities. This potential will not be realized unless there are several substantial changes made.

It should be emphasized that true experimental designs are not in common use for field evaluation and are impossible for many types of community-based prevention or deterrence programs. Thus, the best evaluation design that can be used is a quasi-experimental time-series approach which requires numerous time points prior to and after the program implementation.

Victimization survey data at the national level would be suitable for such evaluations if the surveys were conducted with sufficient frequency, prior to the implementation of a program, so that twelve to fifteen monthly or quarterly estimates of victimization rates would be available, and a continuing series of monthly or quarterly estimates could be made after the program is implemented. Even though these methods would be appropriate in terms of data reliability and validity, the national data cannot be used for program evaluation, because there are no national programs that use common strategies and which are implemented simultaneously throughout the country. Since the method used for national data collection requires personal interviews every six months of a panel of respondents, there are few (if any) cities or states that could afford to conduct these kinds of surveys on a continuing basis. Although the federal government may be

willing to fund victimization surveys in several areas for the purpose of evaluating innovative programs, the areas cannot be identified far enough in advance of program implementation to provide the twelve to fifteen pre-program surveys that are needed to generate twelve to fifteen monthly or quarterly estimates of victimization rates. Thus, even when victimization surveys are fielded in conjunction with new programs, the results (at best) are a "before and after" evaluation design, which is one of the weakest possible types. It is almost impossible to draw definitive conclusions about the effectiveness of a program in terms of crime reduction when a "before and after" design has been utilized.

Another major recommendation from this study is:

One or more studies should be initiated to test different types of surveying procedures that are (a) inexpensive enough to be widely implemented in cities and states, and (b) designed so that a single survey can generate several time-specific victimization estimates. The types of methods that should be tested include mailed and telephone interviewing using rolling monthly sampling procedures. The types of biases that need to be examined include those named under the first recommendation (telescoping, forgetting, and informational differences). In addition, the studies should seek solutions to the complex methodological questions concerning how the data produced from rolling monthly survey procedures should be adjusted to provide the most accurate month-by-month estimates. (This type of procedure is examined more fully in another report produced from this grant; see Schneider, 1977.)

A major assumption underlying much of the discussion in this report is that there are certain types of research and evaluation questions for which official data are inherently inappropriate because they do not contain unreported incidents. Although most researchers believe this to be an insurmountable problem for certain types of research, there is very little empirical information

concerning the situations or conditions which, if they exist, make it reasonable to assume that official data are a representative and unbiased subset of all crime incidents. It is not known, for example, whether the patterns and relationships found in survey data (reported and unreported) differ from the patterns in official data. At the heart of the issue is the question of how the reported and recorded incidents differ from those that were either not reported, or, if reported, were not recorded.

The third major recommendation is:

One or more studies should be undertaken to examine the differences between reported and unreported incidents. The differences in terms of general descriptions of the types of crimes, types of offenders, and so on should be included, but the major focus of the study should be to determine whether there are differences in the patterns and relationships within each set of data which would confound or invalidate the conclusions drawn by studies that used only one of the data sets.

PART I

INTRODUCTION

The past ten years of experience with victimization surveying have been marked by numerous debates concerning the merits of measuring crime through surveys. The debate often has been cast in terms of whether the survey-generated crime data are "better" or "worse" than official crime statistics. This type of global approach to the issue fails to recognize that there are major differences between the two sets of data in terms of what is being measured, the conceptual meaning of the measurements, the appropriate uses of the data, and the types of biases that exist in each data set.

It is a gross oversimplification to assume that survey and police data on crime are simply two measures of the same phenomenon and, therefore, only one or the other should be collected. There is an overlap in the data, since each provides an estimate of crimes that became known to law enforcement officials, but survey data contain reports of many crime incidents that are not in police files. There were almost 800 different crime incidents revealed in the Portland victimization survey of which only 22 percent were found in police records. Most of those that are not in police files were never reported to the authorities. The converse, however, also is true: Reverse record checks conducted in other cities show that the survey methods do not uncover all the incidents that are in police files.[1] Some victims actually forget that the incidents occurred, and others apparently do not wish to tell the interviewer about the incident. Claims that survey data are not an accurate estimate of "total" crime miss the major point. Neither the official data nor the survey data can ever be an entirely accurate representation of total crime. Official data will never include incidents not reported to law enforcement, and survey

data will never include incidents that respondents do not wish to reveal to interviewers. Nevertheless, most would agree that surveys, if properly conducted, have a major advantage in providing an estimate of total crime because victims clearly are willing to tell interviewers about numerous crimes that were not reported to the police.[2] This finding from the surveys was greeted with considerable concern by law enforcement officials. Statements were made that the nonreporting represented a lack of trust and confidence in law enforcement. Subsequent research has indicated that the major reason for not reporting crimes is the comparative triviality of the events, and only a small proportion of the nonreporting can be attributed to a lack of trust in law enforcement.[3]

Although it is interesting to examine the reasons for nonreporting of crimes, it should be noted that survey and police data have very different conceptual meanings regardless of the reasons for nonreporting.

Generally, it is appropriate to say that crimes which become known to the police represent public demand for law enforcement services and, conceptually, are an input to the criminal justice system. These data are essential to law enforcement and other criminal justice agencies. It is from the reported crime data that officials are able to develop crucial information for their day to day operations. In addition, reported crime data can be used for a variety of research and evaluation purposes, including studies on 1) the optimal allocation of law enforcement resources to different areas of a city, 2) the need for future criminal justice system resources, and 3) the effectiveness of certain types of apprehension or conviction programs.

Reported crime rates, despite their value, often are not an adequate outcome measure of criminal justice system performance vis-a-vis questions such as the amount of crime in the community, the probability of victimization,

and the social and monetary costs of crime. Although the primary responsibility of law enforcement may be to apprehend offenders when called upon to do so by the citizens who report crimes, the criminal justice system as a whole also seeks to reduce the total amount of crime, the probability of victimization, the monetary costs of crime and its social costs. Thus, an appropriate outcome indicator of criminal justice system performance is whether reported and unreported crime is being reduced. If the programs that are supposed to bring about a reduction in total crime are likely to increase citizen reporting of crime to the officials, then it is especially important to have measures of both reported and unreported crime.

Measures of reported and unreported crime also are needed for most research concerning social or economic correlates of crime, victim-offender relationships, distribution of crime and its costs among various social groups, and factors affecting the probability of victimization. If the researcher is willing to assume that reported events, as recorded by the police are a representative subset of all crimes, the official data could be used for these purposes. However, most researchers do not believe the assumption is correct, even though there is very little direct evidence concerning how the reported and unreported crimes differ, and how (or whether) these differences would bias conclusions based only on the official data.

The problems with official data mentioned above are inherent and exist even if the very best procedures are used by law enforcement officials in collecting, counting, and classifying crimes. There are other problems frequently mentioned by researchers and evaluators. These include poor collection methods, unreliable counting and classification procedures, policy biases, political pressures, lack of comparability across different cities, and lack of comparability across different time points.[4]

Recognition of these problems resulted in major efforts during the 1960s

to develop methods of measuring reported and unreported crimes through sample surveys of the population. After a number of pioneering methodological studies, the Law Enforcement Assistance Administration and the Census Bureau implemented a series of victimization surveys in the larger cities of the nation, and in 1972, began the National Crime panel which is a nationwide rolling interview of randomly selected households.

The first methodological studies used a reverse record check procedure in which the names of known victims were drawn from police records, and the victims were then interviewed, using a survey instrument designed to jog their memories and to elicit details of the crime incident. The major purposes of the early studies were to establish the most efficient length of the recall period, to identify the most effective types of memory-jogging questions, and to establish methods for minimizing bias in the survey data. Analysis of the data focussed almost exclusively on measuring the extent to which victims "forget" incidents that they had reported to the police (forget to recall them for the interviewer or, for other reasons, fail to tell the interviewer about time), and the extent to which victims telescope incidents. Telescoping refers to respondent error concerning the date when the crime actually occurred. The type of telescoping of major concern in the studies involved a respondent placing an incident in the recall period when, in fact, it occurred prior to the earliest month in the desired time span.

It is important to study the problems of forgetting and telescoping because one of the major purposes of surveying is to provide a more accurate measure of crime, and because forgetting and telescoping both influence the accuracy of survey-generated estimates. Incidents that are telescoped into the time period (when they actually occurred prior to it) inflate the estimated rate; incidents that are forgotten despress the rate.

Unless these two exactly offset one another, the survey estimate of the amount of crime that occurred is inaccurate. Victimization data, however, increasingly are being used for purposes other than a precise, population-level estimate of the amount of reported and unreported crime. Changes in the victimization rate are being used for evaluation purposes. Victimization rates from different geographical areas within a city are being compared and comparisons are being made of the survey-generated estimates of crime across different cities. Researchers are using both national and city victimization data to analyze patterns of victimization, seriousness of crimes, relationships between victims and offenders, and the distribution of crime among subgroups in the population. In addition to the problems of forgetting and telescoping, there are many other potential biases in survey-generated data that could confound these types of research and evaluation studies. Thus, as use of survey data for purposes other than population-level estimates of the crime rate becomes more common, it is necessary to resume the methodological research in order to address several questions that were not considered in the early studies, and to investigate some of the potential biases and error in the data. The issue of major concern is whether victimization survey data are sufficiently reliable and valid that they can be used for research and evaluation when official data clearly are not appropriate.

Concern has been expressed by some researchers that the quality of the information obtained in surveys is poor, and that the classification procedures used in victimization surveying result in biased data.[5] Most analyses indicate that survey-generated estimates of the amount of crime are considerably above police estimates, even when the incidents that respondents say were not reported to the police are excluded. The magnitude varies from one study to another, but some analyses indicate that survey estimates of

reported crimes are twice as great as police estimates.[6] This phenomenon could be produced by respondents saying an event was reported when, in fact, it was not. Or, it could be due to police practices of not recording certain types of events or of down-classifying them. Still another possibility is that survey crime classifiers are responding to different information than were the police and are systematically over-classifying the crimes. This results in a greater number of incidents in the more serious categories than were known to the police. James Levin, in a highly speculative condemnation of victimization surveying, argues the following:

> Because coders must make decisions solely on the basis of unclear, incomplete accounts of respondents as filtered secondhand by interviewers, they inevitably play a role in determining the amount and kinds of crime ultimately extracted from the interviews...Since there are many marginal cases of criminality that are reported (in the interviews) and few precise coding guidelines, many 'crimes' that emanate from the surveys may be artifacts of the coding process...[7]

Another question of major interest is whether victimization data provide an accurate portrayal of the types of crimes that occur, the seriousness of the crimes, characteristics of suspects, and patterns of victimization. As Biderman has noted, recalling crime events or the details of them is not an easy task for survey respondents.:

> The survey method is dependent upon the recall of the respondent. This can be particularly unreliable when he is asked to recall a past event which has few serious durable consequences for the victim or demands of further action on his part.[8]

It is reasonable to believe that the amount of error in the victim's account of the crime will be greater for respondents whose crime experience was further in the past. It also is possible that some types of victims forget or distort information more than others. Selective forgetting or distortion of certain aspects of a crime could be a serious problem for persons conducting studies with victimization data. For example, if respondents

tend to distort actual events as time passes so that they recall the incident as being more serious than it actually was, then survey data will overestimate crime seriousness (other things being equal). The types of biases introduced into survey data by misrecall of the date (telescoping) also need to be explored. If some types of victims are more likely than others to telescope crimes into the reference period, then survey data not only will overestimate crime but will overestimate it for certain types of victims rather than for others.

The overall purpose of the Portland forward records check is to identify some of the types of biases in victimization survey data that have not been examined previously and to develop preliminary information about the magnitude and/or existence of such problems in the data.

In order to achieve this objective, comparisons were made between survey-generated information about crime and police information about the same crime incidents. When survey and police information differ, it is impossible to know which is "correct", but certain types of tests were conducted to estimate the amount of error which reasonably could be attributed to survey procedures. A variety of different kinds of analyses were conducted to determine whether certain types of victims or incidents contribute disproportionately to the amount or direction of error in police and survey data. It should be noted that when police and survey information about an incident are the same, this indicates a high degree of convergent validity for both sets of information. If one is willing to assume that survey-generated data about unreported incidents is as accurate or inaccurate as survey-generated data about the reported crimes, then it is possible to draw some conclusions concerning the overall accuracy, reliability, and validity of survey crime information.

The specific purposes of the Portland Forward Records Check are:

1. To describe and analyze differences between survey and police data for a set of 212 matched cases in relation to the classification of the crime, details of the event that are used to produce the classification, the seriousness of the offense, the characteristics of the offenders, and the activities of the police, victims and witnesses during the crime;

2. To describe and analyze the frequency of "don't know" responses in the data;

3. To describe and analyze patterns of telescoping in relation to victim characteristics;

4. To describe and analyze the incidents from the survey that could not be found in the police data even though respondents said the incidents were reported to authorities;

5. To comment upon the implications of the study in terms of the utility of survey (and police) data and to discuss the implications of the research for the methods used to collect survey information.

It should be emphasized at the outset that this study represents the first forward records check of crime victims and one of only a few studies that have compared official records with survey data on crime events. Thus, the study is highly exploratory and designed to suggest areas of future research, identify major issues, and provide very preliminary information about the overall quality of survey data. Furthermore, the study is confined to a single city (Portland, Oregon) and the results are not necessarily generalizable to police departments in other areas nor to surveys undertaken in other places.

PART II

METHODOLOGY

The methodology used in the study is a forward records check of crime events reported to interviewers during the 1974 Portland, Oregon, victimization survey.[9] The forward records check involved selecting all of the crime events which were reported in the Portland survey that occurred within the city limits of Portland and which respondents said were reported to the police. The address of each crime had been coded by street and house number in the original survey data. A search was made of all original police reports for a time period preceding the earliest month of the survey recall period by at least sixteen months. If a crime event was found at the proper address, the report was checked against the survey data in order to determine whether the two events involved the same victim or household. If so, and if the event constituted a "definite match" with the survey data (see definition below), then the search procedures were stopped for that event. If the event did not fit the definite match category and/or if the victim was different than the one on the survey, then the search procedure continued by examining all original police reports involving crime incidents within five square blocks of the location of the survey crime. If no crimes involving the victim or household on the survey were found within five square blocks, the event was classified as a definite "no match". It should be emphasized that a search was made of all officially recorded crimes, regardless of the classification used by the police, for a time period beginning in January, 1972, and continuing through September, 1974. The earliest month of recall required by the survey was April, 1973.[10]

Approximately 16 percent of the survey crime reports contained no

precise address and, therefore, could not be found with an address search. In order to locate as many of the incidents as possible, a name search was initiated for all the survey crime reports in which the respondent had given at least a last name. There were 89 victims who gave their names, and 103 incidents were reported by these persons to the interviewer. (This is approximately 25 percent of the total number of crime incidents that, according to the victim, had become known to the police.) Police department personnel conducted the name search and provided the research group with the report numbers of incidents that might be the ones which matched the survey data. These reports were then pulled and compared against the questionnaires. The name search was not very productive. Only twelve incidents were found through the name search that were not also found through the address search alone.

After all the search procedures were finished, the incidents were grouped into one of three categories, as a first step in developing the final judgment about whether the police report concerned the same crime reported on the survey.

1. Definite Match. A definite match was defined, initially, as a victim and an incident that matched the survey data in virtually all relevant aspects. The rule was that 90 percent or more of the relevant victim/household characteristics should be the same between the survey and the police data. Age should be within two years, sex, race, and occupation should be correct; the address of the incident and of the victim should be the same, the phone number should match, the partial name identifier should match, and any "unique" characteristics of one should match the other "unique" characteristics including such things as "victims were returning from a Trailblazer (basketball) game," "victim was recuperating from an operation," "wife was in the bathtub," "offender entered through a hole in the roof," and so on. Many crime reports (both

survey and official) contained this type of data. At least 90 percent agreement on the details reported in the survey data and on the police form had to be the same in order to establish that a "definite match" had been found. Characteristics of the crime itself could not be used (e.g., classification, date, weapon, location, etc.).

2. Definite No-Match. A definite "no-match" decision could be made if there was no record of a crime having occurred at the location (or within five square blocks of it) against a victim who bore any resemblance to any household member in the survey. In addition, an event was considered an unmatched crime if reference to the event was found in police records but a separate crime report on it had not been filled out. This happened several times in apartment or boardinghouse burglaries. The police filled out a report on the most serious crime and listed the other incidents and their victims in the narrative section of the report. The third type of no-match was the crimes for which the location given by the survey respondent was too vague or was not known at all and the name given by the respondent was not sufficient to use in the name search. Thus, no search could be undertaken for these crimes.

These rules were sufficient to categorize almost all of the incidents either as matches or no-matches. There were, however, 21 survey incidents (four percent of the total) that could not be categorized either as a match or no-match using these criteria. (See Appendix A for a brief description of each.) Decisions on most of these 21 cases were quite straightforward:

1. Four of the survey victims were located in the police files, but the crime recorded by the police occurred after the person was interviewed and there was no record of the victim having notified the police about an offense prior to the date of the interview.

2. Five of the incidents involved a specific victim (rather than the

household) but the victim identified in the police data was not the person claiming to be a victim in the survey even though the former person resided at the household. Furthermore, there was no similarity between the unique identifiers in the police survey and description of the event.

3. Five survey victims were located in police files for an offense that occurred many months prior to the earliest month of the survey recall period and the details of the two events were dissimilar enough that our determination was to judge all five as no-match cases. These decisions were quite difficult to make and more information about each of the events is contained in Appendix A.

4. One survey respondent reported two separate offenses neither of which matched an offense found in police files for the survey victim. The police record, however, could have been a summary of the two incidents in that it bore some similarity to each of the ones reported in the survey. This case was classified as a no-match.

5. Two of the victims reported that they had experienced a series of offenses and multiple entries were found for them in the police data. The survey data, however, obviously was a summary of all the events in the series and therefore none of the police incidents was a match for the survey event. These were considered no-matches.

The problems in determining whether a police event matched the survey event were far less severe than anticipated. Persons who conduct reverse record checks also must determine whether the victim is recalling the same event that was drawn from police files or a different one, but there has been very little discussion of this or of the methodology used to determine whether an event matched or not. Richard Sparks reports that only four of 237 events (two percent) in his London reverse records check did not match

TABLE 1.

RESULTS OF THE FORWARD RECORD CHECK

Category	N	Percent of Total	Percent of Events For Which Search Was Undertaken
Definite Match	212	45%	53%
No Search (Vague Address)	77	16%	--
No-Match			
No record of victim or event or crime at location of survey incident	160	34%	40%
Event found; no separate crime report filed by police	6	1%	2%
Police report of victim or household found but incidents do not match	21	4%	5%
TOTAL	476	TOTAL NUMBER FOR SEARCH	399

the police report closely enough to consider it the same event, but no other authors of reverse survey studies have discussed the problem or the methodology used to match events.[11]

It should be emphasized that some bias could be introduced into a study by the methods and decisions used to match the crimes. If the rules require too much similarity, then the data will show closer correspondence between the characteristics of the survey event and the police event. If the rules require too little similarity, then apparent differences will be introduced into the data which, in fact, are the result of different crimes having been reported to the interviewer and to the police.

The results of the forward records search are shown in Table 1. Sixteen percent of the original 476 incidents contained addresses too vague to permit an address search (or no address at all) and could not be located through the name search. Many of these incidents were robberies, assaults, personal thefts that occurred away from home, and other similar types of incidents in which the victim was not aware of the exact location of the crime. For the crimes that had precise addresses, 53 percent were definitely matched and 47 percent were not. (The analysis of incidents that could not be found is in Section VI.)

PART III

DIFFERENCES IN INFORMATION PROVIDED TO POLICE AND TO INTERVIEWER

This section contains a description and analysis of the types of differences found between survey and police accounts of 212 matched crime events. The analysis focusses on whether differences were produced by survey procedures and whether there is any evidence of systematic bias in the survey data.

Crime Classification

Differences in crime classification between survey and police could be produced by different classification policies or procedures, by human error on the part of those who do the classifying, or by differences in the information obtained from the victims. Of these three possible sources of differences, the latter is of major concern. Differences produced by policies, procedures, or human error can be minimized or even eliminated during the editing phase of data collection and processing, while differences in the raw information received by police and interviewer represent permanent distortions in the data.

Several deviations from UCR classification rules were found in the original data: 12 in police data and eight in the survey. These deviations were corrected in order to analyze classification differences produced solely by the interviewer and/or the police recording different details about the event.

Description of Differences

The police and survey classification for each event are shown in Table 2. Entries along the main diagonal represent the number of crimes classified in exactly the same way from both police and survey information. Entries that are not on the main diagonal are those that were classified differently.

For example, there were 106 crimes classified as burglaries from both the police and survey information. There were three crimes which the survey data indicated as burglaries but the police information produced a miscellaneous classification (usually illegal trespass). Data under the police burglary entry show that there were 116 crimes identified as burglaries but one of these was a rape according to the survey information, eight were larcenies, and one was in the miscellaneous category.

Differences in classification are summarized in Table 3. Ninety-one percent of the incidents were classified into the same major crime category, and nine percent contained sufficient informational differences to produce a different classification. Personal crimes in the 1974 survey were more likely than property crimes to be classified differently (Table 3), but the total number of personal crimes included in the 212 matched set is too small to permit definite conclusions.

Results of the Portland tests for property offenses are very similar to the comparison of police and survey classification conducted in San Jose (Table 4). Using police data as the standard, the San Jose survey correctly classified 97 percent of the burglaries and 82 percent of the larcenies. Survey classification of personal offenses was the same as police classification in 85 percent of the San Jose cases, while the Portland police classified 75 percent of the survey personal crimes into the same categories as the Portland survey. (This difference between Portland and San Jose is not statistically significant.)

Previous comparison of survey and police classifications have used the index of inconsistency (I) as a measure of dissimilarity in classification (see Table 5).[12] Using a 4x4 matrix, the index is .118 for the Portland data and .145 for all crimes shown in Table 5. These figures are quite similar to

TABLE 2.

A COMPARISON OF CLASSIFICATIONS BASED ON SURVEY AND POLICE INFORMATION: PORTLAND

	Classification Based on Police Information[1]								
Classification Based on Survey Information	Rape	Robbery	Assault	Burglary	Larceny	Auto Theft	Miscellaneous	Total Number	Percentage
Rape				1				1	.5%
Robbery		2			1			3	1%
Assault			10				2	12	6%
Burglary				106			3	109	51%
Larceny		1		8	55		1	65	31%
Auto Theft						18		18	8%
Miscellaneous			1	1			2	4	2%
Total Number		3	11	116	56	18	8	212	
Percentage		1%	5%	55%	26%	8%	4%		

[1]Entries along the main diagonal are the number of events classified in the same way from police and survey data. Entries off the main diagonal represent crimes classified differently.

TABLE 3.

CLASSIFICATION SUMMARY BY TYPE OF OFFENSE: PORTLAND[1]

	Classification From Survey Data	Same Classification: Police		Different Classification: Police	
		N	%	N	%
Personal (Total)	16	12	75	4	25
Rape	1	0	--	1	--
Robbery	3	2	--	1	--
Assault	12	10	83	2	17
Property (Total)	196	181	92	15	8
Burglary	109	106	97	3	3
Larceny	65	55	82	10	18
Auto Theft	18	18	100	0	0
Miscellaneous	4	2	--	2	--
Total	212	193	91	19	9

[1]The table can be read in the following way: According to the survey data, there were 16 personal crimes. Of these, the police data produced a personal crime classification for 12 and a different classification for four.

TABLE 4.

CLASSIFICATION DIFFERENCES BY TYPE OF OFFENSE: SAN JOSE[1]

	Same Classification By Interviewer		Different Classification By Interviewer		Total
	N	%	N	%	
Personal	111	85	19	15	130
Rape	24	80	6	20	30
Robbery	54	89	7	11	61
Assault	33	85	6	15	39
Property	147	91	15	9	162
Burglary	91	97	3	3	94
Larceny	56	82	12	12	68

[1]These data are from "San Jose Methods Test of Known Crime Victims," Statistics Technical Report No. 1, NILECJ, June 1972, Washington, D.C.

TABLE 5.

INDEX OF INCONSISTENCY IN CRIME CLASSIFICATION

Survey	4x4 Matrix Index of Inconsistency[1]	Crime	2x2 Matrix (Portland only) Index of Inconsistency
Washington, D.C.	.326	Assault	.137
Baltimore	.168	Burglary	.121
San Jose	.147	Larceny	.119
Portland:			
4 crimes	.118		
7 crimes[2]	.145		

[1]Crimes included in all surveys (4x4 matrix) are assault, burglary, larceny and robbery. Data from the other surveys are from "San Jose Methods Test of Known Crime Victims," Statistics Technical Report No. 1, NILECJ, June 1972, Washington, D.C.

[2]The seven crimes (7x7 matrix) also include rape, auto theft, and miscellaneous part II offenses.

those from the San Jose and Baltimore studies.

Discussion of Information Differences

The most striking discrepancy in classification is the survey rape incident that matches a police burglary. The details of the event as taken down by the police and by the interviewer were very similar. Nevertheless, slight differences on the two reports concerning what the victim claimed that the offender said could have produced the classification discrepancy. (A woman who was taking a shower was surprised by the entrance of a man through her bathroom window. His remarks to her, as quoted on the questionnaire, resulted in a code of attempted rape, whereas the remarks written in the police report resulted in a code of attempted burglary.)

There were eight incidents in which the survey information indicated larceny and the police data produced a burglary classification. In all eight, the proper classification almost certainly was burglary but the survey contained insufficient information to distinguish between the two types of property crime. Seven of the eight involved the theft of a bicycle which, according to the survey data, had been stolen from the premises. This was interpreted as being in the yard and therefore a larceny. The police record in each of these cases indicated that the bicycle was on the porch (appurtenance to a dwelling) or in the garage or in the house, resulting in a classification of burglary. This problem could be corrected with better probes by the interviewer, and with greater awareness of the fact that classification requires a distinction between items stolen from buildings or attachments to buildings and from other places on the premises. The other larceny-burglary difference involves the theft of a car battery. The survey data indicated that the battery was stolen from the car on the premises of the residence. The police report said that the car was in the garage.

Two offenses were classified from the police record and the survey data as robberies and there were two offenses classified as robbery from one but not the other set of data. One of these was a purse-snatch and the difference in classification was produced by differences in the survey and police information pertaining to whether the offender knocked the victim down. The other classification difference was quite similar except it involved a pickpocket.

Two incidents were classified as assaults according to survey information and as miscellaneous offenses from police information. One of these was a case in which the police data indicated no weapon was used, while the victim told the interviewer the assailant had a tire iron.

The information in Table 6 contains a more precise breakdown of police and survey information on several details of the events. The last column of the table indicates the proportion of incidents in which both the police and the survey data were the same. For most of the informational items listed on the left, there is substantial agreement between the survey and police record as to whether or not the crime event was characterized by that detail. For example, the survey data indicated that there were ten incidents in which the offender hit or attacked the victim whereas the police data indicated eleven such events. The two sources of information agreed on ten of the events (both indicated these ten victims had been hit or attacked), but there was one crime of this type in the police data whereas the survey indicated there had not been an attack.

The most substantial differences in the aggregate data (and even these are rather minor) are whether the offender had a weapon and whether there was evidence of forcible entry. The survey data indicated that eight percent of the matched events were characterized by the presence of a weapon, while the police records indicated that four percent involved a weapon. There were eight incidents in which both the survey and the police data agreed that a

TABLE 6.

INFORMATION DIFFERENCES ON DETAILS OF EVENT[1]

Detail of Event	Survey N	Survey %	Police N	Police %	Number Different	Frequency of Same Details: Number Same With Characteristic	Frequency of Same Details: N Same Without Characteristic	Percentage Agreement
Offender hit or attacked victim	(10)	5	(11)	5	1	10	201	99.5%
Victim was threatened with harm	(12)	6	(15)	7	7	10	195	96.7%
Offender had a weapon	(18)	8	(9)	4	11	8	193	94.8%
Physical injury	(10)	5	(12)	6	8	7	197	96.2%
Medical attention needed	(4)	2	(2)	1	2	2	208	99.0%
Property taken or damaged	(183)	86	(187)	88	8	181	23	96.2%
Offender had right to be there	(10)	5	(13)	6	11	6	195	94.8%
Offender actually got in	(99)	47	(108)	51	27	90	95	87.3%
Evidence of forcible entry	(82)	39	(69)	33	25	63	124	88.2%

[1]Data in the first four columns shown the frequency (and percent) of events characterized by the informational detail on the left. The percent of total agreement represents the proportion of all 212 cases which both the survey and the police agreed on whether the characteristic was present or absent (e.g., 212 minus the number of cases coded differently divided by 212).

weapon had been present. The survey data suggest that 39 percent of the matched cases involved evidence of forcible entry; the police estimate is that 33 percent of the cases had evidence of forced entry. There were sixty-three crimes that both sources of information agreed had evidence of forced entry.

In general, the survey and police data were in substantial agreement on whether an event was or was not characterized by a particular detail.

Discussion of Original Classification Errors

As noted previously, several discrepancies from UCR classification rules were found in the original survey and police data. In most instances, UCR rules are precise enough to yield a single code that is not subject to disagreement, if the "facts" upon which the code is based are clear. There were twelve errors made by the original team of survey coders--an error rate of about five percent. More than half of these errors involved the coder overlooking the UCR rule concerning the difference between burglaries (entry into a structure) and larcenies. There was no particular pattern to the survey errors; above half were of the incidents involving errors classified as more serious and half as less serious than the proper classification.

The discrepancies in police classification apparently were produced by police policies rather than by minor lapses in concentration by the coders. In particular, the Portland police seem to down-classify certain types of assaults involving family members or juveniles. Of the twelve assaults found in police records, seven were coded by the police as simple assaults where the information indicated an aggravated assault. The UCR classification system divides aggravated and simple assault in accordance with whether a weapon was used, injury occurred, and in the final determination, the intent of the assailant. The five incidents down-classified to simple assault by

the police involved either family members or victims and offenders who were under the age of eighteen. There were five apparent assaults classified by the police either as malicious mischief or threats. One of these was a threat against a female victim's life by a man she knew.

The major conclusions from this part of the analysis are:

1. Although slight informational differences exist between the survey and police records of many crime events, the difference is seldom extensive enough to produce a difference in crime classification. For the 212 matched incidents, ninety-one percent had information similar enough to produce the same crime classification, whereas nine percent contained informational differences that would result in different classifications.

2. The similarity of classification was greater for property crimes than for personal crimes with 25 percent of the latter being misclassified because of different information. There were, however, too few personal crimes in the matched incident set to draw definite conclusions from the analysis.

3. The proportion of survey cases and police cases that were characterized by any one of several information details is very close (in the aggregate), and only slight differences exist on a case-by-case basis. The most striking difference between the police and survey data pertains to whether the offender had a weapon or did not have one. There were 22 cases in which either the survey or the police data indicated that the offender had a weapon, but only eight of these were coded as having a weapon by both the survey and the police. In general, however, the police and survey characterized more than 90 percent of the incidents in the same way on details including those involving physical violence, type of entry, right of offender to be there, and evidence of forcible entry.

Crime Seriousness

Two different types of seriousness measures are used to determine whether there is any systematic over or underestimation of seriousness in the survey data, compared with police records of the same events.

The first seriousness scale is a replication of the Sellin and Wolfgang index (see Appendix B) and the second is the amount of monetary loss from the crime. As shown in Table 7, the survey data produced slightly higher estimates of crime seriousness than did the police information.

The frequency of agreement (last column of Table 7) is calculated in terms of the incidents which both the police and survey agreed were or were not in each category of crime seriousness. Clearly, these values are inflated because most crimes are not in any one category. For example, 27 incidents were given a seriousness score of three by both the police and the survey data, but 36 incidents were placed in this category by one source of information but not the other. Both agreed, however, the 149 cases did not belong in the third cateogry. The product-moment correlation between the survey and police seriousness scores is .63 (r^2 = .40). Again, this suggests that there are considerable case-by-case differences in the seriousness estimates.

A considerable portion of the survey's higher estimates of seriousness is produced by two indicators used in the scale. The survey data were more likely to indicate that the offender had a weapon (as shown previously) and generally provided higher estimates of the amount of loss from the crime.

Comparisons of survey and police information on amount of loss are shown in Table 8. In every type of comparison, the survey estimates are higher than those provided by the police even though the correlation coefficients between estimates of loss are rather high.* The implication is that either the survey

*Because of several very large losses, the data were badly skewed. To correct this problem, the natural log of each value was taken and the transformed values were used in the correlation analysis.

TABLE 7.

SERIOUSNESS OF OFFENSES[1]

Seriousness Score	FREQUENCY IN CATEGORY Survey N	Survey %	Police N	Police %	FREQUENCY OF AGREEMENT IN CASE-BY-CASE DATA Agreed: Not In Category	Agreed: In Category	Disagreed	% Total Agreement
0	7	3	7	3	203	5	4	98%
1	18	8	33	16	173	8	31	85%
2	79	37	95	45	95	57	60	72%
3	51	24	39	18	149	27	36	83%
4	35	17	26	12	169	18	25	88%
5	4	2	3	1	207	2	3	99%
6	10	5	4	2	199	1	12	94%
7	5	2	2	1	206	1	5	98%
8 or above	3	1	3	1	206	0	6	97%
$\bar{x}$	2.9		2.5		TOTAL # cases with same seriousness score	119		
s.e.	.13		.10		% of cases scored the same	56		

[1]An explanation of scoring for the seriousness scale is in Appendix B.

TABLE 8.

SURVEY AND POLICE ESTIMATES OF LOSS FROM CRIME

Type Of Offense	% of Cases with No Loss Indicated		Average Dollar Loss		Average Loss Excluding "No Loss" Category		Median Loss	
	Survey	Police	Survey	Police	Survey	Police	Survey	Police
Burglary	19	21	$548	$412	$680	$522	$300	$155
Larceny	12	14	$126	$96	$143	$112	$100	$75
Auto Theft	10	56	$662	$186	$736	$419	$500	$260
All Incidents	16	21	$412	$319	$488	$357	$120	$75

Burglary:[1]	r = .81
Larceny:[1]	r = .77
Auto Theft:[1]	r = .60
All Cases:[1]	r = .82

[1]Correlation coefficients derived from dollar values after the natural log of each value was taken.

respondents systematically overestimated the amount of loss or the police underestimated it. In the auto theft category, there were many police reports which contained no value at all for the stolen car, and this greatly inflated the difference between survey and police estimates of loss. The average loss of burglaries is considerably higher than might be expected because of one incident that involved a loss of more than $14,000 according to both the police and the survey information.

Comparisons between the Portland and San Jose data are shown in Table 9. In the Portland data, the average and median survey values are about 25 percent higher than the police, with the exception of median burglary loss which is 48 percent higher. On the whole, the San Jose survey data are 24 to 33 percent higher than the police.

A question of considerable interest is whether the differences are errors in the survey data and, if so, what type of bias exists in the survey information in addition to a general overestimate (in comparison with police data) of crime seriousness and loss. It is impossible to make any definitive determination of whether the differences are due to survey error, but some indirect information can be developed concerning the amount of error attributable to memory loss or distortion that occurred between the time the event was recorded by the police and when the interview took place. The police data, of course, are obtained shortly after the crime, while the survey information is obtained later and in some cases as much as 12 months after the event. The analysis of whether differences in the seriousness estimates are attributable to memory loss (and, therefore, represent error in the survey data) is based on three assumptions:

1. It is reasonable to assume that respondents forget and/or distort information as a function of the time lag between when the event occurred (as

TABLE 9.

COMPARISON OF PORTLAND & SAN JOSE DATA: LOSS FROM CRIME[1]

	PORTLAND			SAN JOSE		
	Survey (1)	Police (2)	% Difference (1) - (2) / (1)	Survey (1)	Police (2)	% Difference (1) - (2) / (1)
Average Loss						
Larceny	126	96	23.8	358	240	33.0
Burglary	548	412	24.8	598	432	27.8
Median Loss						
Larceny	100	75	25.0	200	152	24.0
Burglary	300	155	48.3	379	270	28.8

[1] San Jose data are from "San Jose Methods Test of Known Crime Victims." See Table 4.

measured by police information) and when the interview took place. Thus, a significant correlation between the amount of error and the length of time (in months) that elapsed between the crime and the interview would be considered evidence of memory loss and survey error.

2. There have been some studies which suggest that memory decay and distortion is more pronounced and occurs more rapidly for older persons than for younger ones. If this is the case for crime seriousness information, then positive correlations between the victim's age and the amount of error would indicate that some proportion of the differences between survey and police data is attributable to the survey.

3. There is one type of error which is known to exist in the survey rather than in the police data. Errors in recalling the date of the event (telescoping) are attributable almost entirely to the survey. If it is assumed that persons who make one type of error also tend to make other types of errors, then a positive correlation between telescoping and the other differences would indicate that some portion of the erroneous information is attributable directly to the survey.

The subsequent analysis also contains information on two other questions:

The first is whether crime victims systematically distort information by overestimating the seriousness of the event as a function of longer time lags between the crime and the interview. If so, then survey data generally would contain higher estimates of seriousness than found in police data because of the time lag. Furthermore, if such distortion occurs, longer recall periods would result in the data containing more error of this type than would shorter recall periods.

The second question is whether there are any particular types of people for whom the survey data consistently show higher (or lower) estimates of

seriousness than police data. If there are, then the survey information will provide a different (and perhaps erroneous) portrayal of crime loss and seriousness among various population subgroups.

The results of the analysis are shown in Table 10. The amount of difference between police and survey seriousness scores and dollar loss (absolute values of the differences) is not related to the time lag between the crime and the interview, nor to the age of the victim, nor to error in the recall of the data (net telescoping). Net difference in dollar loss is related to time lag ($r = -.14$) but the negative correlation means that crimes which occurred further in the past were more likely than recent ones to contain similar estimates of dollar loss in police and survey data. Thus, the data support the contention that differences between survey and police information were not produced by memory loss or distortion occurring between the time the police recorded the information and the time of the interview.

The weak and statistically insignificant (at the .05 level) correlations between overestimates of seriousness and the amount of time that elapsed between the crime and the interview suggest that respondents did not systematically accentuate the seriousness of the crime as time passed and did not distort information in such a way as to recall the event as being less serious.

Three characteristics of respondents were analyzed in relation to the direction and amount of differences between police and survey information concerning crime seriousness. These data indicate that race, sex, and educational level are not related to the type of differences nor to the absolute amount of differences.

The original survey data included several questions designed to tap the respondent's attitudes toward the police. These were correlated with the amount and direction of differences in order to determine whether persons who

TABLE 10.

CORRELATES OF OVER- AND UNDER-ESTIMATING CRIME SERIOUSNESS[1]

		Seriousness Scale: Over-estimates	Dollar Loss: Over-estimates	Seriousness Scale: Net Differences	Dollar Loss: Net Differences
(a)	Time Lag from Crime to Survey Interview	-.01	-.09	.01	-.14*
(b)	Net Telescoping	.00	-.09	.01	-.14
(c)	Age	-.02	.07	.03	.04
(d)	Race (0=black; 1=white)	.00	-.01	.01	-.01
(e)	Sex (0=female; 1=male)	.00	-.05	.05	.00
(f)	Education	.01	-.10	.03	-.00
(g)	Positive Attitude Toward Police	.00	.00	-.01	-.04

*P < .05

[1]See Appendix B for how the seriousness scale was developed.

hold more positive attitudes differ, in any systematic way, concerning the direction or magnitude of differences between the information they provided the interviewer and the information recorded by the police. As shown in Table 10, the type and amount of differences between survey and police data are not related to the respondent's attitude toward the police.

Characteristics of Suspects

Respondents to the survey were asked whether they knew how many persons were involved in the crime, age of the suspects, race, sex, and whether the person(s) was a stranger or was known to the victim. Similar information was obtained from the original police reports for each of the matched incidents. The Portland police records contain the victim's original description of the offender (if any) in the narrative section of the report as well as updated information. It was not possible to determine when the police information on a suspect was entered on the report. Thus, if the police obtained information on a possible suspect and did not notify the victim, then the data would differ for this reason.

Race of Suspect

Both the police and the survey data indicated that 28 of the offenses were committed by whites, but the two sources of information agreed that a white person was a suspect on thirteen incidents and disagreed on the others (see Table 11). Survey data indicated that 31 incidents involved a black suspect whereas the police records showed that black persons were suspected in 25 incidents. There were 129 crimes for which neither the survey nor the police data contained any information about a suspect (61 percent of the total).

The total amount of agreement between police and survey data consists of

the number of incidents on which both agreed on the racial characteristic of the suspect or agreed that the suspect was unknown. The two sources agreed on 74 percent of the incidents and disagreed on 26 percent. Clearly, the greatest amount of agreement, in absolute terms, is that the race of the suspect was unknown (129 cases). If these are excluded, the agreement between police and survey data concerning racial characteristics of suspects is only 34 percent.

Victimization survey data may not be an accurate reflection of racial characteristics of offenders if victims project racial bias or prejudice into their perception of who committed the crime. The data in Table 11 show that there were 31 black suspects, according to the victims, but more than half of the police reports on these incidents (55 percent) indicated that the suspect had unknown racial characteristics. Of the suspects identified in the survey as white, 46 percent were recorded as unknown, black or "other" in the police data. Although the number of cases is very small, the data indicated that these victims slightly overestimated the number of incidents involving black suspects in comparison with police estimates of whether the suspect is white or black.

A similar phenomenon is found when one examines survey responses concerning racial characteristics of persons that the police data show as unknown. Of these incidents, there were 27 that survey respondents claimed to have information on the racial characteristics. Eight (30 percent) were characterized as white compared with 70 percent as either black or other. Police data, compared with survey "unknowns" do not show this pattern. There were 149 cases of unknown suspects according to survey respondents, of which the police records contained racial information on twenty. More than half were characterized as white (55 percent) and 45 percent were characterized as black or other.

TABLE 11.

RACE OF SUSPECT

	POLICE				
	White	Black	Other	Unknown	Totals
SURVEY					
White	15	3	2	8	28
Black	1	13	0	17	31
Other	1	1	0	2	4
Unknown	11	8	1	129	149
Totals	28	25	3	156	212

Total Agreement: 157/212 = 74%

Agreement Excluding Unknown Category: 28/83 = 34%

Additional analysis of the data show that black victims, rather than white, were primarily responsible for overidentification of suspects as black when police data contain no information on racial characteristics of the suspects. For white victims, there were twenty cases in which the police did not record any information on race of the suspect. The white victims told the interviewer that twelve of these (60 percent) were white and eight (40 percent) were black. Black and other nonwhite victims provided information on seven cases that the police said involved an unknown suspect and the victims indicated that five of the seven were black rather than white.

The data presented in the previous tables indicate that victims had a very slight tendency to suspect blacks when the police data indicated the suspect was unknown, but there is no evidence at all that this is due to white victims projecting racial bias into their identification of suspects. Black persons "oversuspected" blacks to a greater extent than whites did. Even though the data do not indicate systematic bias, it should be emphasized that the extent of agreement between survey and police records is very low. The lack of agreement casts doubts on the reliability of racial information suspects.

Offender Known or Stranger

It is widely suspected that victimization surveys underestimate the proportion of incidents committed by persons known to the victim. This phenomenon could be produced by the greater saliency of stranger-perpetrated incidents and a corresponding inability by victims to remember offenses committed by persons they know. It could be due to victims being reluctant to tell the interviewer about incidents committed by friends, acquaintances, or household members. Another possibility, and the only one which can be examined with the matched incident set, is that victims report the crime to the interviewer but do not provide accurate information concerning the fact that they knew who

the offender was. The data in Table 12 do not show any support for this possibility, however. If the police records are correct with regard to whether the suspect is known to the victim or not known, then the survey elicited the correct response in 52 percent of the cases that the police said involved persons known to the victim. The survey elicited the correct response in 58 percent of the cases that police data show involved a stranger. The differences in survey inaccuracies are not sufficiently great to conclude that victims intentionally fail to tell the interviewer that they were acquainted with the suspect. Again, it should be emphasized that the general lack of agreement between the two sets of data casts doubt on the reliability of this information.

Other Characteristics of Suspects

The victimization data did not differ much from police records in terms of the average age of suspects, the number of offenders, or the sex of offenders (Table 13). The average age, from both sources of data, was between 18 and 19 years and both indicated that approximately 30 percent of the suspects were known to be male (most of the others were unknown). There were no discrepancies in information about sex of offender. None of the females identified in the survey were identified as males by the police and none of the males were identified as females.

The major conclusion to be drawn is that the survey and police data generally provide very similar aggregate portrayals of the characteristics of offenders even though there is substantial case-by-case disagreement between the two sources, especially on the race of the suspect and the relationship between the suspect and the victim. The implication of this is that either

TABLE 12.

OFFENDER KNOWN OR STRANGER

	POLICE			
	Stranger	Known	No Data	Total
SURVEY				
Stranger	25	6	16	47
Known	2	13	9	24
No Data	16	6	119	141
Total	43	25	144	212

Total Agreement 157/212 = 74%

Agreement Excluding No Data Category 38/93 = 41%

TABLE 13.

OTHER CHARACTERISTICS OF SUSPECTS

	Survey	Police
Age of Suspect[1] ($\bar{x}$)	18.2	18.7
Number of Suspects ($\bar{x}$)	1.8	1.6
Percent of all incidents with male identified as suspect	30%	30%

[1]This includes estimated age of youngest and oldest suspects.

source of information would provide about the same description of suspects. However, if one wished to analyze correlates of offender characteristics, there are two problems. The first is that one or the other source of data contains considerable case-by-case error (or both have considerable error) which could produce different results from the analysis dependent upon which data set was used. If the error is random, then the strength of association would be diminished, but the results should be the same regardless of whether one conducted the analysis on survey data or on police data.

Analysis of Differences

The data shown in Table 14 provide information on whether differences between survey and police descriptions of offenders are attributable to memory loss or distortion as well as information on whether certain types of victims systematically make certain types of errors.

Positive correlations between the absolute amount of differences and the time lag, net telescoping, or age of the victim would constitute indirect evidence that differences are at least partially attributable to memory loss or distortion and, therefore, represent errors in the survey data. Again, there is no evidence of this from the Portland data. The amount of time that elapsed between the crime and the interview is not significantly correlated with differences between the two sources of data. This indicates that respondents' recollections were just as accurate (or inaccurate) for distant events as for recent ones. Persons who made more errors in recall of the correct date for the crime (net telescoping) are not characterized by greater differences in data about suspects. Moreover, the age of the victim is not correlated significantly with the magnitude of differences between survey and police data.

TABLE 14.

CHARACTERISTICS OF VICTIMS AND RESPONDENTS	CORRELATES OF ABSOLUTE AMOUNT OF DIFFERENCES IN OFFENDER CHARACTERISTICS[1]				CORRELATES OF DIRECTION OF DIFFERENCES IN RECOLLECTION ABOUT OFFENDER CHARACTERISTICS[2]			
	RACE N=36	OFFENDER STRANGER OR KNOWN N=46	NUMBER OF OFFENDERS N=43	AGE OF YOUNGEST N=35	RACE AS WHITE N=36	OFFENDER AS STRANGER N=46	NUMBER OF OFFENDERS N=43	AGE OF YOUNGEST N=35
Time lag from incident to interview	-.12	-.21	-.02	.13	-.11	-.08	-.07	.10
Net telescoping	-.16	-.19	-.06	.08	-.13	-.06	.07	-.06
Age	-.00	.19	-.02	.26	-.06	.07	-.13	.06
Seriousness of crime (survey estimate)	-.30*	.03	-.08	-.05	-.08	.13	.07	-.02
Seriousness of crime (police estimate) .	-.20	.03	-.07	.09	-.06	.13	.02	-.23
Race (0=black; 1=white)	.15	-.06	-.06	.11	.06	-.12	-.14	-.02
Sex (0=female; 1=male)	.35*	-.19	-.09	.13	.06	-.19	.04	-.18
Education	.21	-.06	.10	-.08	.04	.01	-.15	-.06
Positive attitudes toward police	NA	.09	.02	-.16	NA	-.09	-.13	.07

*P < .05

[1]Positive correlations mean that a higher score on the characteristic is related to greater error (differences between the survey and police data). An asterisk indicates the correlation coefficient is significant at the .05 level.

[2]Positive correlations mean that higher scores on the characteristic listed on the left are related to the survey data "over-reporting" (or the police data "under-reporting") the characteristics of offenders listed across the top. An asterisk means the correlation is significant at the .05 level.

Another proposition that was tested concerns whether the survey and police information about offenders is more accurate (e.g., more similar) for serious crimes than for less serious ones. One might propose that some of the differences between survey and police information are attributable to the tendency of victims to forget information about trivial crimes more rapidly than they forget information about serious crimes. The latter, being more salient, should be recalled with greater precision. The data in Table 14 provide very weak support for the proposition. The seriousness of the crime as measured from the survey data and from the police data is significantly correlated with only one of the eight types of error at the .05 level. The negative relationship ($r = -.30$) between survey estimates of crime seriousness and errors in race of the suspect indicate that more serious crimes tend to be characterized with fewer errors.

Characteristics of the victim generally are not correlated with the amount of error, but there is one exception to the pattern. Crime incidents involving men are characterized by more differences between police and survey information about the race of the suspect. It should be emphasized, however, that 36 different relationships were tested. Using the .05 significance level, one would expect to find one or two statistically significant correlations by chance alone. Thus, substantive significance should not be attributed to the significant correlations in Table 14 unless they are replicated in other studies.

The data in Table 14 also show correlation coefficents between selected independent variables and the direction of differences in survey and police information. There are two major purposes for examining correlates of the direction of the differences in police and survey data concerning offender characteristics. The first is to determine whether the time lag between the

crime and the interview date is associated with memory distortion concerning offender characteristics. As shown, there are no significant correlations; this indicates that survey information about suspects does not become distorted as a function of time lag.

The second major purpose is to determine whether certain characteristics of the incident or the offender are associated with systematic differences between police and survey information. This is of interest to persons who might be using survey data to test propositions involving offender types and any of the independent variables shown in the table. For example, one might test the proposition that younger offenders commit less serious crimes than older offenders. If this were tested and a significant correlation obtained from survey data, one would have to consider the possibility that victims underestimate the seriousness of an offense if it is committed by a younger person, or, conversely, that victims overestimate the age of the offenders as a direct function of the seriousness of the crime. Another example would be a study in which the researcher used survey data to test the proposition that younger victims are more apt to be involved in crimes perpetrated by younger offenders. If support were found for the proposition, one would have to consider whether or not victims tend to distort the age of suspected offenders to be closer to their own age.

There are, however, no statistically significant correlations between the direction of differences and characteristics of the victim or the crime incident.

Activities of Victims and Police

The victimization survey included questions on whether the victim tried to prevent the crime, whether there were other persons who saw or heard what

was happening, how long it took the police to arrive (if they were notified), and what the police did after they arrived.

Very little is known about the accuracy of victim responses to questions of this type. One could speculate that victims will over-report the amount of effort exerted to prevent the crime in order to provide the interviewer with a more socially accepted response. There are no particular reasons to believe that survey respondents would misstate or misperceive the presence of other persons, but it is possible that laymen use somewhat more lenient "rules" in determining who is a witness than the police would. It is quite reasonable to expect victims to overestimate the amount of time required for police officers to arrive at the scene of a crime, due to the generally accepted idea that time (subjectively) seems longer in crisis or emergency situations than is actually the case. It also is reasonable to expect that survey data would provide underestimates--compared with police accounts--of the number of activities undertaken by the officers after they arrive. Victims may not be very astute observers of what the police do; they may forget to mention certain types of activities, since the question is open-ended and not designed to jog their memories. On the other hand, the police could overstate their own activities, or they could define certain types of things such as "investigation" differently than the victim. Comparisons of police and survey data on these topics are shown in Tables 15 through 18.

A considerable amount of agreement exists between survey and police records concerning whether the victim attempted self-protection, but there was a slight tendency for the survey respondents to overreport their activities (or the police to underreport them). Of the fifteen incidents in which the police data showed that the victim attempted some type of self-protection, only four were "missed" by the survey. There were, however, twelve victims

who told the interviewer that they tried to protect themselves, but the police report did not indicate that they did. Nevertheless, there is agreement between the two sources of information on 92 percent of the incidents. Even when incidents that occurred in the absence of the victim are excluded, there is 84 percent agreement between the two sources of data.

A similar level of agreement was found concerning the presence or absence of witnesses (Table 16). Most cases did not involve any known witnesses and both sources of data provided similar estimates of the proportion of cases which had and did not have witnesses. There is some disagreement, however, concerning exactly which cases involved witnesses. Of the 41 incidents that police records show involved witnesses, 24 (59 percent) were attributed in a similar way by the survey data. Of the 44 cases that the survey respondents said involved witnesses, there were twenty which the police records showed involved no witnesses.

Survey respondents consistently overestimated the amount of time before the police arrived (Table 17), or the police underestimated it. There were only two survey respondents who estimated the time to be shorter than what police records showed. Almost half the respondents estimated the time within fifteen minutes of the estimate given on the police report, and the other half of the respondents said that the time was at least fifteen minutes longer than indicated by the police report.

The data in Table 18 indicate that the survey respondents recalled a smaller number of police activities than shown in police records.[13] It should be noted that the survey responses were to an open-ended question concerning what the police did after they arrived. Virtually all of the other survey data analyzed in this research were obtained from direct rather than open-ended questions. It is possible that the underestimation of police activities is

TABLE 15.

VICTIM SELF-PROTECTIVE ACTIVITIES

	POLICE			
	No	Yes	UK, NA	Totals
SURVEY				
No	71	3	0	74
Yes	12	11	0	23
UK, NA	0	1	109	110
Totals	83	15	109	207

Total Agreement 191/207 = 92%

Agreement Excluding Unknowns 82/98 = 84%

TABLE 16.

PRESENCE OF WITNESSES

	POLICE		
	None	Some	Totals
SURVEY			
None	151	17	168
Some	20	24	44
Totals	171	41	212

Total Agreement = 83%

TABLE 17.

POLICE RESPONSE TIME

SURVEY ESTIMATE	N	%
Shorter than police record	2	1
Same as police (within 15 minutes)	75	48
Survey 15 minutes longer	45	29
Survey 45 minutes longer	15	10
Survey 90 minutes longer	1	1
Survey 2 to 5 hours longer	12	8
Survey 6 to 15 hours longer	5	3
No data (57)		

TABLE 18.

POLICE ACTIVITIES

Survey Data: Number of Police Activities:	Police Data: Number of Police Activities 0	1	2	3	4	Totals	%
0:	3	24	2	0	1	30	14%
1:	1	86	45	8	2	142	67%
2:	0	15	16	2	1	34	16%
3:	0	1	4	1	0	6	3%
4:	0	0	0	0	0	0	0%
Totals:	4	126	67	11	4	212	
%:	2%	59%	32%	5%	2%		

Survey $\bar{x}$ = 1.08

Police $\bar{x}$ = 1.46

Total Agreement = 50%

partly due to the open-ended question, and that direct inquiries concerning whether the police "warned the offender," "restored order," "arrested the offender" and so on would have resulted in a greater volume of activities being reported in the survey.

The amount and direction of differences concerning activities of victims, witnesses, and police were correlated with selected independent variables and the results are shown in Table 19.

Incidents that occurred further in the past contained no more absolute error than recent ones, but the more distant events were characterized by an underreporting of the presence of witnesses. Individuals who made more errors in recall of the date made more errors in recall of victim activities (r = .22) but fewer errors in recall of police activities (r = -.14). Older persons were more inclined than younger ones to underestimate the presence of witnesses, but otherwise age was not significantly correlated with either the amount nor the direction of differences. The seriousness of the crime (police estimate) was not associated either with the amount nor direction of error. The survey estimate of crime seriousness, however, was significantly related to police activities, indicating the possibility that victims whose reports to interviewers resulted in higher seriousness scores tended to underestimate police activities.

Incidents involving men were more subject to errors concerning the amount of victim activity to prevent the crime, but there is no indication that men systematically overreport the amount of activity to the interviewer. The correlation between race and the errors in information concerning the presence or absence of a witness indicates that there were more errors in incidents involving whites than blacks, but there is no consistent direction to the differences.

TABLE 19.

VICTIM AND POLICE ACTIVITIES

Characteristic	Correlates of Error: Amount				Direction of Error: Survey Respondents "Over-Report"			
	Victim Activities r N~95	Presence of Witnesses r N~136	Police Response Time r N~152	Police Activities r N~175	Victim Activity r N~95	Presence of Witnesses r N~136	Police Response Time r N~152	Police Activities r N~175
Time lag from incident to interview	.08	-.04	-.02	-.03	-.04	-.18*	-.03	.06
Net telescoping	.22*	.01	.04	-.14*	.08	-.05	.02	.04
Age	.03	.11	-.04	.03	-.05	-.15*	-.07	-.09
Seriousness of event (survey)	.01	.10	-.09	.04	-.01	.12	-.05	-.13*
Seriousness of event (police)	-.00	.06	-.02	.02	-.01	.03	.01	.09
Race (0=black; 1=white)	.03	-.19*	.09	-.04	-.02	-.02	.06	-.09
Sex (0=female; 1=male)	.18*	.05	-.05	.09	.08	.04	.00	-.18**
Education	-.07	.13	.03	.02	-.06	.08	-.05	.03
Positive attitude toward police	.00	-.10	-.02	-.06	-.14	-.15*	-.10	.15*

*P<.05 **P<.01

The relationships between attitudes toward the police and all four of the variables representing direction of error are quite suggestive even though only two of the correlations are statistically significant. The correlations, although weak, suggest that persons who have negative attitudes toward the police underestimated police activities, overestimated the length of time before the police arrived, overestimated the presence of a witness, and overestimated the extent of their own activity. The general syndrome suggested from these data is that people with negative attitudes accentuated their own activities (and perhaps those of witnesses) in preventing the crime and downgraded the role of the police.

All of the analysis in this section focussed on differences in information found on the police report and the survey interview form. The evidence suggests that differences in information were not, with a few exceptions, attributable to memory loss or distortion and, for the most part, were not correlated with victim characteristics. This provides some assurance to those who use survey data that, when respondents provide answers to certain questions the answers are relatively unbiased and, with some exceptions, are generally valid indications of what happened. The converse also is true: if police departments have record-keeping procedures as good as those in Portland, Oregon, then the researcher can be relatively confident that the information recorded by the police is a valid indication of what happened.

Respondents in the survey, however, may forget information (or not wish to tell the interviewer about it) and can answer the interviewers question by saying that they do not know the answer. "Don't know" responses to the interviewer and the absence of information on the police report form were both excluded from the analysis in this section and were treated as missing data.

PART IV.

AN ANALYSIS OF "DON'T KNOW" RESPONSES

Four questions will be considered in this section:

1. To what extent does the frequency of "don't know" responses in survey data increase as the amount of time increases between the crime and the interview? If there is a positive relationship between longer time lags and the frequency of don't know responses, then this is evidence that survey data will not be as complete as police data and that longer recall periods exacerbate the problem.

2. To what extent do persons who make errors in recall of the date tend to give don't know answers to the interviewer? It was shown previously that incidents containing errors in the date were no more likely than other incidents to contain different details in the police data, but it may be that persons who err on the date give don't know responses on many of the details.

3. Are trivial crimes more likely than serious ones to result in don't know responses to the interviewer?

4. Are certain types of victims more likely to provide don't know answers than other types of victims?

Two dependent variables were developed for the analyses. The first involves incidents in which the police record had information about the event, whereas the survey had no information at all (originally coded as don't know, refused, or simply no entry coded of any type). The second dependent variable involves incidents in which the survey had information, but the police report did not.

The information elements included in the "don't know" scales are: (a) time of day when the incident occurred; (b) whether the offender actually

got in; (c) how many offenders; (d) youngest and oldest age of offenders; (e) whether the offender was known or a stranger; (f) race of the offender; (g) whether the offender had a weapon; (h) the total dollar loss from the crime.

Results of the Analysis

The first results are shown in Table 20. Positive correlations would indicate that higher values on the predictor variables are related to the police report containing information that is missing in the survey data. The interpretation would be that the information was available when the police investigated the incident but was forgotten by the time of the survey interview or was intentionally suppressed during the interview.

The amount of time that elapsed between the crime and the interview is not correlated significantly with the frequency of don't know responses. This finding is consistent with the analysis in previous sections which showed that the quality of information about the crime does not decline within a 12-month recall period. Persons who made more errors in recalling the date of the crime correctly (net telescoping) are slightly less apt to provide information to the interviewer even though the police record shows that they give the information to the police. This is an indication that persons who make errors on one variable--date of the incident--tend to forget information that they provided to the police. The analysis in previous sections suggested that errors in the date were not correlated with error (difference) on most other information items. The analysis here indicates that respondents tell the interviewer they do not know the answer rather than fabricating an answer during the survey situation.

The survey estimate of crime seriousness is significantly related to fewer don't know responses in the survey data, but the estimate of seriousness

TABLE 20.

CORRELATES OF POLICE HAVING INFORMATION WHERE SURVEY RESPONSE IS "DON'T KNOW", BY CRIME TYPE FOR MATCHED CASES

(Pearson Correlations)

	All Crimes (N=203)	Property Crimes (N=181)	Personal Crimes (N=16)
Time Lag from Incident to Interview	.02	.00	-.33
Net Telescoping	.16**	.14*	-.08
Age	.10	.13*	-.02
Seriousness (Survey)	-.26**	-.25**	-.26
Seriousness (Police)	-.10	-.06	.01
Race (0=black; 1=white)	.06	.08	//
Sex (0=female; 1=male)	-.09	.07	//
Education	.04	.03	.21
Positive Attitude Toward Police	.03	.03	-.06

* P < .05
** P < .01
// Too few respondents to analyze

based on police data is not. If both estimates of seriousness were correlated negatively, at a significant level, with the dependent variable, one could conclude that respondents remember details about serious incidents better than about trivial ones. The fact that only the survey estimate of seriousness is significantly related to the don't know responses, however, suggests that some persons did not tell the police about certain characteristics of the crime related to its seriousness (such as whether a weapon was present or not), or the police did not record it, or the individual later fabricated the information--for one reason or another--during the interview.

The only other significant correlation in the table indicates that older victims were more likely than younger ones to give the interviewer a "don't know" response even though the police had the information in their report.

The dependent variable in Table 21 is a score representing the frequency of a situation where the police records did not have the information whereas the survey data contained it. This situation could occur if respondents "made up" something for the interviewer, if the police failed to record the information, or if the respondent did not tell the police about it but later revealed the information to the interviewer.

There are two statistically significant correlations (at the .05 level) between the survey-based estimate of seriousness and absence of data on the police record. Again, the interpretation that seems most plausible is that the survey respondents gave certain information to the interviewer that increased the seriousness score, but they did not give this information to the police (or the police did not record it).

Data from both tables generally indicate that the age, race, sex, education level and attitudes of the victim are not associated with don't know responses in one source of data when the other contained the information.

TABLE 21.

CORRELATES OF SURVEY HAVING INFORMATION WHERE POLICE INFORMATION IS "DON'T KNOW" BY CRIME TYPE FOR MATCHED CASES

(Pearson Correlations)

	All Crimes (N=203)	Property Crimes (N=181)	Personal Crimes (N=16)
Time Lag from Incident to Interview	.03	.04	-.26
Net Telescoping	.05	.05	.40
Age	-.09	-.12	.50*
Seriousness (Survey)	.14**	.21*	.21
Seriousness (Police)	-.03	-.01	.14
Race (0=black; 1=white)	-.14	-.15	//
Sex (0=female; 1=male)	-.00	-.01	-.14
Education	-.03	-.03	-.36
Positive Attitude Toward Police	-.05	-.07	.32

* P < .05

** P < .01

// Only one black respondent

Indirect evidence about the effect of memory decay on the quality of victimization data can be obtained by using all of the victimizations from the Portland study (not just the matched incident set). As shown in Table 22, the time lag between when the interview occurred and when the victim said that the incident happened is not correlated with the frequency of don't know responses in the survey data. A positive correlation would mean that don't know responses occurred more frequently for events that happened in the more distant months of the recall period and would constitute indirect evidence that victims forget details of the event if it occurred in the more distant past. There is no evidence of this in the Portland data.

The more serious crimes tend to have fewer don't know responses in the survey data (Table 22), suggesting that respondents tend to forget details as a function of the triviality of the crime. It should be emphasized, however, that results from the matched incident set indicated that survey estimates of seriousness show this relationship but police estimates did not. The correlations in Table 22 also suggest that older persons were slightly more apt to provide don't know responses than were younger persons. The relationships are quite weak ($r=.09$) even though statistically significant. Men apparently provide fewer don't know responses than females, but again the strength of the relationship is weak ($r=.07$) and the effect would be relatively trivial in terms of introducing bias into the data.

The major conclusions from this section are:

1. Individuals who make errors recalling the date of the event are more likely (than others) to have provided information to the police and then failed to provide it to the interviewer.

2. The amount of time that elapses between the crime and the interview is not related to the frequency of don't know responses in the survey data.

TABLE 22.

CORRELATES OF DON'T KNOW RESPONSES BY CRIME TYPE

(Pearson Correlations)

	All Crimes (N=972)	Property Crimes (N=776)	Personal Crimes (N=134)
Time Lag between Incident and Survey Interview	.01	.01	-.06
Seriousness	-.19**	-.04	-.28**
Positive Attitude Toward Police	.04	.02	-.01
Age	.09*	.08*	.03
Race (0=black; 1=white)	-.02	-.05	.03
Sex (0=female; 1=male)	-.07*	-.06*	-.13
Education	.05*	.03	.06

*P < .05

**P < .001

3. Older victims are more likely than younger ones to have given don't know responses to the interviewer, but the relationship is not especially strong and should have only a minimal impact on the quality of the victimization data.

PART V.

AN ANALYSIS OF TELESCOPING IN VICTIMIZATION SURVEY DATA

One of the major difficulties in measuring the frequency of events with general population surveys is that survey respondents tend to provide inaccurate information on the actual date of the event. The problem is exacerbated if errors in recollection are not randomly distributed around the actual date. The authors of Surveying Crime note that there is a strongly held belief, but little empirical evidence, that telescoping of crime events is predominantly forward rather than backward (National Research Council, 1976). Forward telescoping occurs when the respondent recalls the event as having taken place more recently than it actually did; backward telescoping refers to misplacement of the event towards a more distant date than when it actually occurred. For surveys that use a specified recall period (such as 12 months) there are two additional characteristics of telescoping:

1. External telescoping occurs when the respondent either "pulls" an incident into the twelve month recall period that actually occurred prior to the most distant month in the recall period (external forward telescoping), or the respondent places an incident outside of the twelve month recall period which actually occurred within it (external backward telescoping). One method of solving the external forward telescoping problem is to use bounded interviews. The sample for bounded interviews is a panel of respondents who are interviewed at least twice. Crimes recalled in the second interim that had previously been reported in the first interview are eliminated. This procedure yields a more accurate estimate of the victimization rate for the time period that elapsed between the first and second interviews.

2. Internal telescoping occurs when the respondent either "pulls" the

incident closer to the interview date from a time period within the proper recall period (internal forward telescoping), or the respondent places the incident farther back into the recall period (internal backward telescoping).

Telescoping creates several problems for those who use unbounded surveys.

1. The amount of crime that actually occurred in the recall period will be overestimated if telescoping is predominantly forward rather than backward. This occurs because a greater number of events are pulled into the recall period than are telescoped backward out of it.

2. The pattern of crime revealed by the survey data will be inaccurate if certain types of crimes are telescoped to a greater extent than other types. For example, if serious crimes are more apt to be telescoped forward than trivial ones, then the survey data will overestimate crime seriousness. The actual estimate is further confounded by the fact that some respondents forget crimes or for other reasons fail to reveal them to the interviewer.

3. The distribution of crimes or crime seriousness among different subsets of the population will be reflected inaccurately in survey data if there are differences among the victims in relation to their telescoping patterns. For example, if older persons are more apt to forward telescope than younger persons, then the survey will overestimate the proportion of victimizations against the elderly, other things being equal.

The purpose of this section is to describe and analyze the telescoping patterns of Portland survey respondents whose crimes were found in Portland Police Department records.

Description of Telescoping

A summary of the telescoping pattern is presented in Table 23. (Tables in Appendix C show the detailed distribution of forward and backward tele-

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1 OF 2

coping, by type of crime, for respondents whose incidents were matched in the police files and for which dates were available.) Of the 203 incidents for which dates were available, 49 percent were placed in a month other than the one in which the incident occurred. By crime type, larcenies tended to be telescoped with greater frequency than other types of crime; 59 percent of the matched larcenies were telescoped, compared with one-half of the assaults, 45 percent of the burglaries and auto thefts, and 44 percent of the personal crimes.

The net telescoping coefficients (shown in row 2 of Table 23) summarize the strength and direction of telescoping. (These were computed by the formula $NT = \frac{P - Q}{P + Q}$ where P = number of events telescoped forward and Q = number of events telescoped backward.) A positive value indicates that, of those who telescoped, there was a net tendency for events to be telescoped forward; a negative value indicates a net tendency toward backward telescoping. Thus, for all crimes, there was a net tendency for 18 percent of the events to be forward telescoped; for larcenies this tendency increased tc 33 percent, while for burglaries net forward telescoping was only 7 percent.

The net months telescoped (row 3 of Table 23) was calculated by subtracting the average number of months that events were backward telescoped (weighted by the number of events) from the average (weighted) number of months that events were forward telescoped. Thus, for all crimes the net average telescoping was 2.24 months forward; for larcenies it was 4.41 months forward, while for burglaries the net average telescoping was only .49 months forward.

The percentage of crimes pulled into the recall period shows the proportion of external forward telescoping observed in these data. Since the interview period ran from May to August, 1974, these figures were calculated separately for respondents interviewed in each month. The monthly figures obtained were then weighted by the number of cases in each of the four interview months

TABLE 23.

PORTLAND DATA

	All Crimes	Larceny	Burglary	Auto Theft	Assault	Personal*
Percentage of Respondents Telescoping	49%	59%	45%	45%	50%	44%
Net Telescoping (+ = forward; - = backward)	.18	.33	.07	.11	.00	.14
Net Months Telescoped (+ = forward; - - backward)	2.24	4.41	.49	3.00	-.17	.57
Percentage of Crimes Pulled into Recall Period (Weighted average by interview month)	11%	22%	8%	10%	0%	0%
Number of Cases	203	61	100	20	12	16

*Assault, Rape, Robbery

and averaged to obtain these overall figures. Consistent with the patterns described above, larcenies were most subject to external forward telescoping (22 percent of matched larcenies were externally forward telescoped), while personal crimes were not subject at all to external forward telescoping.

For comparative purposes, Table 24 presents similar summary statistics calculated for the San Jose data. Although less telescoping is present in these data, a somewhat similar pattern emerges. For all crimes there is again a net tendency toward forward telescoping, and by crime type larcenies display the highest incidence of telescoping.

The fact that there was more forward telescoping in the Portland data is probably attributable to differences in the two studies. The San Jose incidents were drawn from police records and were all within the twelve month recall period. Thus, the study design used in San Jose precluded the possibility of external forward telescoping whereas the Portland study did not.

Boundary Effects

A question of major interest pertains to the magnitude of external forward telescoping and to what extent external forward telescoping is produced by the "cues" surrounding the survey effort to obtain only the events that occurred within the previous twelve months.

A common assumption is that persons may telescope events just across the boundary into the proper recall period producing an abnormally high number (or percentage) of events in the most distant month or two of the recall period. It should be noted that the Portland survey instrument differed slightly from the one used in the LEAA city surveys in that the respondent's recall of the date of the crime is more of a "free choice" question than used by LEAA. The LEAA instrument prefaces the question with a statement that the

TABLE 24.

SAN JOSE DATA

	All Crimes	Larceny	Burglary	Rape	Assault	Robbery
Percentage of Respondents Telescoping	41%	47%	38%	34%	34%	46%
Net Telescoping (+ = forward; - = backward)	.10	-.07	-.06	.20	.33	.38
Net Months Telescoped (+ = forward; - = backward)	.53	.25	.31	.90	.75	1.00
Number of Cases	265	59	85	29	35	57

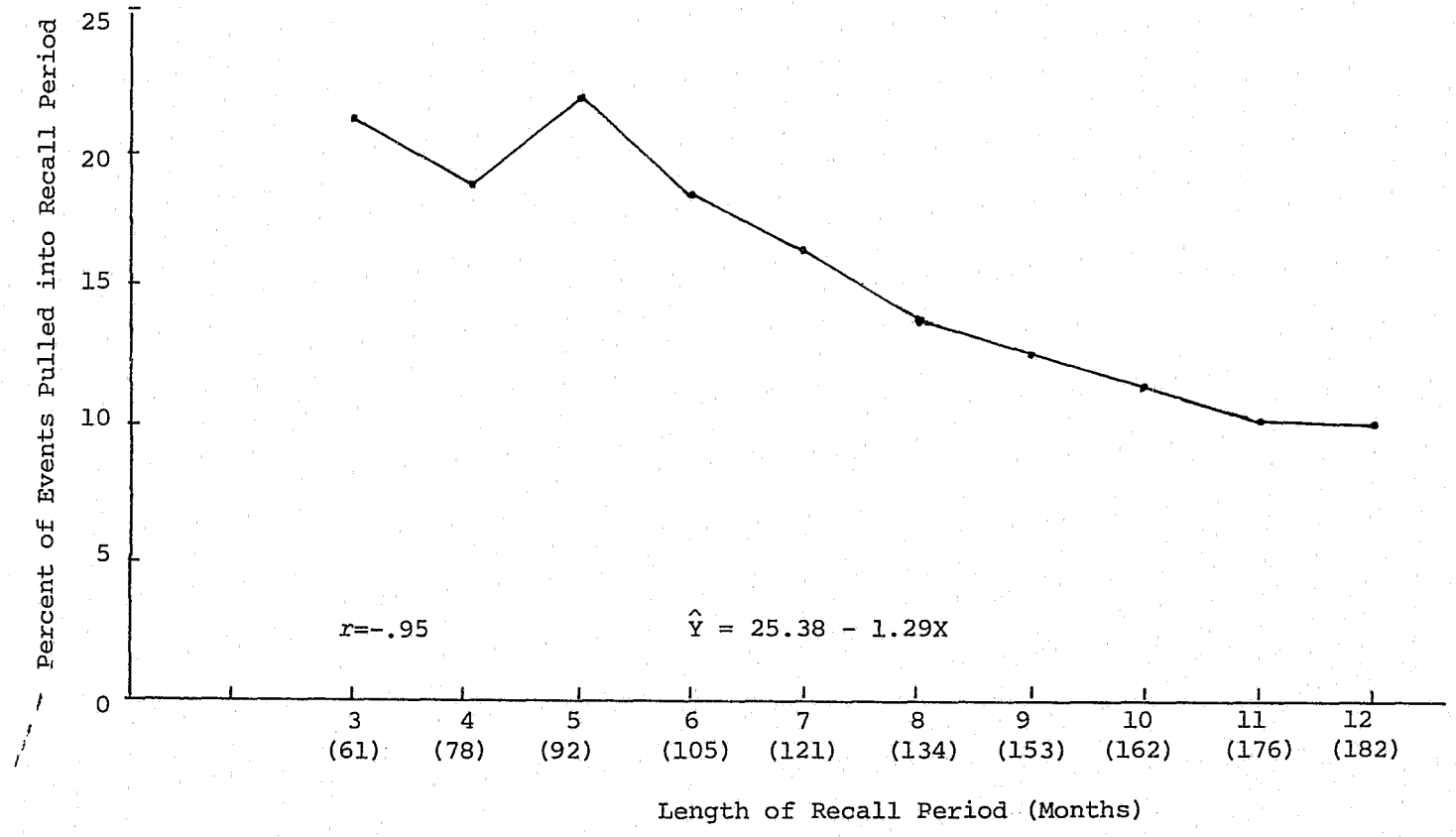
respondent has already said the incident occurred within the past twelve months and then asks for the exact date. The Portland survey instrument did not remind the respondent of what the proper recall period was except on the first parts of the screening questions. The date was not requested until much later in the survey and no cues about the "right" recall period were used. Thus, it may be the case that respondents to the LEAA surveys are more inclined to "intentionally" pull incidents just across the boundary into the recall period than were respondents to the Portland survey.

To test the boundary effect in the Portland area, the percentage of victimization incidents that were pulled into the recall period when the full twelve months were used was calculated along with the percentage that were pulled into the recall period when eleven months of it were used; ten months; and so on. In a sense, hypothetical recall periods of eleven months, ten months, nine months, and so on have been constructed. If the proportion of incidents telescoped into the recall period when the full twelve months is used is substantially greater than for eleven months, ten months, nine months, and so on, one could conclude that there is clearly a "boundary" effect. That is, respondents would seem to have intentionally pulled the incident just across the boundary in order for it to be "counted" in the data.

Figure 1 portrays the proportion of events externally telescoped into the twelve month recall period, the hypothetical eleven month recall period, the ten month period, and so on for the Portland data. The generally downward slope suggests that the proportion of events in the recall period that are telescoped into it decreased as the recall period becomes longer. The implication from the Portland data is that the use of unbounded short recall periods (less than six months) could result in the survey overestimating victimization by 20 to 25 percent.

FIGURE 1.

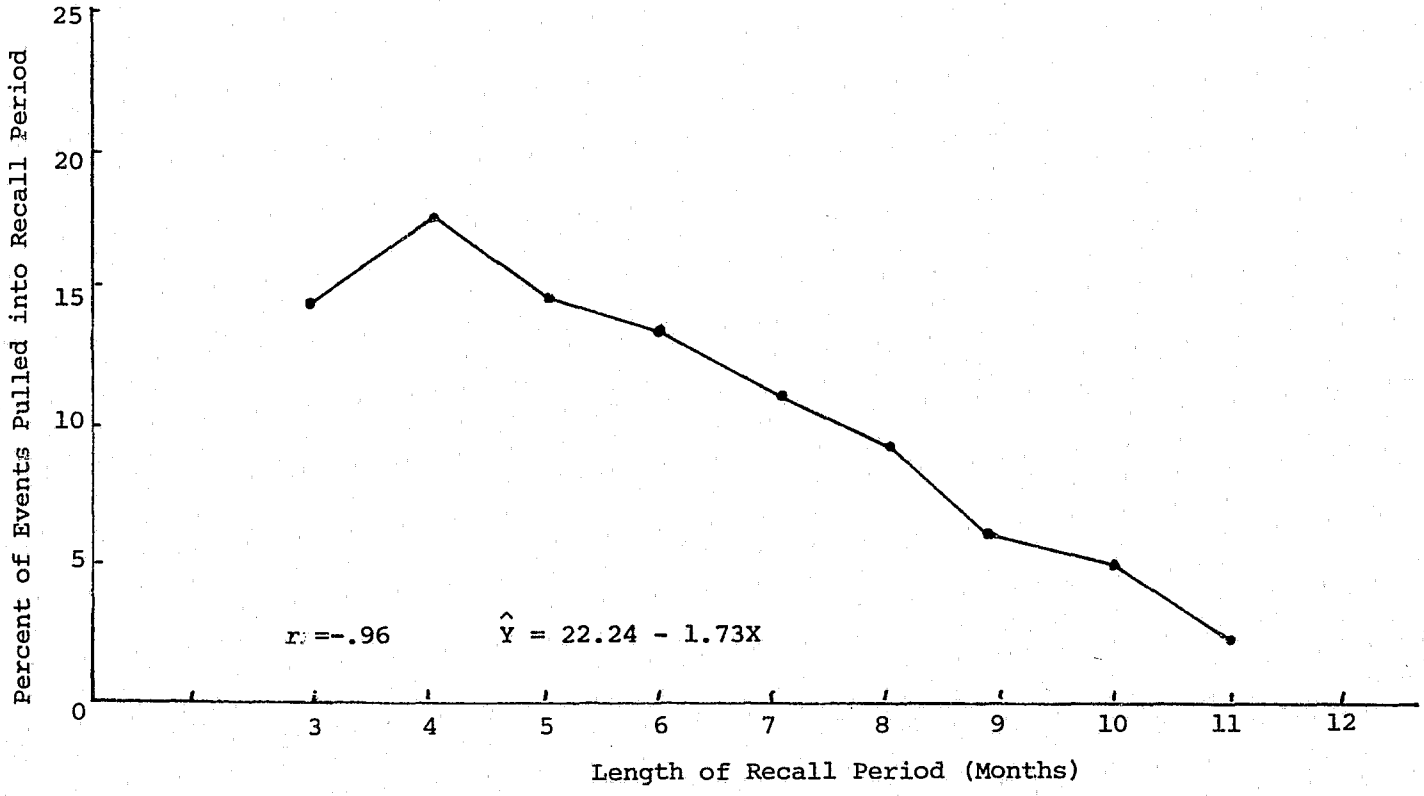
EXTERNAL FORWARD TELESCOPING: PORTLAND[1]



[1] The length of the hypothetical recall period is shown on the x-axis.

FIGURE 2.

EXTERNAL FORWARD TELESCOPING: SAN JOSE[1]



[1]The San Jose cases were drawn from police files and all were within the twelve month recall period. Thus, there was no external telescoping into the twelve month recall period.

There is no substantial evidence from Figure 1 that respondents intentionally pulled events just across the twelve month boundary. If this were the case, one would expect external telescoping to be more marked in the twelfth month than what is observed in the data.

The pattern of external forward telescoping in the San Jose study is shown in Figure 2. It is quite similar to the Portland pattern except that the amount is somewhat lower. This, again, is attributable to the fact that San Jose crime victims included in the survey had all been victimized within the twelve month recall period, thus excluding external forward telescoping of events that occurred prior to the twelve month time span. (The data used in Figures 1 and 2 are shown in Table 25.)

Correlates of Telescoping

The magnitude of forward telescoping (external and internal) in the Portland data appears to be a linear function of the amount of time that elapsed between the interview and the crime event. On the average, the expected amount of telescoping is: $\hat{Y} = -2.69 + .51X$ where Y is the number of months of forward telescoping and X is the number of months between the interview and the actual date of the crime event.

Several characteristics of victims and offenses were examined in order to determine whether certain types of victims are more likely to pull the date forward than are other types. As shown in Table 26, the time lag between the crime and the interview was the only variable with any substantial explanatory power for forward telescoping. The victim's age, race, and educational background were not related to the telescoping pattern. There is some indication that men forward telescoped to a lesser degree than women for property crimes. But the correlation (-.13) is not very high.

TABLE 25.

EXTERNAL TELESCOPING PATTERNS

Number of Months In Recall Period	PORTLAND				SAN JOSE			
	Number Reported In Month	Number Pulled In	Percent Pulled In	Correct Number	Number Reported In Month	Number Pulled In	Percent Pulled In	Correct Number
3	61	13	21	48	81	11	14	70
4	78	15	19	63	108	18	17	90
5	92	20	22	72	128	18	14	110
6	105	19	18	86	155	20	13	135
7	121	19	16	102	177	20	11	157
8	134	19	14	115	199	17	9	182
9	153	19	13	134	214	12	6	202
10	162	19	12	143	232	11	5	221
11	176	19	11	157	243	5	2	238
12	182	21	11	161				

$\hat{Y} = 25.38 - 1.29X$ $r = -.95$ $\hat{Y} = 22.24 - 1.73X$ $r = -.96$

(Where Y is percent "pulled" in X is number of months in the recall period)

TABLE 26.

CORRELATES OF FORWARD AND BACKWARD TELESCOPING

BY CRIME TYPE FOR MATCHED CASES[1]

(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

[1]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.

The implication of the lack of relationship between forward telescoping and victim characteristics is that survey victimization data apparently should provide an accurate portrayal of the distribution of crime among population subgroups. If some types of victims (the elderly, for example) forward telescope more than others, then the survey data would contain too many incidents for the group that forward telescopes.

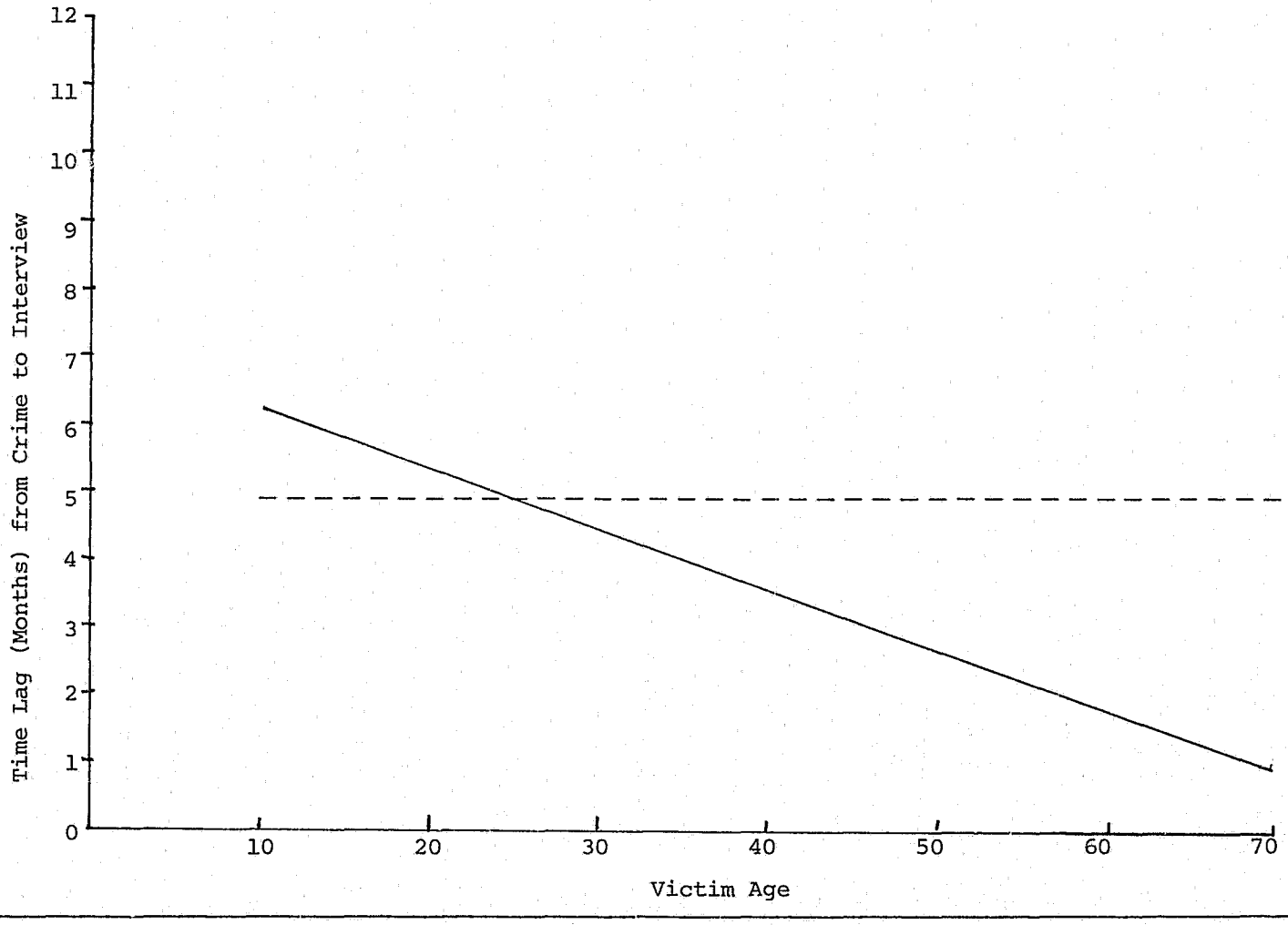
Correlates of Forgetting

It is possible, of course, that some types of victims "forget" crimes more quickly than others. A forwards records check cannot be used to measure forgetting, but indirect evidence can be used to examine the question of whether forgetting varies with characteristics of the victim or the offense.

The major assumption underlying the approach is that all victims are equally likely to have been victimized during each month of the recall period. If so, then there should be no relationship between victim characteristics and the number of months that the victim recalls having elapsed since the crime occurred. This type of analysis is illustrated in Figure 3 using victim age as the independent variable. If victims--regardless of age--are equally likely to be victimized in each month of the recall period and if their age is unrelated to forgetting and to forward telescoping, then the pattern shown by the dotted line in Figure 3 should be found. However, if older victims are victimized throughout the recall period in the same way as younger ones, but if they are more apt to forget the more distant victimizations or more apt to forward telescope, then a pattern similar to that shown by the solid line should be found. This pattern would show that older victims appear to have experienced most of their victimizations within the most recent month or two, but the pattern also could mean that older persons forget the more distant events to a greater extent than younger, or that they forward telescope more.

FIGURE 3.

INTERPRETING POTENTIAL OUTCOMES OF INDIRECT TEST FOR FORGETTING AND TELESCOPING[1]



[1]The solid line would indicate that older victims recalled that their victimizations occurred more recently than did younger victims. The dotted line suggests that victim age is not related to when the person recalls the crime having occurred.

The data for this analysis consist of all victimizations reported to interviewers in the 1974 survey rather than only those that were found in the forward records check.

As shown in Table 27, most characteristics of victims were not related to the victim's recollection of how recently the crime occurred. There is a weak but statistically significant correlation suggesting that persons with more positive attitudes toward the police were victimized more recently than persons with negative attitudes. If the assumption stated above is correct, then this relationship suggests that persons with positive attitudes were more apt to forget crimes that occurred in the most distant months of the recall period or more apt to forward telescope. Conversely, persons with negative attitudes were somewhat more likely to remember incidents and/or less apt to forward telescope.

The seriousness of the crime was not related to the time lag. This is somewhat surprising since one would anticipate that the more trivial crimes occurring in the distant months would be forgotten at a greater rate than the serious ones. This would have resulted in the less serious crimes being predominantly bunched into the more recent months of the recall period.

There is some evidence that persons with more years of education tended to recall that personal crimes committed against them were more recent.

In general, however, the evidence is that most characteristics of the victim and the seriousness of the offense did not influence the distribution of crimes within the recall period. This constitutes tentative and indirect evidence that forgetting and forward telescoping patterns do not differ systematically with the characteristics of the victims or the seriousness of the offense.

Another potential confounding factor in survey data analysis involves

correlated errors. If persons who forward telescope also overestimate (or underestimate) crime seriousness, overreport (or underreport) victim activity, overestimate (or underestimate) police response time and so on, then unbounded survey data will contain aggregate-level error about characteristics of the events. To test for this, the amount of forward telescoping was correlated with the direction of differences between police and survey details about the crime. (The dependent variable is the same difference score used in previous sections. It is calculated by subtracting the police score on the variable from the survey score.)

The results (Table 28) indicate that forward telescopers made more errors in recall of victim activities and that most of these were overreporting the extent of victim self-protection. Otherwise, there are no statistically significant correlations at the .05 level. Even though the correlations are not statistically significant, the relationships between forward telescoping and all four of the characteristics of suspects should be viewed with some concern. The analysis suggests the possibility that incidents which were forward telescoped were characterized by overreporting of the suspect as black (in the survey data), overreporting that the suspect was known to the victim, an underestimate of the number of offenders involved and an overestimate of their ages.

The direction of telescoping is more of a problem in unbounded surveys than the amount of telescoping because the former will result in too many (or too few) events being recalled within the proper time frame whereas the amount of telescoping simply introduces randomly distributed error in the data. It is possible, however, that certain characteristics of victims or offenses are related to the absolute amount of telescoping that occurs, and an analysis of this type is reported in Table 29.

TABLE 27.

CORRELATES OF TIME LAG AS RECALLED BY VICTIM

FOR REPORTED AND UNREPORTED EVENTS[1]

(Pearson Correlations)

Characteristic	All Crimes (N=972)	Property Crimes (N=776)	Personal Crimes (N=134)
Positive attitude toward police	-.07**	-.08**	+.02
Age	-.04	-.03	-.01
Race (0=black,; 1=white)	+.02	+.04	-.09
Sex (0=female; 1=male)	+.02	-.00	+.12
Education	-.04	-.02	-.14*
Seriousness	-.00	-.03	+.08

*P < .05

**P < .01

[1]Positive correlations mean that higher scores on the characteristic are related to longer time lags; negative correlations mean that lower scores on the characteristic are related to longer time lags. For example, for all crimes a negative attitude toward the police is weakly related to longer time lags between the crime date and the interview date.

TABLE 28.

RELATIONSHIP BETWEEN FORWARD TELESCOPING AND

SURVEY AND POLICE INFORMATION DIFFERENCES

Information	FORWARD TELESCOPING: Higher Estimate in Survey r	FORWARD TELESCOPING: Absolute Amount of Difference r	N
Seriousness			
Seriousness Scale	.00	.06	212
Dollar Loss	-.07	-.09	212
Characteristics of Suspects			
Race (white)	-.16	-.05	36
Stranger	-.20	.12	46
Number of offenders	-.17	.10	43
Age of offenders	.16	-.08	35
Activities of Victims and Police			
Victim Self-protection	.20*	.25*	95
Witness present	-.01	.00	136
Police response time	-.12	-.08	152
Police activities	-.02	-.11	175

*$p<.05$

TABLE 29.

CORRELATES OF ERROR IN RECALL OF INCIDENT DATE (TELESCOPING)

FOR MATCHED CASES[1]

(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

[1]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.

Although there are several statistically significant correlations, the only one with any substantial explanatory power is the time lag between when the incident occurred and when the interview took place. Incidents that occurred in the more distant past were more subject to errors concerning when they occurred. In addition, the data indicate that older persons made slightly fewer errors in the date of the event, men made somewhat fewer errors than women, and there were fewer errors for serious crimes than for trivial ones.

These correlations mean that some victim characteristics were related to the absolute amount of error, but the data in previous tables show that victim characteristics are not related to the direction of error. The major implication of this finding for those who use victimization data is that the reliability of the date of the incident differs across some characteristics of respondents and differs somewhat in terms of crime seriousness.

Summary

The analysis provides support for the contention that the amount of crime estimated in victimization surveys (unbounded) will be overestimated due to forward telescoping of events. The data indicate that the magnitude of the problem varies with the type of crime and may be especially serious for larcenies.

There is no evidence that the victimization survey data used in this analysis were biased due to different types of telescoping by respondents with different characteristics. If this result is true of survey data in general, then the surveys should provide reliable estimates of the distribution of crimes among population subgroups.

Incidents that were forward telescoped contained overreports (compared

with police data) of victim activities. With this one exception, the survey information was generally not biased by correlated error between forward telescoping and other characteristics of the crime or suspect.

PART VI.

INCIDENTS THAT COULD NOT BE FOUND IN POLICE RECORDS

The Portland forward records check was not designed to provide information on why certain crimes could not be found in police data. Nevertheless, there is considerable amount of curiousity concerning what happened to the 47 percent of presumably reported incidents that were not found in police data. The analysis in this section is presented in an effort to provide limited (and very speculative) information about the "missing" cases. There are four possible explanations for the missing incidents:

1. The incident was reported and was recorded by the police, but the search procedures failed to locate it. The search procedure could have failed to find the incident because either the survey or police address was wrong (by at least five square blocks, since the search covered an area that large), or the police down-classified the event into a civil offense, or the incident occurred prior to January, 1972, which was the earliest date used in the search. A name search was initiated for 103 incidents in which a last name had been given, but this resulted in finding only an additional twelve incidents. Thus, if a name search had been used for the remaining 296 incidents, an additional 11.6 percent of them might have been found for a total "matched" set of 246 instead of 212. It is not likely that any more incidents would have been found if the search had extended back through 1971 since hardly anyone telescoped incidents from that far in the past.

2. The incident was reported to someone in authority or to some other person and the respondent assumed it became known to the police when, in fact, it did not. The data indicate that the probability of finding the incident

was slightly greater if it was reported to the police by the victim or by a household member rather than reported by a friend. There were sixteen unmatched incidents which the respondent said became known to the police due to a friend having reported it. In addition, there were three incidents presumably reported by a stranger that were not found. It is possible that these nineteen cases were not reported at all and should not have been included in the original total of 399 "reported" incidents with precise addresses.

3. The incident was reported but police discretion resulted in the incident being considered unfounded, or an "exceptional clearance" was issued even before an original police report was filled out, or the event was not recorded for other reasons. It is impossible to estimate the degree of police discretion that was used in eliminating reports of crimes prior to the filing of an original report. It is the case, however, that Portland police occasionally use a single crime report to cover more than one incident and even more than one victim. There were six survey incidents which were described in the narrative section of a police report on a different crime against a different victim.

4. The incident was not reported to the police even though the respondent told the interviewer that it was. Again, there is no way to determine the number of "missing" cases that respondents said became known to the police when, in fact, the police were totally unaware of them.

Of the original 476 victimization incidents that respondents said were reported to the police, there were 212 definite matches. This leaves a total of 264 "missing" cases. Of these, 77 had addresses too vague to permit a search, six were found in the narrative section of a report concerning a different crime and a different victim, 34 additional cases might have been found if a name search had been used for all the incidents, and nineteen may

have been "honest" errors in that the respondent thought the incident was reported by a friend or stranger who witnessed the crime. Applying these estimates to the data, there are still 128 incidents which are not accounted for at all (see Table 30).

If all of the "missing" 128 incidents were actually reported to the police, then the police discretion factor seemingly eliminated 32 percent of the victim crime reports (or down-classified them to civil offenses). If none of the 128 incidents was actually reported to the police, then it appears as if approximately 32 percent of the survey respondents said that they reported a crime when, in fact, they did not. It is more likely that both sources of error exist and that the 32 percent "missing" cases should be divided in some way between respondent misstatements about reporting and police discretion in terms of informally "unfounding" or "clearing" an incident even before it is recorded.

As noted previously, the Portland forward records check was not designed to permit a comprehensive study of why victimizations cannot be found in police data and no additional conclusions can be drawn. Several different types of analyses were conducted, however, in an effort to identify characteristics of the crimes or victims that were definitely "matched" and those that were definitely "not found." Data from those analyses are presented in Table 31.

Differences approaching statistical significance were found for comparisons involving who reported the incident ($p=.12$), and for comparisons based on the sex of the victim ($p=.07$). There were no significant differences in the probability of finding a single incident compared to a series of events, in the probability of finding an incident in which the offender was known to the victim compared to an incident in which the offender was a stranger, and

TABLE 30.

ACCOUNTING FOR THE "MISSING" CASES

	N	Percent	Percent of those for which search was undertaken N=399
Original number respondents said were reported	476	--	--
a) No search due to vague address of crime location	77	16%	--
b) Event found but no original police report	6	1%	2%
c) Estimated number of additional cases that would have been found with full name search	34	7%	9%
d) Estimated number of "honest" errors: respondent thought event was reported by friend or stranger	19	4%	5%
e) Actual number found	212	45%	53%
Actual number not found, including b, c, and d	187	39%	47%
Estimated number not found, excluding a, b, c, and d	128	27%	32%
Estimated number accounted for (sum of b, c, d, and e)	271	57%	68%

no significant difference was observed for black as compared to white victims.

For some portions of the analysis, a comparison was made among reported matched incidents, "reported" but not matched, and unreported incidents. These results are shown in Table 32. Significant differences exist in terms of the time lag between the incident and the interview with the unreported incidents appearing to have occurred more recently. This probably is due to a more rapid forgetting of the unreported incidents which produces fewer such crimes in the most distant months of the recall period. There also are statistically significant differences in seriousness of the crime (with the reported matched being the most serious) and in the age of the victim. In most of the comparisons, the "unreported" but not matched group has characteristics that place it in between the reported and the unreported incidents.

The data in Table 33 show the proportion of all crime incidents from the Portland survey that are in each of the three categories. It is interesting to note that only 22 percent of all the survey-generated incidents were found in police files. The major factor in the incident not being in the files, however, is that the victim did not report the crime. There were 712 incidents which were never officially recorded by the police. Of these, 78 percent were due to victim nonreporting whereas only 22 percent can be attributed to presumably "reported" incidents that could not be found in the police records.

Two concluding statements can be made based upon the analysis:

1. Through a series of adjustments to the data, the best estimate is that approximately 32 percent of the incidents which respondents said were reported to the police actually were not reported or were reported but the police did not record the incident as a crime. If this estimate is correct and if the 32 percent is divided evenly between the two sources of error, then the implication is that about 16 percent of the victims say the crime was

TABLE 31.

CHARACTERISTICS OF "DEFINITE MATCHES" AND "DEFINITE NON-MATCHES"

	Found		Not Found		Total	
	N	%	N	%	N	%
Who told the police?						
Victim	68	54%	59	46%	127	100%
Household Member	124	57%	93	43%	217	100%
Friend	11	41%	16	59%	27	100%
Stranger	0	--	3	--	3	100%
Police	1	--	1	--	2	100%
TOTAL	204		172		376	
Collapsed Chi Square*=4.36 d.f.=2 p=.12						
Series Versus Single Incident:						
Single	183	55%	152	45%	335	100%
Series	29	50%	29	50%	58	100%
TOTAL	212		181		393	
Chi Square=0.260 d.f.=1 n.s.						
Victim-Offender Relationship:						
Stranger	47	48%	51	52%	98	100%
Some Known	14	45%	17	55%	31	100%
All Known	10	35%	19	65%	29	100%
TOTAL	71		87		158	
Chi Square=1.63 d.f.=2 n.s.						
Sex of Victim:						
Male	43	57%	33	43%	76	100%
Female	30	40%	45	60%	75	100%
TOTAL	73		78		151	
Chi Square=3.52 d.f.=1 p=.07						
Race of Victim:						
White	28	39%	43	61%	71	100%
Black	31	44%	40	56%	71	100%
TOTAL	59		83		142	
Chi Square=.12 d.f.=1 n.s.						

*Friend, stranger, and police were combined into a single category when calculating the chi square test.

TABLE 32.

MEAN SCORES ON PREDICTOR VARIABLES

FOR MATCHED REPORTERS, UNMATCHED REPORTERS, AND NON-REPORTERS

	Reported Matched	Reported Not Matched	Not Reported	
Months between incident and interview	6.46	6.40	5.53	$p<.01$
Seriousness[1]	2.90	2.25	1.73	$p<.01$
Attitudes toward police (1=positive; 4=negative)	2.12	2.27	2.31	n.s.
Age	42.92	37.29	36.46	$p<.01$
Education (in years)	12.78	12.86	12.83	n.s.

[1]The seriousness scale used in the analysis includes injury, weapon, loss from crime and other similar indicators (see Appendix B).

TABLE 33.

CRIME TYPE FOR REPORTED MATCHED, REPORTED UNMATCHED, AND UNREPORTED EVENTS

	Reported Match		Reported No Match		Not Reported		Total	
	N	%	N	%	N	%	N	%
Rape	1	--	2	--	2	--	5	100%
Robbery	3	9%	13	37%	19	54%	35	100%
Assault	12	14%	23	27%	50	59%	85	100%
Burglary	103	49%	37	14%	118	46%	258	100%
Larceny	67	14%	70	15%	340	71%	477	100%
Auto Theft	20	34%	14	24%	24	41%	58	100%
Number of Cases	206	22%	159	17%	553	60%	918	

reported when it was not. And, the implication would be that about 16 percent of the incidents reported to the police are not recorded due to police discretion that is exercised before an original crime report is filled out. These figures are highly speculative, of course, and if there is sufficient interest in the "missing cases" then a study should be designed explicitly for the purpose of trying to answer it.

2. If one examines the total number of victimization incidents recalled by Portland city residents in the 1974 survey (reported and unreported), then the problem of "non-reporting" by victims is considerably more serious than the problem of "not finding" incidents in the police files. Of the survey-generated incidents that are not in the police files, 78 percent are "missing" because the victim did not report the crime and 22 percent are "missing" because either the police did not record it or the victim said it was reported but it was not.

3. Comparisons of the characteristics of the "missing cases" with "true" reported and presumably "true" unreported incidents reveal that the missing ones are no more similar to reported than to unreported crimes but instead generally represent a mixture of the "true" reported and unreported. If the missing cases were clearly more similar to the one than to the other, then one might speculate that most of the missing incidents actually belong to which ever group they resemble. Since the analysis suggests they are "in between" the other two categories, then the only reasonable conclusion is that some of the missing cases were reported (but not found) and others were not reported.

PART VII.

SUMMARY, DISCUSSION AND RECOMMENDATIONS

Although there are many issues involved in assessing the value of victimization surveying, the one that provided the rationale for the Portland Forward Records check is whether or not the survey data are sufficiently reliable and valid that they can be used when official crime statistics are inappropriate. The study cannot provide definitive answers to the question because of the small sample size, the fact that all the analyses are based on data from one city and may not be generalizable to other places, and because it represents the first forward records check of crime victims and one of only a few studies which have compared survey information with official data about the same crime events. A few propositions tested in this study have been examined in previous research. The results from the Portland study are quite consistent with previous findings on these and are summarized below.

1. The information obtained through surveying is sufficiently similar to that given to the police at the time of the incident that most crimes are classified in the same way by the two sources of data.

Both the San Jose records check and the Portland study found that 97 percent of the burglaries were classified the same from survey and police data; both found that 82 percent of the larcenies were classified the same way; and there were only slight differences concerning classification of personal crimes. The Portland study indicated that information was sufficient to produce the same classification in 74 percent of the personal crime incidents, whereas the San Jose study (which had a larger sample of personal crimes) obtained the same classification in 85 percent of the incidents. The implication is that even though survey data might be criticized for a variety of

reasons, there is accumulating evidence that criticisms directed toward the accuracy of information needed to classify crimes are not warranted.

2. Survey data from Portland and from the San Jose study contained higher estimates of the dollar loss from the crime.

The range of differences found in the San Jose data was from a 24 percent higher estimate to a 33 percent higher estimate, depending on the type of crime. The range of differences found in the Portland study was from a 24 percent higher estimate to a 48 percent higher estimate. Neither study was able to identify reasons for the higher estimates in survey data compared with police estimates.

Several propositions were tested with the Portland data concerning factors that might have produced higher estimates in the survey. One proposition was based on the possibility that victims distort information about loss as a function of the amount of time that elapsed between the crime and the interview. Higher survey estimates could be produced if victims whose crime experience was further in the past systematically overestimated the amount of loss. However, there was no evidence found in the analysis that this was a contributing factor.

There was no evidence from the Portland analysis which would indicate that the overestimates are contributed disproportionately by certain types of victims. No relationships were found between victim age, race, sex, educational background and the amount of over- or underestimation.

This problem with the data is most acute for researchers who wish to use survey information to estimate the total amount of monetary loss due to crime or the average loss per victim. Data of this type are of value in estimating the expected cost of crime compensation programs, the savings that could result from certain types of crime prevention programs, and cost-effectiveness evalua-

tions. The survey data include estimates for unreported as well as reported crime and, for this reason, might be considered superior to official data even if the error is contained mainly in the survey information. At this time, however, there is no evidence of whether the error was in the survey data or the police data (or both).

3. Telescoping crimes into the reference period that actually occurred prior to the most distant month included in the time span appears to be a major problem in unbounded interviews.

The Portland data showed that larcenies were more likely to be telescoped than other types of incidents. An average larceny was telescoped forward by 4.4 months in the Portland study and 22 percent of all the larcenies were incorrectly placed within the recall period when they actually occurred prior to it. These results are similar to previous studies in that incidents tend to be forward telescoped to a greater extent than they are backward telescoped. There are, however, several differences between the Portland findings and those from San Jose concerning which incidents are most likely to be forward telescoped.

Nevertheless, the study confirms previous research which has shown that telescoping produces error in the surveys in relation to the victimization rate, the comparative frequency of different types of crimes, and the month-by-month trend within the recall period.

The analysis of why telescoping occurs showed that the major explanatory factor is the amount of time that elapsed between the incident and the interview. Incidents that occurred further in the past were telescoped to a greater extent than those which occurred recently. There was no indication from the analysis that certain types of victims were more inclined to telescope incidents than were other types. The only characteristic of the crimes that was

examined in relation to telescoping was the seriousness scale. Although the more serious incidents were telescoped less in the 212 cases examined, the strength of the relationship (r=.11) was not great enough to be statistically significant at the .05 level.

In addition to these, several other propositions were tested in the forward records check. The data in Table 34 are a comprehensive summary of the results from analyses of informational differences concerning factual aspects of the crimes. Data in the table include a summary of the characteristic differences (if any) between survey and police information, as well as a summary of whether evidence was found which would indicate the presence of systematic bias in the survey data.

Four types of biases were examined.

1. Memory Distortion refers to whether victims tend to distort information in a systematic way during the time lag that elapsed between the crime and the interview. Evidence of distortion exists if a statistically significant correlation was observed (.05 level) between the number of months that elapsed and the direction of differences between police and survey information. For example, the differences between police and survey seriousness estimates might change as a function of the time that has elapsed in such a way that more recent incidents contain greater survey overestimates than did the more distant estimates. If so, then there is evidence that victims distort the information as time passes.

2. Memory Loss refers to whether victims tend to make more errors in recalling the relevant details about the incident as a function of (a) victim age (b) length of time since the crime and (c) errors in recalling the date. Significant correlations between the absolute amount of difference and any of these three factors is considered evidence of memory loss and, therefore,

TABLE 34.

SUMMARY OF FINDINGS

Type of Information	Amount of Agreement	Predominant Characteristics of Differences	Evidence of Survey Error or Bias Due To . . .			
			Memory Distortion	Memory Loss	Differential Victim Recall	Differential Forward Telescoping
1. Classification	91%	None	--	--	--	--
2. Details of what happened (9 information elements)	87 to 99%	None	--	--	--	--
3. Seriousness of offense (Sellin-Wolfgang scale)	r =.63 Survey: $\bar{x}$=2.9 s.e.=.13 Police: $\bar{x}$=2.5 s.e.=.10	Survey Estimates Higher	No	No	No	No
4. Dollar Loss	r=.82 Survey: $\bar{x}$=$412 s.e.=83 Police: x=$319 s.e.=89	Survey Estimates Higher	No	No	No	No
5. Characteristics of Suspects:						
a) Race: white or black	34%	None	No	No	No	No
b) Known or stranger	41%	None	No	No	No	No
c) Age of suspect	Survey: $\bar{x}$=18.2 Police: $\bar{x}$=18.7	None	No	No	No	No

[1]Evidence that memory loss occurred is based on statistically significant correlations (.05 level) between the absolute amount of difference and (a) a time lag between crime and interview, (b) error in recalling the date, (c) age of victim. Evidence of memory distortion refers to systematic over- or underestimation as a function of the time lag between event and interview. The summary analysis concerning the other relationships in the table are based on correlations with the direction of differences, not the absolute amount.

TABLE 34. (Page two) SUMMARY OF FINDINGS

Type of Information	Amount of Agreement	Predominant Characteristics of Differences	Evidence of Survey Error or Bias Due To . . . Memory Distortion	Memory Loss	Differential Victim Recall	Differential Forward Telescoping
5. Characteristics of Suspects (cont'd)						
d) No. of Suspects	Survey: $\bar{x}$=1.8 s.e.=.13 Police: $\bar{x}$=1.6 s.e.=.11	Survey Estimates Higher	No	No	No	No
e) Sex of suspect Male or female	100%	None	--	--	--	No
6. Self-Protective Activities of Victim	84%	None	No	No	No	Yes
7. Presence of witnesses	83%	None	Yes	No	Yes	No
8. Police Response Time	48% (within 15 minutes)	Survey Estimates Longer	No	No	No	No
9. No. of Police Activities	Survey: $\bar{x}$=1.08 s.e.=.04 Police: $\bar{x}$=1.46 s.e.=.05	Survey Estimates Lower				
10. "Don't know" Responses in interview when police report contained data	N/A	N/A	N/A	No	No	No
11. "Don't Know" Responses, All Interviews	N/A	N/A	N/A	No	Yes	N/A
12. Month of Incident	51%	Survey Estimate More Recent	Yes	Yes	No	N/A
13. Forgetting Incident (indirect tests)	N/A	N/A	N/A	N/A	No	N/A

evidence that the differences between police and survey data are attributable, at least partially, to the survey procedures.

3. Differential Victim Recall refers to whether certain types of victims contribute disproportionately to the nature of the differences observed between police and survey information. Evidence of differential victim recall is based on whether there are statistically significant correlations (.05 level) between victim characteristics (race, age, sex, educational level) and the direction of differences observed between police and survey information.

4. Differential Forward Telescoping refers to whether certain types of incidents are forward telescoped more than other types and to whether certain types of victims forward telescope more than others. In unbounded surveys, the survey data will overrepresent any type of incident that is forward telescoped to a greater extent than other incidents and, in a similar way, will overrepresent crimes against victims who forward telescope.

The major findings are summarized below:

1. The reliability and validity of survey data depend upon the type of information being considered. As shown in Table 34, the types of information that appear to be most accurate and to have the greatest validity are:

1. The details of what happened during the crime, including whether the victim was attacked, whether the victim was threatened, whether the offender had a weapon, whether there was physical injury, whether medical attention was needed, whether property was taken or damaged, whether offender had a right to be there, whether offender actually got in, and whether there was evidence of forcible entry.
2. The classification of the offense.
3. Age and sex of suspects.
4. Number of suspects.
5. Whether the victim undertook self protective actions.
6. Whether there were witnesses present.

Differences between police and survey information were great enough to be of concern for the following:

1. Seriousness of the offense (Sellin-Wolfgang scale).
2. Dollar loss from the crime.
3. Race of suspects.
4. Whether the suspect was known to the victim or not.
5. Police response time.
6. Number of activities undertaken by the police at the scene.
7. Month during which the crime occurred.

The seriousness of the offense was measured with the Sellin-Wolfgang scale and included several indicators of seriousness. The higher estimates in the survey data were produced mainly by higher dollar loss from the crime and by victim statements concerning whether a weapon was present or not. Efforts were made to determine why the survey data contained higher estimates, but the results were basically identical to those reported above concerning dollar loss: There was no evidence that memory loss or memory distortion produced the differences and no evidence that certain types of victims contributed disproportionately to the higher survey estimates.

It is not possible to develop recommendations concerning how the accuracy of dollar loss and seriousness data could be improved, since we do not have any evidence about the source of the problem. Police record-keeping could produce lower estimates; survey methods in which the value attached to the items stolen are accepted without questioning could produce higher estimates in the survey; or respondent errors could produce the differences. It is important, however, that some additional investigation be undertaken to identify the reasons for the differences and develop better questioning procedures (perhaps for both the interviewers and the police) in order to insure that

measures of crime seriousness and dollar loss are more accurate than indicated in the Portland data.

Police and survey data differed on a case-by-case basis concerning the race of the suspected offender and whether the offender was known to the victim. However, there were no systematic differences in the sense that survey data did not indicate more black (or white) suspects than police data and did not suggest that there were more (or fewer) strangers than the police data. It might be noted that police and survey data were more similar in respect to age, sex, and number of suspects than they were for race or relationship of offender to victim. This result, if replicated in other studies, would suggest that the latter facts about the incident are more sensitive to the respondent. Improvement in the reliability of the data might be achieved through better questioning procedures (by interviewers and/or by the police). As with most of the other data which differed between police and survey records, no evidence could be found concerning why the differences exist. Memory loss, memory distortion, and selective misperception by certain types of victims were tested as possible explanations, but none of these had statistically significant correlations with the amount or type of error.

The implications of the findings are that the reliability of racial data about offenders may be lower than some of the other information making it more difficult to find statistically significant relationships between offender's race and other characteristics of the incident or characteristics of the victim. The same is true for the stranger, non-stranger variable. On the other hand, studies which use these variables to examine relationships between type of victim and type of offender, for example, should not contain systematic biases that could confound the conclusions because error in offender characteristic data appear to be unrelated to victim characteristics and

unrelated to characteristics of the offense.

Survey data overestimated police response time, in comparison with police records, and underestimated the number of activities undertaken by the police at the scene. The most plausible explanation for survey estimates of police response time being higher than the police estimates is that persons, during times of crisis, tend to believe that more time has elapsed than actually is the case. The possibility that police underestimate the time cannot be entirely eliminated, but in Portland this possibility is very remote. The victim's call to the police is recorded, the dispatcher's call to the officer is recorded, and the officer's call that he has arrived on the scene is recorded. The time estimates are kept in seconds, not just in minutes, and even though the persons who copy from these logs onto the police form could alter the response time data, it does not seem likely that they would do so, since positive evidence of response time is available.

A plausible explanation for why the survey data underestimated the activities by the police at the scene is that this is an open-ended survey question and not one designed to job the memory of respondents in the survey. Questions which specifically ask the victim to recall whether the police investigated, arrested someone, or took fingerprints almost certainly would improve the survey data.

2. For most of the types of information elements examined in this study, there is no evidence that the accuracy or completeness of the information declines as a function of the time lag between when the crime occurred and when the interview was conducted. There were, however, two exceptions. First, the accuracy of respondent's recall of the date declined as the time lag increased; and second, there was a tendency for victims to forget that witnesses were present for events that occurred further in the past.

The implication of this finding is that a 12-month retrospective recall period may be just as good as shorter ones if the data are to be used for certain types of purposes. Previous studies have demonstrated without exception that respondents are more apt to forget crimes that occurred further in the past. The evidence in this study suggests that if they remember the incident at all, they tend to remember (accurately) most of the details about what happened. Thus, studies which use victimization surveying for the purpose of analyzing relationships within the data, rather than making population-level estimates of victimization rates, might be able to use longer recall periods--perhaps recall periods even longer than twelve months. The critical question, and one that has not been examined, is whether incidents that are forgotten differ from those recalled in terms of the patterns and relationships between victims and offenders, offenders and certain characteristics of the crime, and so on. Therefore, before definitive conclusions are drawn concerning the optimal recall period for surveys focussing on patterns and relationships, the results in this research should be replicated and similar types of analyses should be conducted using reverse record check procedures so that forgotten incidents can be analyzed.

3. Preliminary evidence from the study indicates that survey data should provide accurate conclusions for studies of:

(a) the distribution of crimes among population subgroups,

(b) the distribution of crime seriousness among population subgroups,

(c) the relationships between victims' characteristics and certain characteristics of the offense,

(d) the relationship between victim characteristics (age, race, education level, sex) and the activities of the victim, police, and witnesses at the time the crime occurred.

Characteristics of victims were not related to the amount of error in

the data nor to systematic misperceptions about the events. Furthermore, there was no evidence that certain types of victims forward telescope more than others. Forward telescoping results in an overestimation (in unbounded surveys) of the amount of crime committed against persons who forward telescope. Thus, the fact that victim characteristics were not related to forward telescoping is an important result from the study.

It should be emphasized, however, that if offenses which are forgotten are characterized by different patterns and relationships than those recalled, then the survey data would not produce reliable conclusions about such relationships. Thus, the results of the forward records check need to be replicated and reverse record checks should be designed to test bias in the forgotten incidents.

Although the survey data appear to be relatively free of systematic misperceptions by certain types of victims, there is a tentative indication that persons with negative attitudes toward the police projected these attitudes into their recollection about what the police did, how long it took the police to arrive, whether there were witnesses present, and the extent of the victim's activities to prevent the crime. Thus, studies that seek to explain victim attitudes toward the police as a function of police activities or response time should be cautious in interpreting the causal direction of observed correlations. The data presented here indicate that persons with negative attitudes may perceive these in a different way than persons with positive attitudes, even though the "facts" are the same.

4. Evidence from this study and others indicates that victimization survey data cannot be used to measure trends in the victimization rate within the retrospective recall period covered by the survey. If telescoping and forgetting were distributed equally (or randomly) across the various months in the

recall period, then one could use the data from a single survey to estimate monthly or quarterly victimization rates (provided, of course, that the size of the sample was sufficiently large). There is a considerable body of evidence, however, which demonstrates that telescoping is primarily forward rather than backward, and that forgetting increases with the length of the recall period. Thus, even though the survey data contain information about the date of each crime event, a single survey yields an estimate only for the entire 12-month recall period (or six months) and not for individual months.

This problem greatly reduces the value of survey data for evaluation purposes. Survey data are needed for most crime prevention and deterrence programs as well as for other evaluations which require comparisons across cities, and programs that would alter citizen reporting rates or police discovery rates. Because these types of programs are focussed on entire geographic areas, it is usually impossible to have a true field experimental design, and the best procedure available to the evaluator is the quasi-experimental time-series design that requires twelve to fifteen pre-program estimates of monthly (or quarterly, or yearly) victimization rates and several post-program estimates. If the survey data could be disaggregated, then each survey using a 12-month recall period would provide twelve estimates; two surveys would yield 24 estimates, and so on.

5. The analysis indicated that the age of the victim was not related to the amount or type of error in the data. Moreover, the study showed that persons who make errors in recalling the correct date are no more likely than others to have given different information to the interviewer than to the police. Both of these results were somewhat surprising, since age is generally presumed to influence memory loss, and since it is reasonable to believe that persons

who make one type of error would be more inclined to make others. A partial explanation was revealed in the analysis of "don't know" responses. The frequency of these increased with respondent age and with the frequency of error in recalling the date of the incident. Thus, it is possible that older victims and those who guess (incorrectly) at the date of the incident tend to say "don't know" to other questions rather than provide erroneous information.

6. Many of the incidents that respondents said were reported to the police were not found in police files. Through a series of adjustments in the data, the best estimate is that approximately 32 percent of the survey incidents that presumably were reported could not be found either because they were not reported or because they were not recorded as a crime by the police.

Discussion and Recommendations

Victimization surveying has the potential for providing considerable information and new knowledge about crime which cannot be obtained from official crime statistics. Unreported crimes constitute a large proportion of all incidents that occur. The absence of unreported incidents in official data represents an inherent and uncorrectable problem with using the official statistics for a variety of research and evaluation purposes. Survey data should provide superior estimates of the amount, costs, and characteristics of criminal victimization. Analysis of the data could, potentially, provide important new insights about crime causation, factors contributing to victimization, and the distribution of crime as well as its costs among different population subgroups.

The results of this study indicate that survey data are sufficiently reliable and valid to be used with confidence for some of these purposes, but

doubts remain about others. Furthermore, the results of a single study, conducted in a single city, with a small sample, are not final answers to these questions and all of the propositions tested in the Portland study need to be reexamined and replicated in other studies before final conclusions are drawn. Although the survey data appear to be quite good in many respects, the full potential of victimization surveying for generating information of the type mentioned above will not be realized unless there is a resumption of methodological research into the types of bias in survey-generated information about crime and the efficiency of various solutions for improving its reliability and validity.

The first major recommendation from this study is:

1. A series of multi-purpose reverse record checks should be conducted in several different cities. The studies should be designed so that information can be obtained in relation to several propositions and the results compared across the different cities. The topics of major concern should include:

(a) The amount of telescoping, forgetting, and differences (between police and survey data in factual information about the incident),

(b) The characteristic nature of the differences (higher or lower survey estimates in comparison with police data, for example),

(c) The extent to which telescoping, forgetting, and differences between police and survey data are correlated with characteristics of the victim, the offense, and the offender.

Ideally, the samples drawn for the studies should be large enough to permit at least a minimum amount of experimenting with different surveying methods, different questioning procedures, and/or different recall periods. The purpose of these studies would be to test propositions such as those examined in this study about the types of bias in the survey data and to experiment with methods of reducing them.

One of the most important contributions that could be made by victimization

surveying is in the improvement of program evaluation efforts. The survey data are needed for evaluating community-based crime prevention programs, crime deterrence programs, programs that alter citizen reporting rates and/or police discovery of crimes in progress, and programs or strategies that are being tested comparatively across different cities. This potential will not be realized unless there are several substantial changes made.

It should be emphasized that true experimental designs are not in common use for field evaluation and are impossible for many types of community-based prevention or deterrence programs. Thus, the best evaluation design that can be used is a quasi-experimental time series approach which requires numerous time points prior to and after the program is implemented.

Victimization survey data at the national level would be suitable for such evaluations if the surveys were conducted with sufficient frequency, prior to the implementation of a program so that twelve to fifteen monthly or quarterly estimates of victimization rates would be available, and a continuing series of monthly or quarterly estimates could be made after the program is implemented. Even though these methods would be appropriate in terms of data reliability and validity, the national data cannot be used for program evaluation, because there are no national programs that use common strategies and which are implemented simultaneously throughout the country. Since the method used for national data collection requires personal interviews every six months of a panel of respondents, there are few (if any) cities or states that could afford to conduct these kinds of surveys on a continuing basis. Although the federal government may be willing to fund victimization surveys in several areas for the purpose of evaluating innovative programs, the areas cannot be identified far enough in advance of program implementation to provide the twelve to fifteen pre-program surveys that are

needed to generate twelve to fifteen monthly or quarterly estimates of victimization rates. Thus, even when victimization surveys are fielded in conjunction with new programs, the results (at best) are a "before and after" evaluation design which is one of the weakest possible types. It is almost impossible to draw definitive conclusions about the effectiveness of a program in terms of crime reduction when a "before and after" design has been utilized.

The second major recommendation from this study is:

2. One or more studies should be initiated to test different types of surveying procedures that are (a) inexpensive enough to be widely implemented in cities and states and (b) designed so that a single survey can generate several time-specific victimization estimates. The types of methods that should be tested include mailed and telephone interviewing using rolling monthly sampling procedures. The types of biases that need to be examined include those named under the first recommendation (telescoping, forgetting, and informational differences). In addition the studies should seek solutions to the complex methodological questions concerning how the data produced from rolling monthly survey procedures should be adjusted to provide the most accurate month-by-month estimates. (This type of procedure is examined more fully in another report produced from this grant, see Schneider, 1977).

A major assumption underlying much of the discussion in this report is that there are certain types of research and evaluation questions for which official data are inherently inappropriate because they do not contain unreported incidents. Although most researchers believe this to be an insurmountable problem for certain types of research, there is very little empirical information concerning the situations or conditions which, if they exist, make it reasonable to assume that official data are a representative and unbiased subset of all crime incidents. It is not known, for example, whether

the patterns of relationships found in survey data (reported and unreported) differ from the patterns in official data. At the heart of the issue is the question of how the reported and recorded incidents differ from these that were either not reported or, if reported, were not recorded.

The third major recommendation is:

3. One or more studies should be undertaken to study the differences between reported and unreported incidents. The differences in terms of general descriptions of the types of crimes, types of offenders, and so on should be included, but the major focus of the study should be to determine whether there are differences in the patterns and relationships within each set of data which would confound or invalidate the conclusions drawn by studies that used only one of the data sets.

APPENDIX A

REVIEW OF THE DIFFICULT MATCH/NO-MATCH DECISIONS*

1. Offenses that involved the correct household, but were considered not to be matches:

   (a) Survey: A man reported that a camera and its case were stolen from his car, parked at home, in May 1974.

   Police: Woman meeting description of spouse of survey victim reported her husband missing and expressed concern about a possible suicide attempt since he was on "pills." Report was made in September, 1973.

   (b) Survey: Thirty-year-old son, living with his parents, reported that the tape deck in his car had been stolen in April, 1974. Offender gained entry because car windows had been broken and were not in place at time of the offense.

   Police: Mother of the survey victim reported that food stamps, a pistol, and other items had been stolen from her car in September, 1972.

   (c) Survey: Mother reported that her 15-year-old son had had his bike stolen in September, 1973. It had been locked to a pole in the yard.

   Police: Male victim was staying with his sister at address of survey respondent while he recovered from an operation when several personal items were stolen from his unlocked room.

   (d) Survey: A 50-year-old female living with her husband reported that in March, 1974, she was followed by two persons who stole her purse at night near her home. They mailed the identification cards and purse back to her, but not the money. She also reported that a roomer in her house (boarder) had threatened to have someone kill her if she continued in her efforts to have him evicted from his room.

   Police: Police records have one offense at the location. It involved a 21-year-old male, renting a room in the basement, who says that he was robbed by an offender armed with a vase in January, 1972. Police records show three other offenses in the vicinity, but none involved the victim included in the survey.

2. Dissimilar events found in police records prior to survey recall period and were considered not to be matches:

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*These incidents were the only ones that involved difficult judgments. The "90 percent rule" required 90 percent concurrence to constitute a definite match and 90 percent divergence to be considered a definite no-match. No characteristics of the crime that would be used in classifying it or in the subsequent analyses were used to decide whether an event matched or did not match.

(a) Survey: A woman reported that in September 1973 someone tried to gain entry at night through a screen. She turned on the lights and called a neighbor, later notifying the police.

Police: A burglary was reported by the victim at the same address in the survey in March, 1973. The family was out for the evening and returned home to find the house completely ransacked. (Police records also contained an event that matched one the survey respondent recalled in the interview but said was not reported.)

(b) Survey: A man reported that his car was stolen from his home in December, 1973.

Police: Spouse with same address as survey victim had reported several items stolen from her car in December, 1972 while it was parked in a parking lot. The items included bongo drums, conga rims, and other musical equipment.

(c) Survey: Man reported that an acquaintance of his who had a key to his house loaned it to someone not known to the survey respondent. The unknown person entered the home and tried to take clothes and other things belonging to the owner of the house. The survey respondent was notified of the incident by the acquaintance who had walked in on the offender and thwarted the attempted burglary.

Police: A man matching the description of the survey victim reported a break-in to his basement in February, 1972. The incident was one of a series that began with entries through unlocked doors. The victim nailed the basement door shut, but in this incident it had been kicked in and jammed into the door frame.

(d) Survey: The father of a 6-year-old girl reported that his daughter had been lured to the home of a 54-year-old acquaintance in the neighborhood who had tried to sexually molest her (oral) but was prevented from further molestation by a relative. The event was said to have occurred in April, 1974.

Police: The father of a 6-year-old girl reported an incident to the police in August, 1973, concerning an incident that occurred in the park between his daughter and a 9- or 10-year-old neighborhood boy. The father told the police later that after further conversations with his daughter, he decided the incident was inconsequential and the kids were just playing around. A 6-year-old playmate of the girl was a witness to the event.

(e) Survey: A 30-year-old male (relative of the survey respondent) said that he was threatened twice in February, 1974--once with a gun and once with a knife. The event happened, the respondent said, during the time that Carl Bowles had escaped and was throught to be in the area. The threats occurred near the home.

Police: Police records show a car stolen from an address within five blocks of the survey incident and have one report of a person reporting their license plates were stolen. Both incidents occurred in December, 1972.

APPENDIX B

The seriousness scale used in the analysis is a replication of Sellin and Wolfgang's 1964 index (Thorsten Sellin and Marvin E. Wolfgang, The Measurement of Delinquency. New York: Wiley, 1964).

a. Injury Component

Question (INC069): (If victim was injured): Did you receive treatment at a hospital, at a doctor's office, or what type of treatment did you receive?

Scoring:	Score
Blank (indicates no injury)	0
1. No treatment	1
2. Treated in doctor's office	4
3. Treated in emergency room	4
4. Overnight at hospital, or more	7

b. Sex Offense

(Crime codes of 120000 through 129999 are rape)

Rape	8

c. Weapon Intimidation

Question (INC030): Did the person(s) have a weapon such as a gun or knife, or something he used as a weapon, such as a bottle or wrench?

Scoring:	Score
1. No	0
2. Yes, gun	4
3. Knife	4
4. Gun and knife	4
5. Other dangerous weapon	4
9. Don't know	0

d. Physical or Verbal Intimidation

Question (INC031): Did the person(s) threaten you with harm in any way?

Socring:	Score
1. No	0
2. Yes	2
9. Don't know	0
Blank	0

APPENDIX B (continued)

e. Forcible Entry

Question (INCO21): Was there any evidence that the offender(s) forced his way in or tried to force his way into the building, such as a broken lock, broken window, forced door, forced window, or slashed screen?

Scoring:	Score
1. Blank or No	0
2. through 8. (other evidence)	1
9. Don't know	0

f. Costs and Losses

Questions concerning losses are called COST1, COST2, COST3...COST5, and represent, in order, money lost; dollar value of items lost and dollar value of damages, none of which was recovered; insurance paid; value paid by offender; value paid by anyone else. The sum of these represents the total value of the loss.

Scoring:	Score
Under $10	1
$10-250	2
$251-2000	3
$2001-9000	4
$9001-30,000	5
$30,001-80,000	6
$80,001-highest	7

APPENDIX C

TABLE 1.

ALL CRIMES

Reported in Interview

Reported to Police	Jan 1973	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan 1974	Feb	Mar	Apr	May	Jun	Jul	Aug	TOTAL
1972 Feb													1								1
Mar						1															1
Apr							1														1
May						1															1
Jun							1														1
Jul																					
Aug						1															1
Sep					1																1
Oct					1				/	1	1										3
Nov																					
Dec	1												1								2
1973 Jan																					
Feb																					
Mar										1		1	1		1						4
Apr					1				1												2
May					6		1	1						1			2				11
Jun				1	2	3				1				1		1					9
Jul		1		1	1		6	2		1				1							13
Aug								6		1											7
Sep						1		2	4	5							1				13
Oct									1	10	2		1	1							15
Nov					1		1	1		1	13		2	1	1						21
Dec									1	1	2	7	1		1						13
1974 Jan											1	2	7	2		1					13
Feb														6	3	1			1		11
Mar			1						1					2	5	1	1				11
Apr														1	7	12	1				21
May								1						1	2	3	13	1			21
Jun																	1	2			3
Jul																			3		3
Aug																					
TOTAL	1	1	1	2	13	7	10	13	8	22	19	10	14	17	20	19	19	3	4		203

TABLE 2.

BURGLARY

Reported in Interview

Reported to Police	Jan 1973	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan 1974	Feb	Mar	Apr	May	Jun	Jul	Aug	TOTAL
1972 Feb																					
Mar																					
Apr																					
May																					
Jun																					
Jul																					
Aug																					
Sep					1																1
Oct										1											1
Nov																					
Dec	1																				1
1973 Jan																					
Feb																					
Mar										1											1
Apr					1				1												2
May					4		1	1									1				7
Jun				1		1															2
Jul		1		1			1														3
Aug								2													2
Sep									2	3											5
Oct									1	8	2			1							12
Nov					1		1			1	8		1								12
Dec											1	4			1						6
1974 Jan												1	4			1					6
Feb														3	3	1					7
Mar									1					1	5						7
Apr															4	5	1				10
May								1						1	2	1	5	1			11
Jun																	1	1			2
Jul																			2		2
Aug																					
TOTAL	1	1		2	7	1	3	4	5	14	11	5	5	6	15	8	8	2	2		100

TABLE 3.

LARCENY

Reported in Interview

Reported to Police	Jan 1973	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan 1974	Feb	Mar	Apr	May	Jun	Jul	Aug	TOTAL
1972 Feb													1								1
Mar						1															1
Apr																					
May						1															1
Jun							1														1
Jul																					
Aug																					
Sep																					
Oct					1						1										2
Nov																					
Dec													1								1
1973 Jan																					
Feb																					
Mar												1	1								2
Apr																					
May					2									1			1				4
Jun					1					1				1		1					4
Jul					1		3	1		1				1							7
Aug								3		1											4
Sep						1			1	2							1				5
Oct										2			1								3
Nov								1			1				1						3
Dec									1			1									2
1974 Jan											1	1									2
Feb														3							3
Mar			1											1			1				3
Apr														1	1	3					5
May																1	4				5
Jun																		1			1
Jul																			1		1
Aug																					
TOTAL			1		5	3	4	5	2	7	3	3	4	8	2	5	7	1	1		61

TABLE 4.

AUTO THEFT

Reported in Interview

Reported to Police	Jan 1973	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan 1974	Feb	Mar	Apr	May	Jun	Jul	Aug	TOTAL
1972 Feb																					
Mar																					
Apr							1														1
May																					
Jun																					
Jul																					
Aug						1															1
Sep																					
Oct																					
Nov																					
Dec																					
1973 Jan																					
Feb																					
Mar																					
Apr																					
May																					
Jun					1	1															2
Jul							2														2
Aug																					
Sep								1													1
Oct																					
Nov											2		1	1							4
Dec												2									2
1974 Jan													3	1							4
Feb																					
Mar																					
Apr															1						1
May																1	1				2
Jun																					
Jul																					
Aug																					
TOTAL					1	2	3	1			2	2	4	2	1	1	1				20

TABLE 5.

ASSAULT

Reported in Interview

Reported to Police	Jan 1973	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan 1974	Feb	Mar	Apr	May	Jun	Jul	Aug	TOTAL
1972 Feb																					
Mar																					
Apr																					
May																					
Jun																					
Jul																					
Aug																					
Sep																					
Oct																					
Nov																					
Dec																					
1973 Jan																					
Feb																					
Mar																					
Apr																					
May																					
Jun						1															1
Jul								1													1
Aug																					
Sep									1												1
Oct																					
Nov											2										2
Dec										1	1		1								3
1974 Jan														1							1
Feb																					
Mar																					
Apr															1						1
May																	2				2
Jun																					
Jul																					
Aug																					
TOTAL						1		1	1	1	3		1	1	1		2				12

FOOTNOTES

1. For general reviews of the problems with official data see Biderman & Reiss (1967), Black (1970), Ennis (1967), Skogan (1975b), Schneider (1975), and National Research Council (1976).

2. See Seidman (1974) and Skogan, "Measurement problems in official and survey crime rates."

3. See Schneider, et al, 1975b.

4. See Seidman (1974) and Skogan.

5. Levine (1976).

6. Skogan, "Measurement problems in official and survey crime rates, 1975b.

7. Levine (1976).

8. Biderman, Victimology and victimization surveys, in Victimology: A New Focus, 1975.

9. Schneider, The 1974 Portland victimization survey: Report on procedures, 1975.

10. No names from the original police reports were taken or copied during the procedures and none were used by the research team in any way except to match the partial name identifier on the coding form. This consisted of the first name and the first and last initials of the last name.

11. Sparks, Crimes and victims in London, in Skogan (ed.) Sample Surveys of the Victims of Crime, 1976.

12. According to Bershad (1969), the index of inconsistency for the 2x2 matrix is, simply, the complement of the correlation coefficient or the complement of phi which is identical to r for 2x2 matrices. Computation of the n x n matrix (L-fold) is:

$$I = N \frac{N - (\text{sum of the diagonal})}{N^2 - (\text{column totals squared and summed})}$$

13. Survey Police Activity and Police Police Activity are composed of the following variables: Police restored order; police warned offender; police advised victim; police promised surveillance; police arrested offender; police investigated; and other. The "Other" category (each with a score of one) includes Police took report over phone; searched area; said they would notify if property found; assisted victim; abused or accused victim; recovered children; fingerprinted; and pursued offender.

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