RANDOM DIGIT DIALING
LOWERING THE COST OF VICTIMIZATION SURVEYS

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PoliceFoundation

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FOREWORD

In 1972, in the first Police Foundation publication about evaluation, I noted, "Another point not news for those few with experience in trying to measure the results of social experiments is that many of our tools are blunt." I added, ". . . the need for better—and cheaper—ones is evident. That means that those who can should support development of better measurement and evaluation tools as well as good experiments themselves." What I had particularly in mind was the very high cost of victimization survey data, for some important purposes a sharper tool than reported crime data. The cost of detailed victimization surveys is so high that, except in rare instances when federal or other large-scale outside funding can be obtained, police or city administrations cannot afford to use them. Because of their cost, the Foundation's use of them in measurement of experiments has had to be limited.

As it turned out, the Foundation was able to seize and act quickly on an opportunity to test whether the random digit dialing (RDD) telephone survey technique would yield just as good results as the conventional personal household interview technique which is inherently so expensive. If so, the cost of obtaining victimization survey data could be greatly reduced. The great, largely unrealized, potential for usefulness of city-level victimization data to police administrators, city and urban county managements, state planners and the research community could come a major step closer to realization.

In the course of conducting the Foundation-sponsored Urban Institute evaluation of the Cincinnati experiment with neighborhood

team policing known as ComSec, Alfred Schwartz became acquainted with prior work of the authors of this report and brought it to Foundation attention. The authors' work showed that RDD telephone survey techniques had matched Census Bureau accuracy in sampling populations in Ohio. Still untested were two questions: whether telephone interviews could regularly be maintained long enough to administer the lengthy set of questions necessary in victimization surveying and whether people would be as willing to report crime victimization experiences over the telephone as in face-to-face interviews.

The Law Enforcement Assistance Administration's National Criminal Justice Information and Statistics Service and the Census Bureau have designed and conducted a series of victimization surveys in 26 cities during the past four years. In this series, the Cincinnati LEAA-Census Bureau victimization survey was to be conducted in February and March 1974, providing the opportunity to conduct a comparative test of the two interview techniques. With Police Foundation Board approval in December 1973, the authors designed the comparison test, the Foundation's Evaluation Advisory Group reviewed the design and plans for its execution, the authors created the capacity to manage, monitor, and conduct the RDD survey, and they completed it in April. Mr. Schwartz has, from the beginning, assisted and guided the research on behalf of the Foundation and interested LEAA and Census Bureau officials have been fully cooperative.

This research report, reviewed by the Evaluation Advisory Group, describes the comparison test. It shows that the RDD technique produced results no less accurate than those of the conventional personal interview victimization survey technique and did so at a cost 70 to 80 percent lower. The larger the survey, the greater the savings.

The report also constitutes a handbook with straightforward directions for administrators, planners, researchers, or sponsors of their work who may wish to replicate the use of RDD victimization survey techniques in other jurisdictions.

Such replications, building upon this first major step, are essential. The Foundation hopes that they may follow speedily, perhaps under national or state sponsorship, to establish firmly the usefulness of RDD victimization surveying and to show whatever boundaries there may be to the conditions under which it should be used.

Exploratory work is already underway at LEAA to determine applicability of the RDD technique to the 60,000-household National Crime Panel Surveys being conducted by LEAA and Census. Should applicability be established the savings would indeed be substantial.

Thanks are due to the members of the Police Foundation Evaluation Advisory Group whose thoughtful assistance improved the design and interpretation of the research reported here. They are Professor Francine Rabinovitz, School of Urban and Regional Planning, University of Southern California; Professor Albert Reiss, Department of Sociology, Yale University; Professor Lee Sechrest, Department of Psychology, The Florida State University; and Professor Hans Zeisel, The Law School, The University of Chicago.

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EXECUTIVE SUMMARY

INTRODUCTION

One major problem that has long existed in the area of law enforcement and criminal justice is the difficulty of measuring reliably and efficiently the incidence of crime. In several ways this limit on the ability to measure crime has put our society at a disadvantage in its attempts to deter crime. First, reliable measures of crime victimization are needed to improve the effectiveness of operations planned to combat crime. Then, as operational programs progress, accurate measures of changes in crime victimization must exist so that the effectiveness of such programs can be evaluated. Thus, reliable measures of crime are essential for both *planning* and *evaluation*.

Until recently, criminal justice researchers did not have adequate methods for measuring crime victimization. In the late 1960s, the Law Enforcement Assistance Administration (LEAA), working with the Census Bureau, began to develop an effective tool based on personal interview survey techniques. A major drawback of the LEAA-Census approach, however, is its cost. For most applications, the LEAA-Census survey methods are prohibitively expensive, especially when local programs are being planned or evaluated, because local resources usually cannot meet the costs the LEAA-Census approach requires. This problem is a serious one because crime is fought mostly on the local level.

Now a technique that promises to be less expensive but equally reliable for measuring crime victimization is at hand. Random digit dial-

ing (RDD), a relatively new telephone survey technique, appears to provide equal reliability for only a fraction of the cost. In the spring of 1974, with financial support from the Police Foundation, the Behavioral Sciences Laboratory at the University of Cincinnati conducted a direct field comparison of RDD and the LEAA-Census approach. This monograph reports on that test of the efficiency of RDD as a tool for measuring crime victimization.

Following this Executive Summary, this volume is divided into two parts. Part I describes what RDD is and how it was tested against other survey interviewing methods. Chapter 1 includes a historical review of the collection of crime victimization data in this country and explains the development of the LEAA-Census techniques on which the RDD survey relied heavily. Chapter 2 discusses the basic appeal of RDD, its advantages and disadvantages. Chapter 3 poses the hypotheses to be tested, while Chapter 4 presents the results of the test of RDD's accuracy and efficiency. Part II (Chapters 5 and 6) is a manual on how to use random digit dialing; it includes details on organizing and processing data collected. Five appendixes, relating chiefly to Part II, follow.

BACKGROUND

Random digit dialing (RDD) is a simple telephone survey technique that has proved to be an efficient, accurate, and highly cost-effective method for measuring crime victimization. This book describes a field test of the RDD survey method carried out in Cincinnati, Ohio, in April 1974 to measure crime victimization. It compares the field test results with those produced by a personal interview survey using the same questionnaire in the same city two months earlier. This book then tells in some detail how to use RDD.

Telephone surveys in general have many advantages over personal interview and mail surveys. These advantages include very high response rates, great savings in field costs, safety and convenience for the interviewer, and confidentiality for the respondent. The near ubiquity of telephones in the United States assures that few persons are excluded from a telephone survey on grounds of inaccessibility. Telephones facilitate follow-ups and make it easy for surveyors to cover large geographical areas.

Random digit dialing has all the usual benefits of telephone surveying, while apparently avoiding the sampling biases—particularly the exclusion of households with unlisted telephone numbers—that traditionally have plagued telephone surveys. RDD can be used to draw quickly, easily, and inexpensively a random sample of households (with telephone service).

A researcher who wants to obtain an RDD sample must first determine all the operating exchanges in the desired geographic area and then select at random one of the exchanges with its corresponding area code, if necessary. The number is completed by randomly selecting the last four digits. This two-step sequence is repeated until the desired quantity of telephone numbers has been generated. The random numbers can be selected by computer or by hand using a random number table. The second part of this book elaborates on this sampling procedure, the recruitment and training of interviewers and supervisors, the questionnaire design, the controls for screening out ineligible respondents, and suggested call-back procedures. It also gives details on processing and analyzing the data acquired.

RDD TELEPHONE SURVEY COMPARED TO PERSONAL INTERVIEWING

The test of RDD detailed in the first part of this book pitted survey sampling by random digit dialing and telephone interview against personal interviewing using a sample drawn by traditional multistage, stratified, clustered sampling procedures. The Law Enforcement Assistance Administration (LEAA) had commissioned the United States Census Bureau to conduct a crime victimization survey using traditional personal interviewing methods in Cincinnati during February and March 1974, as part of a series of such surveys in large American cities. With financial support from the Police Foundation, the staff and facilities of the Ohio Institute for Public Opinion were employed to ask the same questions by telephone to a sample of Cincinnati households selected by random digit dialing.

In addition to a citywide sample, the RDD surveyors further tested RDD against the LEAA-Census method in Cincinnati's Police District No. 1 (P.D. No. 1). Because this district includes the city's worst ghettos, it seemed to be an ideal choice to provide a rigorous test of the efficacy of RDD. The Census Bureau prepared tabulations based

on its respondents living within that district for comparison of sample results.

DEMOGRAPHIC RESULTS

Demographically, the RDD and LEAA-Census samples drawn from throughout Cincinnati were shown to have been representative of the same population. The poor and the blacks were not underrepresented as compared to the Census sample, as had been feared. The RDD sample was slightly skewed toward the more highly educated segments of the population, but the difference was not serious.

The samples from P.D. No. 1 did not differ on six of the nine demographic variables. The fact that RDD data from P.D. No. 1 did not have an education bias indicates that the discrepancy in the citywide data may represent chance sample selection rather than a systematic bias of RDD. The RDD method slightly undersampled males in P.D. No. 1, while the LEAA-Census technique seems to have somewhat undersampled blacks there.

Thus the two techniques were reaching samples of slightly different populations in that police district. Both methods certainly have some degree of error, but it is hard to choose which is the more representative in this situation. For most research purposes, however, the differences should not prove to be important. Most studies, including crime victimization studies, can employ RDD even when the target population is low-income, inner-city residents. Moreover, the P.D. No. 1 data indicate that RDD may be somewhat more successful than personal interview surveys in locating black respondents and securing their cooperation.

VICTIMIZATION RESULTS

RDD proved as satisfactory as personal interviewing for collecting crime victimization data. In the citywide samples, the victimization rates for personal crimes were slightly higher in the RDD sample, while the RDD rates for household crimes were considerably higher in every category; the rate for all household crimes measured in the RDD survey was 39 percent greater than the rate found in the LEAA-Census survey. These higher reported rates are evidence that RDD may be a superior method for collecting victimization data. In the P.D. No. 1 sample, the

victimization rates were very similar in the two surveys, with none of the rates significantly different.

The proposal for the RDD project submitted to the Police Foundation to obtain funding for this research stated, "If the RDD survey measures higher (beyond sampling error) crime victimization than the LEAA survey, the RDD survey can probably be judged a superior method." Because of the difficulties in measuring the "true" level of crime victimization, the scientifically sound method that measures the most crime is probably the best method. This research did show RDD to be scientifically sound and to measure significantly more crime than the LEAA-Census method.

ATTITUDINAL RESULTS

Expressions of opinions about crime trends, neighborhood safety, and police performance constituted the third type of data measured in the survey. Answers to four of the five questions asked in the RDD and LEAA-Census surveys showed no differences between the samples. The differences on the remaining question were statistically significant, but substantively rather small—that is, an analyst would have come to the same conclusions regardless of which set of answers was used. Unfortunately there is no absolute standard by which to decide if either of the surveys measured "true" public opinion. Nonetheless, the close similarity of the results leads to the conclusion that RDD and the more traditional LEAA-Census method are equally good for measuring public opinion data.

A review of all the evidence suggests that the performance of the random digit dialing technique is remarkably good. Replications of this research should be conducted to verify RDD's value, but the current evidence strongly supports RDD as a useful tool for the measurement of crime victimization.

RELATIVE SAMPLE BIASES

The omission of citizens without telephone service does not appear to bias the sample demographically; nor does it adversely affect the substantive information being collected—in this case, crime victimization data and attitudes toward crime-related matters. This finding has special significance when one considers that persons without telephone

service are slightly more likely to be poorer, less educated, and non-Caucasian.

Although telephone coverage is not complete, there are two reasons why this does not put a telephone survey at a special disadvantage compared to traditional interviewing methods. First, interviewing by telephone misses only a small segment of the population. This group unreachable by telephone is currently less than 10 percent of the total households, and it is not uniformly poor, black, and less educated. Given the small size and the heterogeneity of nonsubscribers, their differences have only a minor net effect on the representativeness of an RDD sample. Second, traditional sampling and personal interviewing also do not cover the entire population. Although personal interview surveys can reach nontelephone households, they have some trouble locating and interviewing the poor, the black, and the less educated members of society. Thus the two techniques have a propensity to underrepresent similar demographic characteristics, and the net results are samples which are demographically very comparable.

RDD COST ADVANTAGES

The one area, therefore, in which RDD and LEAA-Census surveys can be truly differentiated is cost. RDD costs about one-quarter as much as the personal interview technique in this crime victimization survey comparison. The relative cost of RDD goes up somewhat when samples of fewer than 1,000 are needed, and the cost decreases somewhat for larger samples.

RDD has proved itself a very valuable tool for the planner and evaluator in the criminal justice system. Researchers can use RDD to gather accurate crime victimization and attitudinal data. RDD's relatively low cost makes it more accessible to local police departments and other groups that do not have large research budgets. The police can use RDD to measure crime, administrators of the courts can use it to tap citizen attitudes toward innovations in the judicial system, and public officials can use it to measure the acceptability of proposed prison reform. Although it is not suggested that RDD replace all other forms of survey research, this technique represents a valuable instrument for collecting useful data at a reasonable price.

PART I

CHAPTER 1

HISTORICAL PERSPECTIVE

WHAT IS RDD?

Random digit dialing (RDD) is telephone interviewing coupled with the use of a sample of telephone numbers generated completely at random. Although RDD is not a new technique,[1] many investigators have avoided it for fear that it produces seriously biased results. In this study, however, RDD has proved to be quite effective.

A researcher wishing to obtain an RDD sample must first determine all the operating exchanges in the desired geographic area. Once the list of eligible exchanges is obtained, the procedure for generating the numbers is quite straightforward. First, one of the exchanges with its corresponding area code, if necessary, is selected at random. Then the number is completed by randomly selecting the last four digits. This two-step sequence is repeated until the desired quantity of telephone numbers has been generated. The random numbers can be selected either by computer or by hand with a random number table. (Chapter 5 elaborates on this sampling procedure.)

Before proceeding to a detailed discussion of the random digit dialing survey technique and its use in measuring crime victimization, it seems appropriate to review the recent history of the measurement of crime in this country. Since 1930, the FBI has been maintaining national crime statistics in the form of the Uniform Crime Reports (UCR). The UCR rates are still the most widely quoted measure of crime. A different crime victimization measure, derived from LEAA-sponsored surveys, is now gaining increased use and recognition.

UCR STATISTICS COMPARED TO SURVEY RESEARCH DATA

The UCR statistics are a composite of the crime reported to the nation's police departments. Local police departments send to the FBI data about crimes reported to them. The FBI then tallies the data and publishes the resulting statistics. These statistics have been quite useful, but the method of collection has led to some difficulties. First, the UCR statistics do not include data for all geographic areas, because a few jurisdictions do not participate in the program. Second and more important, the UCR measure includes only *reported* crime. Thus, any crime not reported to the police is not included and does not officially exist.

Two problems for planners and evaluators in the criminal justice system, therefore, arise from the UCR method of collecting statistics. First, the UCR figures do not measure all crime. In fact, in some of America's major cities the actual crime rate is probably at least five times the official UCR rate.[2] Although it is hard for *any* method to measure all the crimes committed, a system that includes only a small portion of the actual crime has a serious flaw.

The second problem is that when the UCR rates increase or decrease, it is impossible to determine the real cause of the change. No one can be sure whether the actual rate or the percentage of crime being reported has changed. For example, suppose the UCR rate of burglary increased from 50 reported burglaries per 1,000 citizens to 60 per 1,000. That increase could be the result of a 20 percent increase in crime, or it could mean that the number of burglaries has stayed the same but the reporting rate has increased 20 percent. Changes in the reporting rate are not uncommon and the percentage of crime reported to the police is subject to many variations.

As the Presidential Commission on Law Enforcement and Administration of Justice pointed out in 1968, "One change of importance in the amount of crime that is reported in our society is the change in the expectations of the poor and the members of minority groups about civil rights and social protection."[3] As these groups come to trust the police more, they will report more of the crimes committed against them. In such a situation, the UCR rate would increase even though the actual crime rate might not necessarily change. The middle and upper classes, too, will tend to report more of the crimes com-

mitted against them if they feel that the police can solve the cases. Thus, when UCR rates are relied on, these rates can be misleading. If the police do a good job, they will hear about more crime and the UCR crime rate will go up because the reporting has increased. If the police do a poor job of combating crime, the reporting rate may decrease but the "true" crime rate may increase.

The Presidential Commission comments, "Perhaps the most important change for reporting purposes that has taken place in the last 25 years is the change in the police. Notable progress has been made during this period in the professionalization of police forces. With this change, Commission studies indicate, there is a strong trend toward more formal actions, more formal records, and less informal disposition of individual cases."[4] Thus, alteration in local police practices can affect the amount of crime reported to the FBI without any change in the actual crime rate or even in the amount of crime being reported by the public to their local police.

Another factor that probably increases the amount of reporting for some crimes is the sizable increase in insurance coverage against theft.[5] As more homeowners and businesses take out theft insurance, the reporting rate probably increases because it is often necessary to file a report with the police in order to be reimbursed by the insurance company for the loss.

Obviously, there are serious problems in relying on UCR statistics, especially when sensitive measures of crime are required for planning or evaluation purposes. It is important to develop techniques that provide a more accurate and complete measure of crime victimization. Because the reporting of crime to the police is a confounding factor in the measurement of crime, it seems appropriate to adopt a method that does not require the citizens to report through the police. Survey research—interviewing a representative sample of the citizens and asking them what victimizations they have experienced during a specified period—can accomplish such an objective. Experience has shown that a survey-based approach presents a more nearly complete and accurate measure of the victimization rates for certain categories of crime.[6] In addition, a survey is not subject to the kinds of reporting variations that plague the UCR rates.

A survey-based method does not resolve all the problems of measuring crime. Although the survey method measures far more crime than the UCR does, it still will not detect all crime. Some of the people

who do not report crime to the police will also not report victimizations to a survey interviewer. The victims may not want to take the time; they may fear that the interviewer is a criminal "setting them up"; some, such as rape victims, may not want anyone to know about crimes committed against them; or their knowledge of the law may be inadequate for them to identify what happened to them as a crime. The crime detected by the survey method is, therefore, a *lower bound* on the true level of crime, for categories of crime that can be so measured.

The survey method is the most effective approach presently available for measuring crime, but as noted, it does not measure all crime. For this reason, as crime surveys are improved or better methods are developed, they will measure more crime. On the assumption that there is far more crime perpetrated than has heretofore been measured by any means, one can postulate that the scientifically sound method that detects the most crime is probably the best method.

Survey research as a method for collecting data on crime and its victims has one other major advantage over the UCR approach. As the survey interviewer determines the amount of crime committed against an individual, the interviewer can also gather from the respondent other demographic and attitudinal data of use to planners and evaluators in the criminal justice system. Detailed data about the time, place, and perpetrators of the crimes reported can also be gathered. Analysis of these data can reveal which groups are most likely to be victimized, when crimes occur, which groups do and do not respect the police, and many other items of interest. The planner and manager in the criminal justice system should find such data extremely useful, especially when the questionnaire has been designed to answer specific policy questions. A well-designed survey can provide answers to questions that had previously been left to "educated" speculation. Unfortunately, the UCR does not have this flexibility.

THE DEVELOPMENT OF THE LEAA APPROACH

In the mid-1960s, the Presidential Commission on Law Enforcement and Administration of Justice sponsored the first major surveys of crime victimization. A national survey of crime administered by the National Opinion Research Center (NORC) in 1966 found that the actual crime rate is at least twice that reported by the UCR.[7] The study also began to deal with the methodological problems of a survey-based ap-

proach.[8] Additional studies in Washington, D.C., Chicago, and Boston, conducted by the Bureau of Social Science Research and the Survey Research Center of the University of Michigan, provided more detailed data on high-crime areas.[9] These improvements included "careful examination of the manner and sequence in which questions about victimization should be asked, the difficulties arising from memory fading or failure on the part of respondents, the limitations encountered in comparing victim data to police offense statistics, and many of the analytical modes and constraints for presenting the survey findings."[10]

Many victimization surveys have been conducted since these early studies,[11] but the most interesting are those initiated by LEAA and the United States Bureau of the Census in preparation for their National Crime Panel surveys. According to Anthony Turner,

> The National Crime Panel is an omnibus national probability sample of households and businesses which are interviewed to provide estimates of crime victimization and other related crime measures. Interviewing is conducted on a monthly basis by the Bureau of the Census for the Law Enforcement Assistance Administration in several thousand sample units, with each month's interviews constituting an independent, representative subsample of the total. After each six months of interviewing, the sample households and businesses are re-interviewed, again in monthly subsets so that a continuous measurement process is in motion. The National Crime Panel thus provides data for the United States as a whole and sub-national groupings of metropolitan areas by size.[12]

The Law Enforcement Assistance Administration and the Bureau of the Census conducted four pilot studies in Baltimore, Maryland; Dayton, Ohio; San Jose, California; and Washington, D.C.[13] Their findings were incorporated into the National Crime Panel design and into additional sample surveys that were subsequently conducted in other major American cities. Turner summarized the four pilot studies as follows:

> (a) Types of crime events that have been validated as suitably subject to measurement via a victim survey are, in quasi-legal terms, armed robbery, strong-arm robbery, purse-snatch with force, assault with or without physical injury, rape, forcible entry burglary, unlawful entry burglary, auto theft, larceny of household goods, and various attempts and combinations of the above.

(b) More importantly, the detailed data are collectable in surveys of the type being conducted by LEAA-Census in such a way as to permit analytical categorization of anti-social events into a behavioristic context. For example, incidents are aggregated into groupings such as 'crimes directed against persons for economic gain,' 'crimes directed against establishments for economic gain,' 'violent stranger-to-stranger crimes,' etc.

(c) The manner in which questions are phrased and administered to respondents is one of the most critical features of the survey design. The correct choice of wording is as crucial in surveys of victimization as in surveys focused on other social phenomena (unemployment, chronic illness, for example). This is so because people (victims) do not think of their experiences with crime in terms of offenses, such as robbery or aggravated assault (indeed they generally do not know the accepted meaning of such terms). Instead, they experience life-events—being held up in an alley, being slugged in the mouth by an irate acquaintance—and tend to remember such events in that context. The extensive developmental work of LEAA-Census in this aspect of victim surveys has culminated in the present version of the questionnaire being used for the National Crime Panel surveys. Moreover, it has been found that the optimum questionnaire design is one in which a set of fairly specific 'screening' items is administered to elicit all incidents which may have befallen a respondent before asking the relevant details about any incident mentioned.

(d) Victims do not remember all victim-events with equal clarity or completeness. The optimum recall period for capturing victim-derived information depends upon the number of sample interviews which the survey practitioner can afford to conduct, balanced against the expected biases due to respondent memory failure. In general, for measuring very specific victimization experiences, a reference period of longer than 12 months is not very reliable. For continuous or periodic surveys of the same individuals for trend analysis, a 6-month reference is preferable to one of 12 months—both from the standpoint of more timely data and from the standpoint of more reliable data. Twelve-month reference periods are useful for measures of change in evaluating crime programs if conducted two

or more years apart; in this instance, less reliable measures of absolute level in the amount of crime that they accrue from using the longer recall period (12 instead of 6 months) affect the pre and post surveys in the same way so that the measure of *change* is unaffected and therefore valid.

(e) In a household survey, the procedure of having each person respond for himself produced more accurate assessment of victim-events than the practice of allowing a single household respondent to provide information on victimization for all household members.

(f) Better response and accuracy is obtained through personal visit interviews (at least as an initial contact) than through telephone or mail surveys.

(g) In commercial surveys, the general lack of written records maintained by establishments precludes the measurement of crimes such as vandalism, through reliance on their administrative files. Furthermore, businessmen are not usually able to differentiate losses due to shoplifting from those due to employee theft, and frequently these two crimes are not separable from general inventory shrinkage, making victim survey-derived measures thereby impossible.[14]

The LEAA National Crime Panel and local area surveys comprise the most comprehensive attempts ever undertaken in this nation to measure crime. Although these efforts generated a wealth of data, the LEAA-Census method is not perfect, and it is very expensive. The LEAA-Census victimization surveys attempt to measure only certain types of crime. Although the range of crimes included in the LEAA-Census surveys ultimately may be expanded, a survey approach is not appropriate in all situations. Survey methods have not proved useful in measuring the occurrence of homicide, prostitution, gambling, and other crimes where the victim is unable or unwilling to acknowledge involvement. Survey respondents, on the other hand, have proved generally willing to report crimes such as rape, robbery, assault, personal thefts, property thefts, and burglaries. Survey respondents are also more willing to talk about certain crimes (such as assaults by strangers) than about other types (such as assaults by relatives). In addition, the victimization surveys must interview a large number of persons, because crime is a relatively rare event at the individual level. Large samples, however, are very expensive.

Inflation and the rising cost of energy have conspired to increase greatly the cost of face-to-face surveys such as those used by LEAA-Census. At the same time, government units at all levels, academic researchers, and public interest groups are finding it more and more difficult to acquire the financial resources needed to support their data collection needs. Criminal justice researchers concerned about the victims of crime are especially hard hit, because crime victimization surveys require large samples in order to locate a representative proportion of victims. LEAA spent about $500,000 for each of its city surveys.

In light of these financial pressures, it is surprising to find that face-to-face interviewing is often used without serious consideration of its cost effectiveness—even in situations where personal interviewing is not necessarily required. Researchers often insist on the personal interview because they assume it to be the *only* way to achieve an accurate and representative sample.[15] Under conditions of financial exigency, however, survey consumers cannot afford to let personal interviewing stand as a sacred cow. Other options exist, and their potential and cost effectiveness must be reexamined carefully and continually. Indeed the research reported herein tests the contention that random digit dialing telephone surveys are an efficient, accurate, and inexpensive alternative to most personal interviewing applications, particularly in the area of crime victimization surveys.

Notes

1. Apparently the first methodological study of RDD is "Random Sampling by Telephone: An Improved Method," by Sanford Cooper, in the *Journal of Marketing Research* 4, November 1964, 45-48. Although a number of commercial marketing research firms use a form of RDD, a 1973 article reports that only 3 of the 17 academic survey research organizations responding to the editor's poll had ever used RDD. See Mary Spaeth, "Interviewing in Telephone Surveys," *Survey Research* 5, January 1973, 9-13.
2. Anthony G. Turner, "Victimization Surveying: Its History, Uses, and Limitations," in U.S. Department of Justice, LEAA National Advisory Commission on Criminal Justice Standards and Goals, *Criminal Justice System*, (Washington, D.C.: U.S. Government Printing Office, 1973), Appendix A.
3. Presidential Commission on Law Enforcement and Administration of Justice, *The Challenge of Crime in a Free Society* (New York: E. P. Dutton and Co., 1968), 107.

4. *Ibid.*, 108-09.
5. *Ibid.*, 112-13.
6. For example see *ibid.* and LEAA National Criminal Justice Information and Statistics Service, *Criminal Victimization Surveys in the Nation's Five Largest Cities*, (Washington, D.C.: U.S. Government Printing Office, April 1975). Strictly speaking, the UCR and LEAA-Census rates are not directly comparable. The UCR rates are based on reports *to the police of incidents* in a community, whereas the LEAA-Census rates are based on the reports to surveyors *by victims* of all the crimes committed against the victim regardless of where they occurred; that is, inside or outside the community. This is less of a problem at the national level than it is at the community level, but even nationally the fact that the UCR does not receive data from all law enforcement areas causes problems. In addition, the LEAA-Census city rates do not include crimes committed in a city against a resident of another city, visitors, and commuters. The UCR rates will include such a crime if it is reported to the police. Even if these discrepancies were corrected, however, the LEAA-Census technique still has been proved to measure far more crime than does the UCR method.
7. Report of the Presidential Commission, *The Challenge of Crime*, 97, and Philip H. Ennis, *Criminal Victimization in the United States: A Report of a National Survey*, U.S. Presidential Commission on Law Enforcement and Administration of Justice Field Survey II (Washington, D.C.: U.S. Government Printing Office, 1967).
8. Report of the Presidential Commission, *The Challenge of Crime*, 96-97.
9. For a brief discussion of these studies, see Anthony G. Turner and Richard W. Dodge, "Surveys of Personal and Organizational Victimization," unpublished paper presented at the Symposium on Studies of Public Experience, Knowledge, and Opinion of Crime and Justice, Washington, D.C., March 16-18, 1972.
10. Turner, "Victimization Surveying."
11. *Ibid.*
12. United States Bureau of the Census, "Household Survey of Victims of Crime: Second Pretest (Baltimore, Maryland)," (Demographic Surveys Division, November 30, 1970, unpublished). Law Enforcement Assistance Administration, "The San Jose Methods Test of Known Crime Victims," (Statistics Division Technical Series, Report No. 1, 1971). United States Bureau of the Census, "Victim Recall Pretest (Washington, D.C.)," (Demographic Surveys Division, unpublished).

13. Turner, "Victimization Surveying."
14. *Ibid.*
15. This belief has been propagated and reinforced by many textbooks. Most texts on survey methods do not even discuss telephone approaches. In *Survey Research* (Evanston, Ill.: Northwestern University Press, 1963), Charles Backstrom and Gerald Hursh even go so far as to state, "Never interview by telephone" (p. 138). That may have been good advice in 1963, but conditions have changed dramatically since then. In addition to the wider availability of telephones, there is now the possibility of drawing samples by RDD. Many survey research firms, especially those specializing in marketing studies, have begun to use telephone surveys with some form of RDD. Most of this work, however, is progressing on the basis of expediency, because little serious research has been done on the relative accuracy of RDD.

TABLE 1
MAJOR ADVANTAGES OF THE THREE INTERVIEWING METHODS

ADVANTAGE	METHOD		
	Personal	Mail	Telephone
Inexpensive	no	yes	yes
Random sampling generally feasible	no	no	with RDD
Entire spectrum of the population potentially contactable	yes	no	no
Sampling of special populations	yes	with list	sometimes
Easy to cover large geographic area	no	yes	yes
Control over who is actual respondent	yes	no	yes
High response rate	sometimes	no	yes
Easy call-backs and follow-ups	no	no	yes
Long interviews generally possible	yes	sometimes	sometimes
Explanations and probings possible	yes	no	yes
Visual materials may be presented	yes	yes	no
Nonthreatening to respondent	no	yes	yes
Interviewer can present credentials	yes	yes	no
Safe for interviewers	no	N.A.	yes
Easy supervision of interviewers	no	N.A.	yes

cedures can be used with RDD to select an eligible and desired respondent. Within certain limitations RDD also can be used for surveying special and rare populations. RDD samples, in addition to being much less expensive than the complex samples typically used for face-to-face interviewing, may be even better because clustering is avoided.[4] Moreover, random digit dialing eliminates the listing errors unavoidable in creation of a master sample for personal interviewing.

RDD VERSUS PERSONAL INTERVIEWING: COST COMPARISON

As indicated, the greatest advantage of using the telephone instead of a personal visit is the tremendous cost savings during the sampling and interviewing stages, especially when random digit dialing is employed. RDD eliminates the high cost and expense of compiling the sample of households to be contacted for the personal interview in this way.

With RDD, a fairly simple computer program that can generate a random sample of potential telephone numbers is developed. An experienced programmer can write and test the necessary program in a few hours; moreover, if it is designed properly, the program can be reused for future projects.

For each new RDD survey it is necessary only to identify the working telephone exchanges in the geographic area to be covered and to supply these exchanges to the computer program.[5] If the phone numbers generated are punched onto computer cards (an easy way to handle them), only a small amount of additional clerical time is required to run the cards through an interpreting keypunch.

Of course, many of the randomly generated phone numbers either are not working numbers or are business numbers; hence some of the telephone interviewer's time is lost. Also, because telephone exchanges usually include some numbers attached to locations outside the area under investigation, it is necessary to screen out geographically ineligible households. Although an interviewer may have to dial several times before reaching an eligible household, this repeated dialing adds only a few minutes per completed interview to the research time. With touch-tone telephones it takes only 20 to 30 seconds to dial and discover whether the number yields an eligible household, or is not in service. Even if seven or eight calls are required to reach an eligible household, the interviewer has wasted only three or four minutes. In personal interviewing, by contrast, it may take ten times as long to locate a correct dwelling unit.

Drawing up the RDD sample for the survey covered in this report cost $35 in staff time, plus $10 for computer time to prepare the sample phone numbers. Thus, the sampling costs for 800 households completely interviewed came to only six cents per household. A com-

parable sample drawn for traditional personal interviewing would have cost at least several dollars per household.

Interviewing costs for RDD surveys are also quite low. For example, in the crime victimization survey carried out for this study, all persons 12 years old or older in each household were canvassed. The amount of time spent with an individual fluctuated widely, since it depended upon the number of crime victimizations experienced by the respondents during the past year. Overall, the survey of one entire household usually took an hour. The average cost for the interviewers' and supervisors' time came to $3.85 per household or $1.83 per person (at rates of $3.00 per hour for the interviewers and $4.00 per hour for the supervisors). Although directly comparable unit costs for personal interview surveys were not available for this study, those familiar with conducting them will be aware that they are substantially higher.

The best way to illustrate the cost effectiveness of RDD is to compare, major category by major category, the costs of conducting a crime victimization survey using personal interviewing to the costs of conducting a comparable survey using RDD. Table 2 presents such a comparison.

TABLE 2
COST COMPARISON OF PERSONAL INTERVIEWING AND RDD FOR 1,000 HOUSEHOLDS

STUDY COMPONENT	COST	
	Personal Interviewing Method	RDD Method

survey using RDD.

The total cost of the 1,000-household RDD survey is therefore only 29 percent of that of the comparable personal survey. Cost savings accrue through the use of RDD because of savings in the sampling and data collection phase (sampling, interviewing, coding, and keypunching) of the survey project. The project planning, questionnaire design, analysis, and report writing phases of a survey project should have similar costs for both RDD and personal interviewing because design and analysis costs are relatively inelastic with regard to sample size.

Because data collection and sampling costs are highly elastic to sample size, however, RDD will provide even larger cost savings if sample sizes are increased beyond the 1,000 households used in the foregoing example (as was the case in the RDD surveys on crime victimization reported herein, when the RDD cost was only 20 to 25 percent of that of the comparable personal interview surveys). If the sample is smaller than 1,000 households, the percentage savings will decrease.

POTENTIAL BIASES IN A TELEPHONE SURVEY

Financial savings are meaningless, however, if the resulting sample and tabulations are seriously biased. Many professionals and laymen are wary of *any* telephone survey. When telephones were less ubiquitous, this fear was warranted. Telephone subscriptions in the United States have grown, however, so that more than 90 percent of the households can now be reached by telephone (see Table 3). Of course, the remaining 10 percent could possibly be a distinctive subgroup and their omis-

TABLE 4

EHOLDS WITH A TELEPHONE AVAILABLE VERSUS ALL HOUSEHOLDS, AND IN POLICE DISTRICT NO. 1, CINCINNATI, 1970

PERCENTAGE					
Citywide			Police District No. 1		
one	All	Difference	Telephone	All	Difference
8	75.6	+2.2	44.2	46.2	-2.0
2	24.4	-2.2	55.8	53.8	+2.0
7	40.7	+4.0	11.8	8.3	+3.5
3	59.3	-4.0	88.2	91.7	-3.5

urth Count Tabulations (on magnetic tape).

		1965
1970	86.7	U.S. Census, Public Use Sample, April 1970
1976	92.8[a]	LEAA, National Crime Panel, January 1976

[a]90.4 percent had telephones in the housing unit; the remainder reported the available telephone was located outside the housing unit.

sion could result in biased samples; but the evidence to be presented in Chapter 4 indicates that this is generally not the case when the sampling is done by random digit dialing.[7]

Data from the 1970 Census can give an indication of the biases to be expected on demographic characteristics. The Census Bureau inquired whether a telephone was "available" to the household. This question was meant to include any telephone on which the occupants could receive calls, whether the telephone was located in the housing unit or elsewhere. Because RDD is suitable for reaching only those persons with a telephone actually in their housing unit, the Census Bureau definition of "telephone available" is broader than desirable for a strict comparison. Nevertheless, the proportion of households with telephones available "elsewhere" is so small that the Census data can still be very instructive.[8]

In preparing summary tables at the city and tract level, the Census Bureau cross-tabulated telephone availability only with household race and housing tenure. Table 4 reproduces these figures for the entire city of Cincinnati and for Cincinnati's P.D. No. 1 (the low-income neighborhood to which special attention was given in the RDD survey).

As might be expected, black households, which tend to be less able to afford telephones, would be slightly underrepresented in a citywide telephone survey (assuming a perfectly representative sample). Surprisingly, though, blacks would probably be slightly overrepresented in a telephone survey of P.D. No. 1 because that area happens to contain an unusually high proportion of Appalachian whites—a group which has been found to be even less likely than blacks to have telephone service.[9] Although these differences in representation exist, they are very small for most practical purposes. The differences on housing tenure are larger and suggest a potential threat of underrepresentation of apartment dwellers.

National data can yield even more insights into the demographic biases associated with telephone availability. Using the Census Bureau's 1970 Public Use Sample allows more control over the variables that can be tabulated. The results appear in Table 5. Household income and race are associated with telephone availability, although the degree of potential bias is not so severe as might have been feared. Neither age nor sex shows important differences, but the educational level of the head of household does show some bias toward the better educated. The greatest disparity remains in housing tenure.

The Public Use Sample also allows an examination of individual-level characteristics; these results appear in Table 6. The underrepresentation of black persons is about the same as for households, and again no important differences show up with regard to sex and age. The education bias among all persons, however, is less pronounced than for household heads.

$15,000–$24,999	14.6	13.0	+1.6
$25,000 or more	04.2	03.7	+0.5
Race of Head			
White and other	91.8	89.7	+2.1
Black	08.2	10.3	-2.1
Age of Head			
Under 25	05.5	07.1	-1.6
25–34	17.9	18.2	-0.3
35–49	30.3	29.1	+1.2
50–64	27.2	26.6	+0.6
65 or older	19.2	19.2	0.0
Sex of Head			
Male	79.8	78.8	+1.0
Female	20.2	21.2	-1.0
Education of Head			
0–8 years	24.1	26.8	-2.7
9–12 years	47.4	47.1	+0.3
More than 12 years	28.5	26.1	+2.4
Housing Tenure			
Own	67.4	63.0	+4.4
Rent	32.6	37.0	-4.4

SOURCE: 1970 U.S. Census, 1 in 10,000 Public Use Sample tape.

TABLE 6
CHARACTERISTICS OF PERSONS 12 YEARS OLD AND OLDER WITH A TELEPHONE AVAILABLE VERSUS ALL PERSONS NATIONWIDE, 1970

PERSONAL CHARACTERISTICS	PERCENTAGE		
	Telephone	All	Difference
Race			
White and other	91.5	89.5	+2.0
Black	8.5	10.5	-2.0
Sex			
Male	47.5	47.9	-0.4
Female	52.5	52.1	+0.4
Age			
12–15	10.9	10.5	+0.4
16–19	8.6	9.5	-0.9
20–34	24.0	25.5	-1.5
35–49	24.3	23.0	+1.3
50–64	19.7	18.8	+0.9
65 and older	12.4	12.6	-0.2
Education Completed			
0–8 years	24.2	26.2	-2.0
9–12 years	53.1	51.9	+1.2
More than 12 years	22.7	22.0	+0.7

Race of Head			
White and other	90.8	89.5	+1.2
Black	9.2	10.4	-1.2
Age of Head			
Under 25	8.1	9.8	-1.7
26–34	20.7	20.9	-0.2
35–49	25.8	24.9	+0.9
50–64	25.6	24.7	+0.9
65 or older	19.8	19.6	+0.2
Sex of Head			
Male	75.9	75.3	+0.6
Female	24.1	24.7	-0.6
Education of Head			
0–8 years	19.5	21.0	-1.5
9–12 years	45.1	45.4	-0.3
More than 12 years	35.4	33.6	+1.8
Housing Tenure			
Own	67.8	64.1	+3.7
Rent or no cash rent	32.2	35.9	-3.7
Persons Aged 12+ in Household			
Mean	2.31	2.27	+.04
Personal Incidents in Household			
Mean	.143	.144	-.001
Household Incidents in Household			
Mean	.124	.129	-.005

SOURCE: LEAA National Crime Survey, January 1976 panel (based on 10,043 interviewed households).

TABLE 8
CHARACTERISTICS OF PERSONS WITH A TELEPHONE AVAILABLE VERSUS ALL HOUSEHOLDS NATIONWIDE, 1976

PERSONAL CHARACTERISTIC	PERCENTAGE		
	Telephone	Total	Difference
Race			
White and other	90.5	89.4	+1.1
Black	9.5	10.6	-1.1
Sex			
Male	47.4	47.6	-0.2
Female	52.6	52.4	+0.2
Age			
12–17	14.8	14.7	+0.1
18–24	14.2	15.5	-1.3
25–34	18.2	18.3	-0.1
35–49	20.8	20.2	+0.6
50–64	19.1	18.6	+0.5
65 or older	13.0	12.8	+0.2
Education Completed			
0–8 years	22.2	23.3	-1.1
9–12 years	49.5	49.6	-0.1
More than 12 years	28.2	27.1	+1.1

(approximately)	-10.5	-4.6	-4.0
Race of Head			
Nonwhite	-2.9	-2.2	-1.1
Age of Head			
Under 25	-1.6	-1.4	-1.7
25–34	-0.1	-0.3	-0.2
35–49	{+3.0}	+1.4	+0.9
50–64		+0.4	+0.9
65 or older	-1.2	0.0	+0.2
Sex of Head			
Male	+1.1	+1.0	+0.6
Education of Head			
Elementary (0–8)	N.A.	+2.7	-1.5
Housing Tenure			
Own	N.A.	+4.4	+3.7

viewing would have encountered serious problems in reaching a representative sample in 1960 and earlier, but the present sampling situation is very encouraging and bound to improve further.

Notes

1. The 1960 figure is based on the March 1960 Current Population Survey as reported in United States Bureau of the Census, *Current*

Population Reports, Series P-20, No. 11 (Washington, D.C.: August 2, 1961). The 1970 figure is computed from the Public Use Sample (1:10,000) of the 1970 Census. The 1976 figure is derived from the January 1976 panel of the LEAA National Crime Survey which included 10,043 households. Telephone industry statistics yield estimates higher by about five percentage points, but they admit that their figures are on the high side (in some localities their estimates exceed 100 percent). The Census and survey data reported here are more accurate, although possibly a little low.

2. Telephone books and specially compiled lists of telephone numbers systematically exclude all households with unlisted numbers. These numbers may have been unlisted either because the owners requested unlisted numbers (voluntarily) or because the telephones were installed after publication of the telephone books or special listing (involuntarily). In April 1974 (with the telephone book based on June 1973 listings), 28 percent of the telephone numbers in Cincinnati were unlisted: 18 percent voluntarily and 10 percent involuntarily. The households with unlisted telephones are significantly different from households with listed telephones. See Alfred J. Tuchfarber, Jr., "Random Digit Dialing: A Test of Accuracy and Efficiency," unpublished doctoral dissertation, University of Cincinnati, 1974, available through University Microfilms, Ann Arbor, Michigan.
3. Space does not permit a complete description of the details of traditional area probability sampling, but a brief summary follows. In a nationwide survey, the first step is to draw a sample of counties. A certain number of counties containing the largest metropolitan areas is automatically included. Others are randomly selected with the chance of being drawn determined by the county's population. To ensure that selection is representative, the counties are usually grouped by geographic location and/or degree of urbaniza-

4. When the unit of analysis is the individual rather than the household, RDD yields only randomly selected clusters of individuals—that is, the household members. To produce a true probability sample of individuals, one would need to enumerate the household and select the respondent from a random table, the usual procedure in face-to-face interviews.
5. The comments here about the ease of programming apply to a situation where pure random sampling is being employed, generally the case in a city or small area survey. If special features are desired, such as stratification, the programming would be more complicated.
6. This survey was conducted as part of the evaluation of the Community Sector Team Policing Project in 1973. The cost figures have been adjusted to reflect 1976 prices through the use of the United States Cost of Living Index. The data on the cost of the survey were obtained from Alfred Schwartz of The Urban Institute, the evaluation project manager. Because the planning and analysis phases of the survey were too closely interwoven with other parts of the evaluation project to be extracted, cost data were available only for the sampling and data collection phase of the survey. Thus the planning and analysis phase costs were estimated to be the same as those of the RDD survey.
7. It is true that households without telephones (10 percent) are completely excluded from the sample in RDD. Yet there are no inaccurate listings or missed households in RDD as there are with the type of sample associated with traditional survey methods. For example, it is not hard to imagine more than a few ghetto households being missed in the typical master sample. It is difficult to say which technique completely excludes more households from any possibility of inclusion in the actual survey sample, but all sampling techniques in practice exclude some households completely.

8. According to the LEAA National Crime Panel survey of January 1976 only 2.7 percent of the households with a "telephone available" report that the telephone was located "elsewhere," outside the housing unit. How precisely this figure applies to the 1970 Census is unknown, but the proportion is small.
9. We have seen other survey data from P.D. No. 1 which indicate that Appalachians are much less likely to have telephone service than are blacks. Thus one should not interpret Table 3 as providing evidence about black neighborhoods in general.

OUTLINING THE PROBLEMS

Census data do show some areas of potential bias in telephone surveys, but the size of these discrepancies seems small. Of course, an actual RDD survey may encounter other practical problems, such as differential cooperation, which may lead to an end product that is either more or less representative than the census data suggest. This problem is inherent in any survey. Even traditional personal interviewing surveys have difficulty reaching certain segments of the population. (The United States Census is not a perfect enumeration.) In addition, demographic accuracy is not the only important feature of an RDD survey. The reliability of the substantive content is most important; in many ways the accuracy of the substantive questions is the critical part of a survey. Thus, if respondents are less willing to talk over the telephone about crime victimizations or attitudes toward the criminal justice system, then an RDD survey would not be suitable for these topics.

The ultimate test of random digit dialing would be to perform an RDD survey on a population with known characteristics of demography, behavior, and attitude. In the absence of the opportunity to do this, the second best test would be a replication of a well-done personal interviewing survey. Then the question would be, does RDD produce results as good as those produced by the best traditional method? If the results turn out to be the same, RDD could be considered superior because it is a simpler and less expensive method—even if its results contained some of the same errors and inaccuracies of the tra-

ditional method. Should RDD produce significantly different results, a judgment would be necessary as to which technique had produced better results. Indeed, respondents might be more willing to discuss victimization experiences over the telephone than with a personal interviewer because the physical absence of the interviewer makes the situation less threatening. If this proves to be true, then a telephone survey could be expected to yield higher estimates of crime.

Fortunately, a good opportunity existed for comparing RDD to a traditional personal interview survey. LEAA had commissioned the United States Census Bureau to conduct a crime victimization survey in Cincinnati during February and March 1974; this survey was one of the series of surveys being done in 26 large cities as part of the National Crime Survey program. In Cincinnati, 19,903 persons 12 years old and older from 9,708 households were asked about crimes committed against themselves or the household during the previous 12 months. The households were selected according to traditional, complex sampling procedures. The interviewers made their initial contacts in person and interviewed as many persons as possible during the first visit. If some member of the household was not present or if additional information was needed, the interviewers were allowed to call back and complete the questioning either in person or by telephone. (The call-backs were made by telephone in 22 percent of the cases.)

With financial support from the Police Foundation, the staff and facilities of the Ohio Institute for Public Opinion were employed to ask the same questions by telephone to a sample of Cincinnati households—a sample, of course, selected by random digit dialing. Interviewing took

family owned or rented the dwelling. The RDD interviewers also inquired about the address and the number of telephone lines serving the household, and asked whether the telephone number was unlisted.

A second series of questions sought information about crimes that may have been committed against the respondent during the previous 12 months. Again, the household respondent was asked to report about crimes committed against the property of the household (unlawful entry and/or theft of property not kept on the person, such as a TV, car, or bicycle). When any crimes, threats, or potential crimes were reported, the respondents were asked to give detailed information including facts about what happened, the monetary value of any losses, the seriousness of any injuries, and some descriptive information about the criminals (if known). These details were used to classify the nature and seriousness of the crime.

While the demographic and crime questions dealt with factual information, some opinions were also solicited about crime-related matters, as, for example, whether the respondent believed the local police were doing a good job and whether crime appeared to be increasing or decreasing. Opinion questions, of course, have neither a right nor a wrong answer but merely register the respondent's feelings at the moment.

The primary RDD research objective was to compare the responses from the LEAA-Census survey and RDD survey on these three types of questions to see whether the two survey techniques yielded the same information. If the data collected with an RDD survey agreed with the LEAA-Census data collected primarily by personal interviewing (within

the limitation of statistical sampling error), then RDD can be said to be at least as accurate as personal interviewing.

LIMITATIONS OF THE TEST

This test, of course, has some limitations which need to be specified here. The greatest limitation is that the standard of comparison is not perfect. The Census Bureau presumably used the best possible procedures to ensure the validity and accuracy of its survey. Yet, because its complex sampling procedures still manage to miss a very small proportion of the population, its results are not infallible. Although little is known about this unsampled segment, most survey researchers consider this loss to be small and unimportant for a survey of the general populace. RDD also certainly misses some of the hard-to-locate group, in addition to excluding *systematically* households without telephone service. The question is whether RDD's omission of nontelephone households seriously biases the sample relative to the best known alternative sampling method.

The RDD study also pits the medium of telephone interviewing against that of personal interviewing. This is a separate aspect of the survey method which may have its own influence upon the quality of the data. The small quantity of literature comparing telephone and personal interviewing does not show either form to be clearly nor consistently superior. This study does not represent a definitive test either. Rather, the combined effect produced by random digit dialing *with* telephone interviewing is compared to the effect of traditional complex sampling combined *with* personal interviewing. Although these com-

in social composition from all households in the population, then an RDD survey would yield seriously erroneous estimates of population characteristics.

Two factors work against such an outcome. One is that there are now so few nontelephone households that biases are likely to have little noticeable effect upon a survey of the general populace. A corollary to this postulate is that persons without telephones are the same ones who are hard to locate in a personal interview survey. Thus, both techniques are probably biased in the same direction. The problem, then, is to discover whether RDD is *more seriously* biased.

The second factor is that nontelephone households are not concentrated in one segment of society. If only the poor, or only blacks, or only the less educated lacked telephone service, then the Census Bureau data presented earlier would have looked very different. In fact, the vast majority of poor people, blacks, and less educated people do have telephone service. However, even among the wealthiest and most advantaged segments of society there are families with no telephone service either by choice or because of temporary circumstances. The available data do show some demographically related differences, but these are small for most practical purposes. Thus, although some minor disparities can be anticipated, this hypothesis can be framed in terms of "no difference."

HYPOTHESIS 2: RDD will measure as much crime as the LEAA-Census method or more, and there will be no statistically significant differences between the two surveys on the estimates of seriousness of the crimes.

Because crime victimization is highly correlated with social class and place of residence, a demographically biased survey would be expected to yield inaccurate victimization data, too. The authors expect that the demographic biases, if any, will be too weak to distort the victimization data. There might, however, be some differences due to the interview medium. For instance, if respondents are more comfortable talking about crime over the telephone, then they might be more willing to report their victimizations to a telephone interviewer. Several studies have shown that sensitive information, such as contraceptive use and drinking patterns, can be reliably measured over the telephone.[1] A priori, the hypothesis that the RDD method will measure as much crime as, or more crime than, the LEAA-Census method has been established satisfactorily.

If the data support Hypothesis 1 but not Hypothesis 2, such a finding would be important evidence in deciding which technique to use for future victimization surveys. The primary reason for conducting a victimization survey is to obtain a more accurate estimate of crime rates than is available from police records. The LEAA-Census surveys still do not measure the actual crime rate, because some of their respondents are uncooperative or forgetful. Lower RDD-measured rates would indicate that RDD does not elicit enough cooperation on this topic. In that event, it would be a poor alternative. On the other hand, higher RDD rates would indicate that this method is more effective for measuring victimizations. Naturally, if the two surveys should yield essentially the same rates, then the rate of crime measured would not be a relevant criterion for choosing between the two techniques.

items as well as on personal items is comparable to that collected in person." See "Interviews by Telephone and in Person: Quality of Responses and Field Performance," *Public Opinion Quarterly* 40, Spring 1976, 51-65.

3. These are not the surveys that were used in the cost comparison presented earlier in this chapter. It was impossible to compare the costs of these two surveys for a number of reasons including the following: (1) The sample sizes were dramatically different. (2) All of the design and analysis of the LEAA-Census survey was conducted in Washington, D.C., as part of a larger project. (3) Some of the coding and all of the keypunching were done in Washington, D.C., as part of a larger operation.

CHAPTER 4

COMPARING THE SURVEY TEST RESULTS

In this chapter, personal interview data from the LEAA-sponsored victimization survey conducted by the Census Bureau in Cincinnati during February and March 1974 will be compared with data acquired by the RDD survey of crime victimization in Cincinnati during April 1974. The questions covered demographic characteristics, crime victimizations, and opinions on crime-related matters.

DEMOGRAPHIC CHARACTERISTICS

It is important first to ascertain whether the two survey methods reach the same population of potential respondents. It has been acknowledged that the telephone systematically excludes all persons who are not subscribers, but personal interviewing also misses certain seg-

TABLE 10
HOUSEHOLD DEMOGRAPHIC CHARACTERISTI
RDD AND LEAA CITYWIDE SAMPLES

HOUSEHOLD CHARACTERISTIC	PERCENTAGE	
	RDD	LEAA
Household Income		
Less than $3,000	20.2	21.7
$3,000–$7,499	27.1	28.0
$7,500–$9,999	10.1	13.1
$10,000–$14,999	25.7	21.0
$15,000–$24,999	13.1	12.2
$25,000 or more	3.9	4.0
	(N = 635)	(N = 8,756)
Race of Head		
White and other	74.7	73.3
Black	25.3	26.7
	(N = 800)	(N = 9,708)
Age of Head		
Under 25	13.9	14.7
25–34	22.0	18.4
35–49	18.4	20.0
50–64	23.9	24.1
65 and older	21.8	22.7
	(N = 799)	(N = 9,708)

	64.7 35.3 (N = 800)	64.1 35.9 (N = 9,708)	-0.6 +0.6	χ^2 = .09 df = 1 n.s. at .01
	20.7 44.1 35.2 (N = 795)	25.1 45.2 29.7 (N = 9,609)	-4.4 -1.1 +5.5	χ^2 = 52.87 df = 2 SIGNIFICANT at .01
	39.9 60.1 (N = 800)	39.1 60.9 (N = 9,708)	+0.8 -0.8	χ^2 = .19 df = 1 n.s. at .01
	2.07 (N = 800)	2.05 (N = 9,708)	+.02	Standard error of diff = .04 n.s. at .01

TABLE 11
PERSONAL DEMOGRAPHIC CHARACTERIST
RDD AND LEAA CITYWIDE SAMPLES

PERSONAL CHARACTERISTIC	PERCENTAGE		
	RDD	LEAA	Di
Race			
White and other	74.5	72.5	
Black	25.5	27.5	
	(N = 1,652)	(N = 19,903)	
Sex			
Male	43.0	44.6	
Female	57.0	55.4	
	(N = 1,655)	(N = 19,903)	
Age			
12–15	8.1	8.7	
16–19	9.3	10.0	
20–24	14.4	14.5	
25–34	19.3	15.7	
35–49	16.0	16.6	
50–64	17.8	18.7	
65 or older	15.1	15.8	
	(N = 1,655)	(N = 19,903)	
Education Completed			
0–8 years	22.0	26.3	
9–12 years	48.7	49.4	
More than 12 years	29.3	24.3	
	(N = 1,629)	(N = 19,881)	

below $3,000 had access to a telephone as compared to 87 percent of all American households. (Table 5 also showed the degree to which a telephone survey could potentially underrepresent the poor.) These figures are, of course, four years older than the RDD survey data, and they do not reveal how Cincinnati may differ from the entire country. The Census data do, however, provide some indication of the nature of possible income bias.

Table 10 shows that an income bias does not seem to be present. The distribution of household income in the two surveys is very similar; the differences are not statistically significant. The RDD sample has a slightly lower proportion of respondents in the lowest income group (under $3,000), but the difference is so small that it is impossible to know whether it is a systematic bias or a chance occurrence resulting from sampling variation. This finding indicates that if nontelephone households are indeed predominantly low-income families, their numbers are so small that excluding them does not produce an important distortion in an RDD sample drawn from a heterogeneous geographic area.

Race of household head, age of head, and sex of head also do not show statistically significant differences. There is, however, a significant difference with regard to education of head. Although it is logical to expect a bias in favor of better educated households, the difference here is fairly large. Given the nonsignificant differences on the other variables, it is difficult to interpret this finding.

Two other family characteristics that can be tested are housing tenure and family size. Tenancy status refers to whether the family's

reasons. First, an education bias could be expected to be accompanied by biases on other demographic variables, but such biases did not appear. Second, the national level data (Tables 5 and 7) did not indicate that an educational bias was to be expected. Thus the differences on the education distributions pose something of a mystery.

Eleven demographic characteristics now have been tested for statistical significance. Although a significance level of .01 was employed for each individual test, the 11 tests are not independent. The real probability that one of the 11 would turn out significant is .11.[1] Because this is a somewhat high probability, there may be no education bias after all. The observed differences on education may be primarily the result of chance sampling variation, as evidence presented later indicates.

To explore further the nature of the RDD survey's educational bias, controls for additional variables can be introduced. Educational attainment is highly associated with age, because of changes in the normal school-leaving age during certain historical periods. In addition, the RDD sample includes teenagers who are likely to be still in school. Table 12 shows the differences on educational attainment between the two surveys within four age groups. Although the degree of bias varies among age groups, age does not seem to be an intervening variable that would explain the apparent bias.

A similar control for race may be instructive, because whites tend to have more education than blacks. This control appears in Table 13. Here the discrepancy remains for whites but there is no statistically significant difference for blacks. This finding suggests that an accurate educational representation may be more difficult to achieve among whites,

TABLE 12

I COMPLETED, CONTROLLING FOR AGE FOR ALL PERSONS
RDD AND LEAA CITYWIDE SAMPLES

IN	PERCENTAGE			SIGNIFICANCE
	RDD	LEAA	Difference	
	42.6	42.1	+0.5	$\chi^2 = 9.82$
	49.7	53.7	-4.0	df = 2
	7.7	4.2	+3.5	SIGNIFICANT
	(N = 281)	(N = 3,678)		at .01
	5.5	5.8	-0.3	$\chi^2 = 14.41$
	42.2	50.1	-7.9	df = 2
	52.3	44.1	+8.2	SIGNIFICANT
	(N = 550)	(N = 5,906)		at .01
	19.3	24.7	-5.4	$\chi^2 = 10.89$
	54.3	53.4	+0.9	df = 2
	26.4	21.9	+4.5	SIGNIFICANT
	(N = 545)	(N = 6,974)		at .01
	41.1	50.0	-8.9	$\chi^2 = 20.95$
	31.5	34.2	-2.7	df = 2
	27.4	15.8	+11.6	SIGNIFICANT
	(N = 238)	(N = 3,117)		at .01

TABLE 13
YEARS OF EDUCATION COMPLETED, CONTROLLING FOR RAC
RDD AND LEAA CITYWIDE SAMPLES

YEARS OF EDUCATION	PERCENTAGE	
	RDD	LEAA
White and Other		
0–8 years	18.2	23.5
9–12 years	43.7	47.0
More than 12 years	38.1	29.5
	(N = 1,210)	(N = 14,305)
Black		
0–8 years	33.6	33.7
9–12 years	52.7	55.8
More than 12 years	13.7	10.5
	(N = 412)	(N = 5,370)

but it does not help to explain why the total RDD sample shows a larger than expected proportion of more highly educated persons.

Finally, the relationships should be examined for a few other combinations of demographic variables. One important combination to check is race and sex, because there has been much evidence that surveys tend to underrepresent black males. How does RDD compare to the LEAA-Census effort on this problem? The answer is in Table 14, the top two sections of which show that the RDD survey did slightly underrepresent black males in comparison to the LEAA-Census survey. Still, this discrepancy is very small and not statistically significant.

Curiously, as the third section of Table 14 shows, the slightly lower proportion of black males is not a problem in the 18- to 30-year-old age bracket. Measurement of black males of this age was originally anticipated to be a problem, because this group is less likely to have a settled lifestyle. As it turned out, the greatest differences measured in the two surveys occurred among those younger than 18 (41.5 percent versus 49.2 percent males) and those older than 55 (38.7 percent versus 44.6 percent males). The small number of blacks in the RDD sample, however, precludes drawing definitive conclusions about this relationship.

Table 15 portrays a final set of demographic comparisons. Here, income distribution is depicted according to race. The pattern of differences for the white group is as expected—those with lower incomes are slightly underrepresented. Surprisingly though, the poorest group of blacks is slightly overrepresented in the RDD sample, relative to the LEAA-Census sample. These differences, however, are not statistically

TABLE 14
SEX, CONTROLLING FOR RACE FOR ALL PERS
RDD AND LEAA CITYWIDE SAMPLES

SEX	PERCENTAGE		
	RDD	LEAA	Difference
White and Other			
Male	44.2	45.5	-1.3
Female	55.8	54.5	+1.3
	(N = 1,228)	(N = 14,456)	
Black (All)			
Male	39.4	42.7	-3.3
Female	60.6	57.3	+3.3
	(N = 421)	(N = 5,447)	
Blacks, 12–17			
Male	41.6	49.2	-7.6
Female	58.4	50.8	+7.6
	(N = 82)	(N = 1,041)	
Blacks, 18–30			
Male	40.4	40.8	-0.4
Female	59.6	59.2	+0.4
	(N = 109)	(N = 1,460)	
Blacks, 31–55			
Male	39.4	39.2	+0.2
Female	60.6	60.8	-0.2
	(N = 127)	(N = 1,784)	
Blacks, 55 and Older			
Male	38.7	44.6	-5.9
Female	61.3	55.4	+5.9
	(N = 93)	(N = 1,161)	

tempted crimes are counted as well as those actually completed. As Table 16 shows, the respondents in the RDD survey reported 39 percent more household crimes per 1,000 than those interviewed by the LEAA-Census team (378.8 versus 271.7). Although the magnitude of the differences varies from category to category, the RDD survey shows a substantially higher rate for each of the crime categories.[2] Because all of the RDD victimization rates are higher than those reported by LEAA, the first part of the RDD hypothesis—that the random digit dialing survey would measure as much crime as or more crime than the LEAA-Census method—is automatically satisfied. If any of the RDD rates had been lower, the standard error of the difference would have been employed in a one-tailed test of significance.[3]

Table 17 reports the victimization rates for personal crimes: crimes committed against a person in his or her presence, plus larcenies without contact committed away from the home. Personal crimes include all forms of assault and theft involving contact with the person. Attempts and threats are also counted. Personal larceny without contact is also included in this sector; this crime is the theft of personal property away from the home (such as at work or in a hotel) but without contact between the victim and the thief. Again, the total rate of personal crimes reported by the RDD respondents exceeds the rate reported to the LEAA-Census interviewers (217.0 versus 172.6). The RDD rates were also higher for the three subcategories of robbery, assault, and theft. Because the RDD rates are higher than those found in the LEAA-Census survey, significance tests are not necessary and the first part of the second hypothesis is satisfied.

It is clear that the RDD respondents were reporting more crimes

TABLE 15

HOUSEHOLD INCOME, CONTROLLING FOR RACE OF HEAD F(
RDD AND LEAA CITYWIDE SAMPLES

HOUSEHOLD INCOME	PERCENTAGE	
	RDD	LEAA
White and Other		
Less than $3,000	13.4	16.8
$3,000–$7,499	26.5	26.8
$7,500–$9,999	10.3	13.7
$10,000–$14,999	28.8	23.2
$15,000–$24,999	15.9	14.4
$25,000 or more	5.0	5.1
	(N = 486)	(N = 7,106)
Black		
Less than $3,000	39.8	35.1
$3,000–$7,499	28.8	31.5
$7,500–$9,999	10.7	11.8
$10,000–$14,999	15.7	14.6
$15,000 or more	5.0	7.1
	(N = 156)	(N = 2,602)

TABLE 16
HOUSEHOLD CRIME VICTIMIZATION RATES PER 1,000
RDD AND LEAA CITYWIDE SAMPLES

CRIME	PER 1,000			
	RDD[a]	LEAA[b]	SEdiff[c]	Sig.[d]
Burglary	187.5	143.3	14.4	NTN
Household larceny	148.8	103.4	13.1	NTN
Motor vehicle theft	42.5	25.0	7.4	NTN
Total household crimes	378.8	271.7	18.0	NTN

[a]Based on 303 incidents reported by 800 households.

[b]Based on 2,638 incidents reported by 9,708 households (estimated from weighted data).

[c]Estimated standard error of the difference (used in test of significance).

[d]NTN = no test necessary.

TABLE 17
PERSONAL CRIME VICTIMIZATION RATES PER 1,000
RDD AND LEAA CITYWIDE SAMPLES

CRIME	PER 1,000			
	RDD[a]	LEAA[b]	SEdiff[c]	Sig.[d]

interviewers did all their work from a central location, they were closely monitored, and they were paid by the hour regardless of how many interviews they completed. Thus, it was difficult for them to cheat, and they had no motivation for magnifying the number of crime incidents.

On the other hand, the Census Bureau interviewers went out alone into the field. In this situation they had significant opportunity to modify the prescribed interviewing procedures inappropriately, and situations can be imagined in which they would be so motivated. Households in high-crime areas are not particularly pleasant places to be interviewing strangers. An interviewer uncomfortable in such a location might well ignore some crimes or respondents in order to terminate the interview more quickly. Of course, the Census Bureau was careful to validate the work of its staff. Five percent of the households were reinterviewed by another Census Bureau employee to check the accuracy of the first interview. Despite this deterrent, procedural modification may have been a problem, especially among the inexperienced interviewers that the Census Bureau had to employ to complete its large-scale effort in the time allotted. Because the amount of error detected and the remedies when inaccuracies were found are not known, the Census Bureau staff is not being censured here. But unauthorized interviewing procedures cannot be ruled out as a plausible explanation for at least part of the observed differences in the crime rates. As far as the present study is concerned, telephone interviewing has an advantage for collecting crime victimization information because the interviewers do not have any motivation to treat victimized households any differently from the way they handle those that have not been victimized.

Serious consideration must also be given to the possibility that people are more willing to report crime victimizations over the telephone. Unfortunately, the present data do not permit an independent test of this possibility because the influence of the telephone as a communications medium cannot be distinguished from what may have been imperfect work on the part of the Census Bureau interviewers. Yet, in a very practical sense, it is not important for purposes of this study to know the precise source of the observed differences. What is important is that the RDD method yielded higher crime rates that cannot be attributed to sampling biases nor any form of overreporting. This evidence indicates that RDD may be a preferable technique for collecting crime victimization data. As noted in Chapter 1, the scientifically sound method that detects more crime is probably the more accurate method.

Although respondents reported crime to the RDD interviewers at a higher rate, they were not simply reporting a larger number of trivial crimes. This is evident from Tables 16 and 17, in which the rates from the RDD survey are higher for almost every type of crime. Another way to check these data is by computing the Sellin-Wolfgang Severity Code —a guide to the relative severity of various crimes—for each crime.[4] Table 18 shows frequency distribution of the severity codes from the two surveys. A low code indicates a minor crime, while a high code indicates a serious crime. The distribution of severity codes is very similar between the two surveys. The RDD survey has a slightly lower average, but the difference between the two means is not statistically significant.

The second part of the second hypothesis of the study stated that

5	1.9	2.1
6	1.2	1.3
7	.3	.7
8	.4	.1
9	.1	.1
10	.1	.2
11+	.0	.2
	99.8	100.1
	(N = 685)	(N = 6,477)
Average	2.04	2.11
Variance	2.12	2.37

Standard error of difference = .06 n.s. at .01

NOTE: Columns do not add to 100 percent because of rounding.

CITIZEN ATTITUDES

The questionnaire used in these surveys included a third type of data—namely, citizen attitudes on several topics related to crime.[5] Among other things, the respondents were asked for their *impressions* of the crime trend, neighborhood safety, and police performance. The respondents merely gave their opinions about how they perceived the situation, so of course there were no right or wrong answers.

The results for the five attitude questions are reported in Table 20. The differences between the two surveys are very slight on all five questions. Nonetheless, the differences are statistically significant for the one question on the United States crime trend. Given the ease with which statistical significance can be achieved with large samples, not

TABLE 19

TO HAVE BEEN REPORTED TO POLICE BY TYPE OF CRIME
RDD AND LEAA CITYWIDE SURVEYS

	PERCENTAGE			
	RDD	LEAA	Z-Statistic	Significance
	55.5	54.6	+.21	n.s.[a]
	26.8	29.3	-.57	n.s.
	69.1	74.9	-.72	n.s.
	51.0	50.5	+.04	n.s.
	41.9	42.7	-.15	n.s.
	31.9	32.6	-.22	n.s.

TABLE 20
RESPONSE TO ATTITUDE QUESTIO
RDD AND LEAA CITYWIDE SAMPL

A. Within the past year or two, do you think that crime in your neighbo
mained about the same?

	PERCENTAGE	
	RDD	LEAA
Increased	31.2	32.9
Decreased	6.8	7.7
Same	48.2	45.3
Haven't lived here that long	6.7	5.9
Don't know	7.1	8.2
	(N = 1,372)	(N = 8,791)

B. Within the past year or two, do you think that crime in the United S
mained about the same?

	PERCENTAGE	
	RDD	LEAA
Increased	72.1	68.0
Decreased	6.0	7.9
Same	16.0	18.7
Don't know	6.0	5.3
	(N = 1,372)	(N = 8,789)

ıld you feel being out alone in your neighborhood *at night*—very safe, reasonably
ry unsafe?

PERCENTAGE			
RDD	LEAA	Difference	Significance
16.7	19.4	-2.7	χ^2 = 10.03
40.3	39.0	+1.3	df = 3
19.4	20.6	-1.2	n.s. at .01
23.6	20.9	+2.7	
(N = 1,371)	(N = 8,772)		

ıow safe do you feel or would you feel being out alone in your neighborhood?

PERCENTAGE			
RDD	LEAA	Difference	Significance
59.8	59.9	-0.1	χ^2 = 3.25
33.0	34.0	-1.0	df = 3
5.1	4.7	+0.4	n.s. at .01
2.0	1.4	+0.6	
(N = 1,372)	(N = 8,779)		

ıat your local police are doing a good job, an average job, or a poor job?

PERCENTAGE			
RDD	LEAA	Difference	Significance
56.0	53.2	+2.8	χ^2 = 7.26
31.6	34.8	-3.2	df = 3
7.7	8.6	-0.9	n.s. at .01
4.7	3.4	+1.3	
(N = 1,372)	(N = 8,782)		

THE POLICE DISTRICT NO. 1 SUBSAMPLE

As an added test of RDD, a supplemental sample of interviews in the neighborhoods covered by the Cincinnati Police Division's Police District No. 1 (P.D. No. 1) was carried out. These neighborhoods include Cincinnati's downtown area, surrounding areas of slum dwellings (including poor blacks and poor whites), and a small neighborhood of working class whites and young professionals. This area was chosen because of its high concentration of poor people and because the Cincinnati Bell Telephone Company records show it to have the city's lowest residential telephone subscription level. If RDD has problems with coverage and biases, these problems should be most pronounced in an area like P.D. No. 1.[7]

With the supplemental interviews, there was a P.D. No. 1 sample of 662 households composed of 1,128 persons aged 12 and older. Upon special request for a comparable sample, the Census Bureau provided tabulations based on their respondents living within P.D. No. 1. The LEAA-Census subsample of P.D. No. 1 consisted of 756 households and approximately 1,247 persons before weighting. Because these special tabulations are not so detailed as those that could be drawn from the computer tape of the citywide sample, it is impossible to make as many comparisons within P.D. No. 1 as have been made for the entire city.

Tables 21 and 22 present the demographic comparisons for the P.D. No. 1 samples. There are statistically significant differences on only three of the nine comparisons: on the race of household head, race of person, and sex of person variables.

The larger representation of blacks in the RDD sample was a surprise because blacks in general, as noted, have a slightly lower prob-

ability of telephone subscription than whites, but the P.D. No. 1 area is atypical in both its black and its white residents. The persons living throughout most of the area are among the poorest of their races. The poor Appalachian whites living in P.D. No. 1 are less likely, as previously noted, to subscribe to telephone service than are poor blacks. But these differences seem insufficient to cause the degree of underrepresentation of whites found in the RDD survey.

The reason for the discrepancy may rest with the inability of the Census Bureau to locate the proper proportion of blacks for its survey. The 1970 Census is known to have grossly underenumerated blacks.[8] Because the sampling and weighting procedures used for the LEAA survey were based primarily on the 1970 Census, it is indeed conceivable that the Census Bureau's method was inherently biased against blacks. This methodological bias is probably exacerbated by the fact that the small, predominantly black area under investigation has undergone major demographic changes since 1970. Unfortunately, neither survey method is flawless, and there is no absolute standard for comparison available. Thus, a definitive statement about which method most accurately samples the racial composition of a community is impossible. There are good reasons to believe, however, that traditional sampling methods are far from perfect in securing a sufficient proportion of blacks.

The sex differences are somewhat harder to explain. The national data examined in Chapter 2 and the results from the citywide sample would not indicate a probable sex bias. Because men are at home less often than other family members, interviewers do have more difficulty

TABLE 21
HOUSEHOLD DEMOGRAPHIC CHARACTERIST
RDD AND LEAA SAMPLES FOR P.D. NO. 1

HOUSEHOLD CHARACTERISTIC	PERCENTAGE		
	RDD	LEAA	Difference
Household Income			
Less than $3,000	50.7	51.2	-0.5
$3,000–$7,499	29.9	28.5	+1.4
$7,500–$14,999	14.3	14.5	-0.2
$15,000–$24,999	3.9	3.9	0.0
$25,000 or more	1.3	1.8	-0.5
	(N = 536)	(N = 756)	
Race of Head			
White and other	31.2	41.1	-9.9
Black	68.8	58.9	+9.9
	(N = 662)	(N = 756)	
Age of Head			
Under 35	24.4	28.0	-3.6
35–49	19.7	21.4	-1.7
50–64	26.1	24.2	+1.9
65 or older	29.7	26.5	+3.2
	(N = 658)	(N = 756)	
Housing Tenure			
Own	7.5	6.6	+0.9
Rent or no cash rent	92.5	93.4	-0.9
	(N = 658)	(N = 756)	
Persons Aged 12+ in Household			
Mean	1.71	1.65	+.06
	(N = 662)	(N = 756)	

TABLE 22

SONAL DEMOGRAPHIC CHARACTERISTICS
RDD AND LEAA SAMPLES FOR P.D. NO. 1

ERISTIC	PERCENTAGE			SIGNIFICANCE
	RDD	LEAA	Difference	
	28.8	36.7	-7.9	χ^2 = 16.88
	71.2	63.3	+7.9	df = 1
	(N = 1,127)	(N = 1,247)		SIGNIFICANT at .01
	38.6	44.0	-5.4	χ^2 = 7.22
	61.4	56.0	+5.4	df = 1
	(N = 1,127)	(N = 1,247)		SIGNIFICANT at .01
	10.1	10.9	-0.8	
	9.8	9.2	+0.6	
	8.4	10.7	-2.3	χ^2 = 7.71
	13.7	13.8	-0.1	df = 6
	15.7	17.3	-1.6	n.s. at .01
	21.6	19.7	+1.9	
	20.7	18.3	+2.4	
	(N = 1,118)	(N = 1,247)		
	44.2	45.2	-1.0	χ^2 = 3.64
	46.6	43.6	+3.0	df = 2
	9.2	11.2	-2.0	n.s. at .01
	(N = 1,106)	(N = 1,246)		

phone than would males.[9] If either of these suppositions is correct, the RDD survey could do a better job of reaching female respondents than the LEAA-Census survey did, especially in an area like P.D. No. 1. Such an explanation cannot be verified with the present data, but if the source of the problem should lie in the greater accessibility of females to telephones, then researchers should be aware that telephone surveys may have some problems achieving a proper sex balance in neighborhoods of this type. Fortunately, most surveys deal with the general population where this problem will not occur to the same degree.

On the positive side, it is worth noting that the P.D. No. 1 subsample does not show a significant difference on the education variables. This finding indicates that the education differences found in the citywide sample may have been more the result of chance sampling variation than a serious bias inherent in RDD.

Tables 23 and 24 present the crime victimization rates for the P.D. No. 1 samples. In this instance, some of the RDD rates are slightly lower than the corresponding ones from the LEAA-Census survey. This is uniformly true for the personal crimes. Given the sample sizes involved, these differences are small, and none is statistically significant at the .01 level.[10]

Given the particular characteristics of the P.D. No. 1 neighborhoods, the RDD survey seems to have done rather well. There appears to be some sex bias here, but RDD may be doing better than the traditional method in representing the racial composition. Otherwise, the two P.D. No. 1 subsamples seem to be very similar. Tabulations of severity levels and the attitude questions were not available from the Census Bureau for the P.D. No. 1 comparisons.

TABLE 23
COMPARISON OF HOUSEHOLD CRIME VICTIMIZATION RATES PER 1,000
RDD AND LEAA SAMPLES FOR P.D. NO. 1

CRIME	PER 1,000			
	RDD[a]	LEAA[b]	SEdiff[c]	Sig.[d]
Burglary	214.5	186.2	21.9	NTN
Household larceny	72.5	74.1	15.0	n.s.
Motor vehicle theft	36.3	24.9	9.7	NTN
Total household crimes	323.3	285.2	25.6	NTN

[a]Based on 218 incidents reported by 662 households.

[b]Based on 216 incidents reported by 756 households (estimated from weighted data).

[c]Estimated standard error of the difference (used in test of significance).

[d]NTN = no test necessary; n.s. = not significant.

Thus these results apparently support the use of RDD as a means for collecting crime victimization data. Indeed, the figures in Tables 23 and 24 are what would be expected if there is no difference between the two methods as measured by the results achieved. When this evidence is combined with the results from the citywide samples as well,

[a]Based on 209 incidents reported by 1,128 persons.

[b]Based on 285 incidents reported by 1,247 persons (estimated from weighted data).

[c]Estimated standard error of the difference (used in test of significance).

[d]n.s. = not significant.

[e]Total personal crimes include rapes and attempted rapes.

quality and reliability to those data collected by a traditional personal interview. In addition, RDD surveys are clearly much less expensive and easier to administer.

The demographic comparisons showed no serious biases for the citywide samples. The poor and blacks were not underrepresented as had been feared. The RDD sample was slightly skewed toward the more highly educated segments of the population, but the magnitude of the difference does not seem to be serious. In addition, the fact that the data from the P.D. No. 1 samples did not have an education bias indicates that the discrepancy in the citywide data may represent chance sample selection rather than systematic bias of RDD. The P.D. No. 1 data and LEAA-Census data differed on the sex ratio, but whether this factor is positive or negative for RDD is difficult to determine. On the positive side, the P.D. No. 1 data did indicate that RDD may be somewhat more successful in locating black respondents and in securing their cooperation.

RDD proved very satisfactory for collecting crime victimization data. In the citywide samples, the victimization rates for personal crimes were slightly higher in the RDD sample; while the RDD rates for household crimes were considerably higher in every category—the rate for all household crimes was 39 percent greater than the rate found in the LEAA survey. These higher rates are evidence that RDD may be a superior method for collecting victimization data. In the P.D. No. 1 sample, the victimization rates were very similar in the two surveys, with none of the rates significantly different.

Expressions of opinion about crime trends, neighborhood safety, and police performance constituted the third type of data. Answers to four of the five questions revealed no significant differences between the two samples. Percentage differences in response to the remaining question, while statistically significant, were very small and probably do not represent important discrepancies. Thus the attitude questions also seem to support the view that RDD is a reliable survey method.

A review of all the evidence indicates that the performance of RDD was remarkably good. The omission of citizens without telephone service does not appear to bias the sample demographically, nor does it adversely affect the substantive information being collected—in this case, crime victimization data and attitudes toward crime-related matters. This finding has special significance when one considers that persons without telephone service are slightly more likely to be poorer, less educated, and non-Caucasian.

Although telephone coverage is not complete, there are two reasons why this factor would not put a telephone survey at a special dis-

1. A test of significance as used here is a statistical means for determining the probability that two samples randomly drawn from the same population would show a certain degree of difference on some characteristics. Because samples are being used, they cannot *always* be expected to have the same distribution as the original population. Purely by chance, the survey can come up with too many college-educated people, or too many women, or whatever.

   The chi-square statistic is a way of summarizing the differences between frequency distributions produced by two different samples. If the chi-square value is small, the samples are assumed to have been indeed drawn from the same population and the observed differences are most likely the artifact of chance. When chi-square is a large value, however, the differences are probably the result of drawing the samples from populations that actually differ on this characteristic. This is because large chi-square values are unlikely to occur by chance. The probability of obtaining a large chi-square can be determined by consulting the appropriate statistical table. This probability is called the "significance level."

   A significance level of .01 has been chosen here; this means that if the sampling were repeated 100 times, this large a difference (or a larger one) would be anticipated to occur only one time. It is more common to choose a significance level of .05. The choice of significance level should be related to how many "errors" the researcher is willing to accept. An "error" in this study could cost millions of dollars. Therefore, instead of accepting the probability of one "error" in 20, as would be the case at .05, the RDD survey chose to accept only one "error" in 100—that is, the .01 significance level.

   To complicate things, multiple tests, like these done here, increase the probability of finding at least one chi-square which is

significantly large merely due to chance. The formula for calculating this increased probability is

$$1.00 - (1.00 - \rho)^k$$

where ρ is the significance level used for the individual test and k is the number of tests performed. Of course, it should be remembered that tests of statistical significance reveal only the *probability* that a real difference has been found.

When two averages must be compared (as at the bottom of Table 10), the standard error of the differences has been used in the test of significance. The standard error is computed from the degree of variation (variance) on that item in each of the surveys. The standard error can be used to determine the probability that the observed difference in the averages occurred due to chance. The chi-square test and t-test used in the significance testing are the most powerful tests of which the authors are aware for the type of testing required here.

The relative sizes of the two samples have some interesting effects on the significance tests. First, the large sample sizes make it easy to find statistical significance even when the differences that exist are not substantively important—for example, although a difference of 1 percent can be statistically significant if the samples are large enough, seldom would a 1 percent difference be important enough to worry about. Second, in the citywide samples, the LEAA-Census sample is much larger than the RDD sample; hence one can be more certain of the accuracy of the LEAA-Census estimates than of the smaller RDD estimates. Fortunately the significance tests take this fact into account.

2. Because the RDD sample contains 303 household crime incidents and 359 personal crime incidents, crime breakdowns more detailed than those presented here cannot be provided. Even in the large

the normal curve must be consulted. For a one-tailed test at the .01 level, this table shows that the standard error of the difference should be multiplied by 2.33. If the observed difference is larger than this quantity, then the difference is significant at the .01 level.

4. The Sellin-Wolfgang Severity Code is discussed in detail in subsection "Creation of the Severity Code." See below, Chapter 6.
5. The attitude questions were asked only of persons 16 years old or older. The Census Bureau included the attitude questions in the questionnaires for a randomly selected half of the households. RDD interviewers posed the attitude questions to all RDD respondents who were old enough to answer them. To save time, however, only five of the questions from the Census Bureau's full list were asked.
6. As a case in point, the Gallup and Harris Polls often disagree about the level of voter support for American presidential candidates. Their polls taken in late November and early December 1975 gave opposite indications of President Ford's chances for reelection. See *Newsweek*, January 5, 1976, 16-17. The best explanations for this disagreement are differences in time (the surveys were taken two weeks apart), chance sampling variation, and volatile opinions.
7. The authors chose to sample the entire district rather than just the slum areas, because the Police Foundation had already done extensive survey research throughout the entire district for other projects.
8. See Jacob S. Siegel, "Estimates of Coverage of the Population by Sex, Race, and Age in the 1970 Census," *Demography* 11, February 1974, 1.
9. The authors are indebted to Lee Sechrest of Florida State University and the Police Foundation Evaluation Advisory Group for suggesting these alternatives.

10. With almost all of the other crime rates being higher in the RDD samples, it is a little surprising to find that all the personal victimization rates are lower in the P.D. No. 1 sample. The authors suspect that this finding may be the result of the slight underrepresentation of men, because men are much more likely to be the victims of most types of personal violence or theft.

PART II

CHAPTER 5

USING RDD

This chapter contains recommendations on how to conduct an RDD survey. Although the recommendations are specific to the collection of crime victimization data, the RDD approach can be applied to the collection of almost any type of survey data.

PLANNING AND ADMINISTRATION

As with any undertaking, it is advisable to designate one person as project director of the survey. Ideally this person should have survey research experience and understand why the data are being collected and how they are going to be used. If such a person is not available, the group seeking to collect the specific data should engage a survey research organization or at least a special consultant to help with the

SAMPLING

Sampling for an RDD survey is quite simple and straightforward. First, determine all the operating telephone exchanges (area code, if necessary, plus three-digit prefixes) in the geographic target area. For small area surveys, this information usually can be found in the first several pages of the local directory. When a statewide, regional, or national sample is sought, working exchanges within area codes can be obtained from the *Distance Dialing Reference Guide* published by the AT&T Long Lines Department. This information is also available on computer tape from AT&T.[1] Once this list of exchanges has been constructed, the procedure for generating the numbers is very straightforward. Starting with one exchange chosen at random (including its area code, if necessary) the surveyor generates a four-digit random number to complete the phone number, repeating this sequence until the desired quantity of telephone numbers has been created. The exchange should be randomly selected anew for each number.

The random numbers can be selected either by computer or by hand using a random number table. Because the hand method is laborious, boring, and error-prone, the computerized method is superior. An example of a FORTRAN IV program to generate a random sample of phone numbers, which produced the numbers for the surveys reported here, appears in Appendix A. The same appendix also includes a reproduction of a computer card generated by this program. The information punched on the card includes the randomly generated telephone number, an indication of the survey for which the number is to be used, a reminder of how many times the interviewer should try to reach the

telephone number, and a sequence number. Although this program and output format were those used in the test of RDD, this approach is not necessarily the best one for other surveys. For instance, the sample could be printed on computer paper instead.

Two complications deserve special attention. First, the same exchange can occur in more than one area code and in surveys covering large geographic areas, the person drawing the sample must take this fact into account. For example, if the geographic target area were the entire state of Ohio, the exchange "221" would appear in the list of exchanges four times because it exists within each of Ohio's four area codes. In such a situation, the method used to select the sample must ensure that the correct area code is linked to the exchange; the area code and exchange must be considered as a single unit. The first step of the RDD process, then, is to pick randomly an exchange *with* its associated area code. In the Ohio example, these combinations would include 216-221, 419-221, 513-221, and 614-221 as four of the possible "exchanges" to be selected from throughout the state.

Another problem is that some or all of the telephone exchanges may include numbers inside the geographic target area and some outside it. For example, suppose Cincinnati is the target area for the survey, but that exchange "265" overlaps both Cincinnati and an adjacent suburb. When this happens, it is necessary to devise a method to screen out the households outside of the desired target area—in this case, those in the suburban area. (This problem is discussed at greater length later in this chapter.) Usually this screening is best done as part of the interviewing process itself—that is, the first questions should determine

but that exchange "789" has only 1,000. Now if 200 "telephone numbers" are randomly created by the method previously described, 100 would be expected to have the "234" prefix and the other 100 would have the "789" prefix, because each of the *possible* telephone lines has an equal probability of being selected. Of course, in the sample of 200 "numbers" there is no way for the surveyor to know which are connected to households and which are not. As the RDD interviewers begin calling, they will find many business or not-in-service numbers in the sample. These numbers should be discarded. In the end, however, the interviewers should have reached 20 households with the "234" exchange and 10 households with the "789" exchange, because one-fifth of the total possible lines in the "234" exchange are actually connected to working residential numbers and one-tenth are connected in the "789" exchange. Although the distribution of working telephone numbers would be unknown in advance, the proper proportions would be automatically obtained because each number inherently has an equal probability of selection.

In the example, the numbers used represent the *expected* outcomes. In a real sample, the proportions are not likely to come out exactly, of course, because of sampling variation. This is not a problem, because the formulas for computing the confidence interval of estimated crime rates, attitudes, demographic characteristics, or whatever, take into account this inherent feature of random sampling. An additional feature of the RDD sampling method is that the generated numbers do not necessarily have to be called in the sequence in which they were generated. Furthermore, interviewing can stop before the list of random numbers is exhausted, and additional numbers can be created

if needed. It is important, however, that the random nature of the generated number not be systematically disrupted. Thus, interviewers should not concentrate on exchanges which they like (that is, those from the "better" parts of town) nor avoid exchanges they do not like. Similarly, interviewing should not be done from one exchange (or group of exchanges), exclusively, before progressing to the next.

RECRUITMENT OF SUPERVISORS AND INTERVIEWERS

There is a truism in survey research that a survey can be made worthless at almost any phase of the process—sampling, questionnaire design, interviewing, coding, or analysis. The two most dangerous phases are sampling and interviewing. One of the advantages of RDD is that sampling is simple, straightforward, and thus easy to do correctly. As with any form of survey data collection, however, RDD data collection can be seriously biased in the interviewing phase. Thus it is vital that competent professional personnel handle the supervision and interviewing. There are several ways to secure use of such personnel, including hiring a survey organization to conduct either the entire study or only the interviewing phase.

If neither of these options is feasible, the project director can recruit his or her own interviewers and supervisors. Although this recruitment must be done with great care, it is manageable mainly because a pool of free-lance interviewers and supervisors exists in almost every large American city. The Census Bureau and national survey firms hire such persons on an irregular basis. One way to locate workers is to ad-

higher, varying from about $3.50 to $5.50 per hour. Interviewers and supervisors should be paid for time spent in training.

TRAINING OF SUPERVISORS AND INTERVIEWERS

The training of interviewing supervisors and the interviewers themselves is also very important. The project director should be in charge of the training process because the supervisors and interviewers must thoroughly understand the overall purpose of the project, the purpose of each question, and the other operational aspects of the survey.

The supervisors should be instructed first in the supervisory aspects of their job and about RDD, before the interviewers are trained. They need to know why the project director decided to do things in a certain way, because they will probably have to explain the reasoning to the interviewers. They should be given a questionnaire to study before the interviewer training begins. Training experienced supervisors should require only three to four hours. Most of the supervisors' instruction with regard to specifics of the questionnaire can be done in conjunction with training the interviewers.

Training the interviewers for a crime victimization survey will take at least two full days. The following items should be covered carefully:

- Purpose of the study
- Tasks of the supervisors
- Scheduling, pay, and other administrative arrangements
- Use of the telephone equipment
- Use of the RDD sample
- Handling of refusals, terminations, no answers, and callbacks

- Careful and complete discussion of every question to be asked of the respondents
- Complete discussion of how to handle all possible answers to the questions.

It is vital that the interviewers and supervisors understand the purpose of the study. The project director should explain carefully why the data are needed. If this briefing is not handled well, the interviewers are likely to collect inaccurate or incomplete data.

Every interviewer must understand the purpose and meaning of *every* question. Each question and all possible answers that can be anticipated should be discussed. If, as suggested in the next section, the survey employs the questions developed by LEAA-Census, the Census Bureau training manuals have a discussion of each question that can be modified as necessary. The process of explaining the questions one by one is very time-consuming, but it is vital to the success of the study.

After the interviewers have become familiar with the study, they should practice by interviewing each other and their friends. Also useful are role-playing exercises, in which sets of situations are constructed to illustrate typical interviewing problems. One interviewer assumes the role of the "problem" respondent and gives answers corresponding to the specified situation. Another interviewer tries to handle the situation, with the supervisor correcting mistakes and giving advice. Finally, the interviewers should conduct several practice interviews with the target population before the real fieldwork begins.

The project director should make sure that more than enough interviewers are recruited and trained at the training session. A problem

the victimization experience that are not crucial to the computation of crime rates. The demographic and attitude questions can be adopted, modified, or excluded as the purpose of the study requires. Yet, unless only a subset of the types of crime is to be measured, all of the screen questions are necessary, as is most of the Incident Report Form. (The questionnaires used in this study appear in Appendix B.)

INTERVIEWING: THE PHYSICAL LAYOUT AND EQUIPMENT

One particular aspect of the interviewing process must be emphasized. That is that all of the interviewing should be carried out from a central office. Many survey research organizations allow their telephone interviewers to call from their homes. The interviewers usually prefer this method, because it gives them greater freedom, involves little travel, and allows them to stay at home all day. The survey research organization need not supply extra telephones (a saving of about $25 per month per phone), nor is it required to devote scarce and/or expensive space to the telephone bank.

Although advantages exist for home interviewing, there are far more advantages to having all calls made from a central office:

- Central office calling eliminates all the distractions of home: Babies do not need their diapers changed and children do not need lunch prepared or served.
- Eight hours' pay in a central office usually means eight hours of productive effort; whereas a recorded (and paid) eight hours of work at home may actually have been only

five or six hours of work. This translates into more interviews finished faster, which may be very important if a tight time schedule exists.

- In a central office, supervision is continuous, so mistakes are detected early and corrected quickly. In effect, a continual training process takes place, so the interviewers usually improve in quality.
- Although it may not be immediately obvious, the interviewing crew develops an *esprit de corps* working together. Coworkers encourage each other and consider the attainment of production goals a group success.
- In a central office, special equipment can be used—often impossible if interviewing is done at home. As discussed later, lack of touch-tone equipment and headsets reduces efficiency and increases per-interview costs.
- If interviewing is done at home, accounting for long distance interviewing can be difficult. To be properly reimbursed the interviewer must keep a record of every call made. This takes extra time and can cause an accounting nightmare.
- Blank questionnaires and all other necessary materials are readily available in a central office. Completed interviews can be turned in to the supervisor and checked immediately. This is very helpful in reducing coding and editing costs and in cutting errors.

Each of the interviewers at the central office should work at a

used within the city were also in use outside the city. Thus, if the generated telephone numbers had been used indiscriminately, some of the sample would have been drawn from outside the desired geographic boundaries. To combat this problem, a screening procedure was developed to ensure that only eligible respondents were interviewed. For the RDD citywide sample the screening was relatively simple, because most people were able to answer whether or not they lived within the city limits. The interviewers were supplied with a list of neighborhoods that *were* inside the city limits. If respondents proved unsure as to whether or not they lived within the city, they were asked what neighborhood they lived in and the interviewer checked the response against the list of accepted areas. This procedure worked quite effectively.

The P.D. No. 1 sample, however, presented a much more serious problem both because few Cincinnati residents knew in what police district they lived and because the match between the geographic area defined by P.D. No. 1 and the telephone exchanges used in the area was very poor. Although all P.D. No. 1 telephone lines originate from the Central Exchange Office of Cincinnati Bell, this office also serves some surrounding areas. Consequently, about half of the households reached had to be eliminated. The technique devised to accomplish this screening worked extremely well. It basically consisted of asking a series of increasingly specific questions to determine the location of the respondent by neighborhood and breaking off the interview when it became apparent the telephone was located outside the area being surveyed. (Appendix B contains a copy of screening questions.) The interviewers quickly became astute at selecting only eligible respondents,

proving that with carefully designed and tested screening procedures, surveys of even complex geographic areas can be handled, be they congressional districts, townships, neighborhoods, or whatever.

The key to efficient screening is to take into account *all* ways of identifying the desired area. For example, does it consist of identifiable neighborhoods, or can it be identified by zip code or by well-known boundaries such as major streets?

Several additional points need to be made about screening. At first it would seem that the necessity for complex screening on a survey would dramatically increase the refusal rate for the study, but this did not prove to be the case. In fact, the refusal rate for the P.D. No. 1 sample was only slightly higher than the refusal rate in the citywide sample. The citywide screening was quite simple but the P.D. No. 1 screening was reasonably complex. The probable reason that complex screening does not dramatically increase refusal rates is that screening questions are exactly the type of questions recommended for beginning the interviewing process. They are easily answered and nonthreatening, and they help convince the respondents that they are truly anonymous.

CALL-BACK AND NO-ANSWER PROCEDURES

In survey research, and especially in telephone-based survey research, it is vital to handle call-backs adequately. In the Cincinnati study, call-backs were all the more important because all members of the household 14 years and older had to be interviewed independently and proxy interviews had to be obtained for 12- and 13-year-olds. As is generally true in telephone surveys, two types of call-backs had to be

out (at work, at school, at play) each time the call was attempted.

- The sample number was a working telephone that had not yet been disconnected even though the previous subscriber had moved.
- The sample number was in a phone booth or some other public location where it was not normally answered.
- The local telephone company had the sample number connected to equipment which produced a ring even though the line was not in service.

Every effort must be made to reach the second type of no-answer cited above, and for this reason a special rotation scheme for no-answers should be set up. The best way to reach a no-answer is to try the number at different times of the day and week. The supervisors should be given the responsibility of seeing that these calls are made. If the first call was made during the day, the second call should be made during the evening. If the second call does not result in a contact, another attempt should be made later in the week, especially on a Saturday.

Call-backs should be handled similarly. Because there has been initial contact with the household, the interviewer should make every attempt to set up an appointment with a specific respondent. Once a household is reached, a file folder should be set up for it. The interviewer should keep all of the interview protocols and special notes together in this folder. Where possible, the interviewer who made the initial contact with the household should attempt to complete the survey of this household. Because it is not always possible for the same person to

make the second call, the supervisors should have a special call-back file. Call-backs can be filed according to the day on which the appointment has been made, so that a different interviewer can call at the appropriate time. (Appendix D provides a breakdown on the final disposition of all dialings made in the RDD survey.)

With any survey technique refusals are another serious problem, and this is no less true in an RDD study. Interviewers must be instructed and trained to make every effort to coax the respondent into completing the interview. Failing this, a refusal call-back procedure should be initiated, involving a separate, later attempt to complete the interview. In some cases the supervisor, introducing herself as "*the* supervisor," can make this additional attempt. This procedure is instituted under the assumption that many respondents refuse to be interviewed only because of something that is going on in the household at the exact time the interviewer calls. Some examples are an illness in the family, a misbehaving child, supper on the stove, or a family dispute. In the RDD citywide sample, the refusal call-back procedure cut refusals by 31 percent (from 11.1 percent to 7.6 percent); while in the P.D. No. 1 sample the refusal rate was lowered by 29 percent (from 12.0 percent to 8.5 percent). Thus, this procedure proved to be quite valuable.

CODING AND KEYPUNCHING

Coding in RDD is no different from that used in any other type of survey research. In the crime victimization study, the questionnaires were made self-coding. The interviewer simply circled the number corresponding to the answer given by the respondent. The only exception

address, he or she was offered the option of naming the nearest street corner. Generally by the end of the interview, the interviewer had established sufficient rapport so that few persons failed to give an address or the nearest street corner.

CHAPTER 6

DATA PROCESSING AND ANALYSIS

The questionnaires used in the present crime victimization surveys force the resulting data to be organized in a rather complex fashion. Actually three distinct levels of information are dealt with. At the highest level are data about the *household in general:* where it is located, how long the family has lived in that location, whether the quarters are owned or rented, what the total family income is, and so forth. Information about *each household member* yields a second layer of data. At the person level are the individual's characteristics (age, sex, education, and so forth), the person's responses to the attitude questions, and an indication as to whether the particular respondent reported any victimizations. Finally, the *incident reports* constitute a third level of data. These reports provide the detailed information about each incident reported during the interviews. Of course, some

When data containing information from several different levels is to be stored, the data file can be organized in a hierarchical form. A hierarchical file contains multilevel data recorded in a sequence where the lower level items immediately follow the higher level item with which they are associated. In the RDD victimization survey, the record containing the household information is the highest level of the hierarchy. The person records, each containing the background information about a specific respondent, are the second level. All of the person records for respondents in the same household are grouped together and placed after their household record. At the third level come the incident records containing the details of each reported victimization. All of the incidents reported by a particular respondent are grouped together and physically placed after that respondent's person record. Of course, some respondents may not have any incident reports.

As an example of how a hierarchical organization would look, consider the data collected from two hypothetical households. In household A there is one person, and he has reported one incident. The household record should be placed first, followed by the person record, followed by the incident record as:

Household Record A
  Person 1 in household A
    Incident 1 from person 1 in household A.

Now consider household B with five respondents. Suppose that the first person reported three incidents, the fourth person reported two, and the others reported none. When these are added to the records for household A, they would appear as follows:

Household record A
  Person 1 in household A
    Incident 1 from person 1 in household A
Household record B
  Person 1 in household B
    Incident 1 from person 1 in household B
    Incident 2 from person 1 in household B
    Incident 3 from person 1 in household B
  Person 2 in household B
  Person 3 in household B
  Person 4 in household B
    Incident 1 from person 4 in household B
    Incident 2 from person 4 in household B
  Person 5 in household B

As other households are added, their data records would be placed in a similar sequence.

Although the organization of a hierarchical file is logical and straightforward, it presents several problems for computerized data analysis. One problem is that each type of record usually differs from others in length. In the data file from our RDD survey, the household record occupied 94 columns on the computer tape, while the person record was 48 columns long, and the incident record required 158 columns. This variation means that the data records must be stored on the computer in a variable length format and that they can be used only on computers that tolerate variable length records. Fortunately, most of the current, sophisticated computers will accept variable length data

pass them along to the statistical program as though they were coming from a homogeneous record. If the statistical program does not have the capability of doing this on its own, then the analyst must write a special program to reorganize the required variables into a homogeneous record format.

The creation and manipulation of hierarchical files are somewhat complicated; however, a competent programmer can handle the task without undue difficulty, given an appropriate statistical package with which to work. Because this type of data is becoming more common in social research, more standard statistical programs are adopting procedures for handling hierarchical files. Thus, in the future, the analyst's job should be easier.

CREATION OF THE CRIME CODE AND CRIME RATES

In addition to organizing the survey data into a hierarchical file, the analyst must create some new variables out of information provided by the respondents. Perhaps the most important of these is the "crime code," a standard set of categories used by LEAA to classify the type of crime described by the respondent. By examining responses to the incident questionnaire items, the analyst can determine the category which properly describes the crime. Table 25 lists the 36 crime codes and their verbal descriptions. Appendix E provides the precise question responses which determine each code, and the FORTRAN program used to compute the crime code in the RDD survey.

The complete set of crime codes is more detailed than that needed for most analyses. In preparing the crime rate tables for Chapter 4, some of the original codes were combined into broader crime areas. In

TABLE 25
LEAA CODES FOR TYPES OF CRIME

CODE	DESCRIPTION
1	Rape with theft
2	Attempted rape with theft
3	Serious assault with weapon with theft
4	Serious assault, no weapon, with theft
5	Minor assault with theft
6	Rape without theft
7	Attempted rape without theft
8	Serious assault with weapon without theft
9	Serious assault, no weapon, without theft
10	Minor assault without theft
11	Attempted assault with weapon without theft
12	Attempted assault, no weapon, without theft
13	Robbery with weapon
14	Robbery, no weapon
15	Attempted robbery with weapon
16	Attempted robbery, no weapon
17	Purse snatch, no force
18	Attempted purse snatch, no force
19	Pocket picking
20	Burglary, forcible entry, nothing taken, property damage
21	Burglary, forcible entry, nothing taken, no property damage
22	Burglary, forcible entry, something taken
23	Burglary, unlawful entry, without force
24	Burglary, attempted forcible entry
25	Larceny under $10
26	Larceny $10–24

sample and multiplying the result by 1,000. In preparing Table 12, 150 incidents of burglary were revealed for our RDD sample. Because RDD surveyors interviewed 800 households, the rate per thousand comes to $150 \div 800 \times 1{,}000 = 187.5$. Personal crime rates are computed similarly by dividing the number of incidents of that type by the number of persons interviewed and multiplying the result by 1,000.

CREATION OF THE SEVERITY CODE

Many analysts find it useful to study crimes in terms of their relative severity. Rather than asking whether the crime was a larceny or an assault, they would like to know how severe the crime was on some consistent measure of seriousness. The reason for this inquiry is that a major larceny can be worse for the victim than a minor assault, while a major assault, such as rape, is usually considered much worse than any larceny.

Several schemes have been devised for rating the severity of crimes. One of the most widely used methods is the Sellin-Wolfgang Severity Code. In the early 1960s, when Thorsten Sellin and Marvin Wolfgang were studying juvenile delinquents,[2] they needed some way to distinguish between the more serious and less serious crimes of these delinquents. Their approach was to have samples of students, police officers, and juvenile court judges rate the relative seriousness of various components of criminal acts. Analysis of these ratings provided scores which could be assigned to each component. Table 26 presents a list of these scores.

A particular crime is rated by adding up the scores for each component as they appear in the Sellin-Wolfgang list.[3] A simple act, such as

TABLE 26
SELLIN-WOLFGANG SERIOUSNESS SCORES

ELEMENT	SCORE VALUE
Minor injury to victim	1
Victim treated and discharged	4
Victim hospitalized	7
Victim killed	26
Victim of forcible sexual intercourse	10
Intimidated by weapon, add	2
Intimidation of persons in connection with theft, etc. (other than in connection with forcible sex acts):	
Physical or verbal only	2
By weapon	4
Forcible entry of premises	1
Value of property stolen and/or damaged:	
Under $10	1
$10–250	2
251–2,000	3
2,001–9,000	4
9,001–30,000	5
30,001–80,000	6
Over $80,000	7
Theft of motor vehicle (recovered, undamaged)	2

a boy's being assaulted by two companions who inflict minor injuries, would receive a score of 1, because "minor injury to victim" is the only component present. Sellin and Wolfgang give the following example of a complex crime, where the numbers in parentheses indicate the score

Basically, each household should be counted in inverse proportion to the number of telephone lines reported. A household with one telephone receives a full weight of 1.00; for a household with two telephone lines, the weight is .50. A weight of .33 is assigned when there are three or more telephone lines. In the RDD sample, there were 736 households with one telephone line, 60 with two, and 4 with three or more, yielding a weighted sample size of 767.32 households. (1.00 X 736) + (.50 X 60) + (.33 X 4) = 767.32. Although this method is satisfactory for many tabulations, the test of statistical significance ideally should be based on a weighted sample size that reflects the actual number of households (800). To achieve this weighted sample, each basic weight was multiplied by 1.0426, which is the unweighted sample size divided by the weighted sample size (800 ÷ 767.32 = 1.0426). Table 27 summarizes the weights for the RDD citywide sample.

In the RDD survey, these weights apply to the persons and incidents as well as to the household. Each person and incident receives a weight equal to the weight of the household to which that person or

TABLE 27
HOUSEHOLD WEIGHTS FOR RDD CITYWIDE SAMPLE

NUMBER OF LINES	BASIC WEIGHT		ADJUSTMENT		FINAL WEIGHT
1	1.00	X	1.0426	=	1.0426
2	.50	X	1.0426	=	.5213
3+	.33	X	1.0426	=	.3475

incident belongs. Most standard statistical programs are able to accept these weights and use them in preparing tables and summary statistics.

The weights used in the LEAA-Census surveys are derived by a much more complicated procedure, although they serve essentially the same purpose. The Census Bureau supplies the weights along with the raw data. For more details on their procedures, see Bureau of the Census (1975).

After creating hierarchical files, calculating crime codes and seriousness indices, and assigning appropriate sample weights, the analyst proceeds to the analysis of the data as he would with any set of data. The analyses conducted in this study were rather simple and straightforward, because no complex procedures were required to test the hypotheses. Crime victimization data organized as described here can be analyzed with techniques as simple as cross-tabulations or as complex as path analysis. The type of analysis depends of course on the types of questions being asked.

Notes

1. In the work for the RDD study, the OSIRIS program package was used to build and maintain the hierarchical files because it was the only software system available on the computer being used which could do the job. The OSIRIS system has limited capabilities for handling hierarchical files, but they were adequate for this RDD project. Some of the programs distributed by the Data Use and Access Laboratory (DUALabs) in Arlington, Virginia, (in particular CENTS-AID II) are specially designed to handle hierarchical data sets like the LEAA surveys. The developers of the SPSS programs are planning to add the capability of handling hierarchical

APPENDIX A

RANDOM NUMBER GENERATING PROGRAM AND TELEPHONE NUMBER CARD

```
      COMPLEX*16 EX(1500)/1500*'          '/
      REAL*8 MF(3)/'MALE','FEMALE','      '/
      INTEGER*4 LIMIT/1500/,SEX,LABEL(20),NOYES(2)/'NO','YES'/
     +, START
C
C  EX = LIST OF EXCHANGES TO BE USED WITH AREA CODES AND LONG-
C       DISTANCE ACCESS NUMBER IF NECESSARY.  THE NUMBER IS STORED
C       IN CHARACTER MODE IN THE FIRST NINE CHARACTERS OF THE ARRAY
C       ELEMENT.
C  LIMIT = SIZE OF ARRAY 'EX' = MAXIMUM NUMBER OF EXCHANGES = 1500
C  NOUT = NUMBER OF PHONE NUMBERS TO BE GENERATED (IF ZERO, ONLY
C         EXCHANGES ARE READ AND PRINTED; NO NUMBERS ARE GENERATED).
C  SEX = WHETHER OR NOT SEX OF RESPONDENT SHOULD BE SPECIFIED
C        (0=NO; 1=YES)
C  MF = SEX DESIGNATION
C  LABEL = 80-CHARACTER LABEL TO BE PRINTED AT TOP OF LIST OF EXCHANGES
C  START = STARTING SEQUENCE NUMBER
C
C*********************************************************************
C
C  FIRST INPUT CARD MUST CONTAIN AN 80-CHARACTER LABEL (MAY BE BLANK).
```

```
20    IEX=I
      READ(5,15,END=50)
      NOUT=0
      PRINT 25
25    FORMAT('0***NUMBER OF EXCHANGES EXCEEDS PROGRAM LIMITATIONS')
C  PRINT EXCHANGES
50    PRINT 55, IEX
55    FORMAT('0',I5,' EXCHANGES PROVIDED'//
     + '0LIST OF ELIGIBLE EXCHANGES FOLLOWS:')
      K=(IEX+9)/10
      DO 60 I=1,K
60    PRINT 65, (EX(L),L=I,IEX,K)
65    FORMAT('0'10(A8,A4))
C
C  GENERATE PHONE NUMBERS
C
      KOUT=0
      JS=3
      E=IEX
      IF(NOUT.EQ.0) GO TO 110
      DO 100 I=1,NOUT
70    CALL RANDU(IX,IY,RN)
      IX=IY
      IE=INT(RN*E)+1
      IF(IE.GT.IEX) GO TO 70
80    CALL RANDU(IX,IY,RN)
      IX=IY
      J=INT(RN*9999.)+1
      IF(J.GT.9999) GO TO 80
      F=BNBCDS(J)
      IF(SEX.EQ.0) GO TO 95
85    CALL RANDU(IX,IY,RN)
      IX=IY
      JS=INT(RN*2.)+1
      IF(JS.GT.2) GO TO 85
C  OUTPUT NUMBERS
95    WRITE(7,96)EX(IE),F,MF(JS),IDENT,MAXNA,START
96    FORMAT(13X,A8,A1,'-',A4,1X,A6,1X,A4,T58,I1,T76,I5)
      KOUT=KOUT+1
      START=START+1
100   CONTINUE
110   PRINT 120,KOUT
120   FORMAT(//'0',I5,' PHONE NUMBERS GENERATED')
      END
```

NOTE: Subroutine RANDU is a random number generator from the IBM System/360 Scientific Subroutine Package (SSP), GH20-0205-4

TELEPHONE NUMBER CARD

Phone # 241-7709 PD 1 Max. No Answer 6 3889

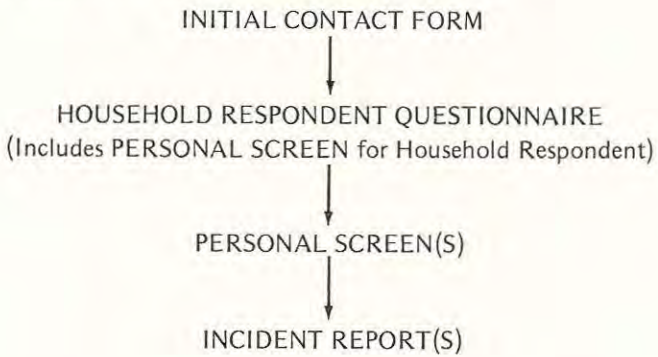
Final Disposition

___1. Not in service
___2. Business
___3. No contact made (or phone booth)
___4. Contact, but refused to be interviewed
___5. Contact, but no eligible respdnt (even after c.b.)
___6. Contact, but interview not completed
___7. Contact, and interview completed

No-Answer Record			
Date	Time	Date	Time

Call-Back Record			
Date	Time	Talk to	Call Made

The flow of the questionnaires was as follows:



Put another way the flow is as follows:

1. Initial Contact Completed.

2. Household respondent responds to Household Respondent Questionnaire.

3. All other members of the household over 14 are administered the Personal Screen. Members 12 and 13 years of age have Personal Screens answered by household respondent.

4. A Crime incident Report is answered by any respondent over 14 who has been victimized. Household victimization and victimizations of those 12 and 13 years of age are reported on by the household respondent.

THE QUESTIONNAIRES FOLLOW

POLICE FOUNDATION/UNIVERSITY OF CINCINNATI
CRIME VICTIMIZATION/RDD STUDY

INITIAL CONTACT FORM – CITYWIDE SAMPLE

My name is _____ and I'm calling for the University of Cincinnati.

We're conducting a survey and we'd like your help. Could I speak to the man or lady of the house. (REPEAT INTRO IF NECESSARY)

1. Do you live inside the Cincinnati city limits?

   Yes – CONTINUE
   No – TERMINATE. Thank you for your time. We're only interviewing Cincinnati residents.

INITIAL CONTACT FORM – POLICE DISTRICT 1 SAMPLE

My name is _____ and I'm calling for the University of Cincinnati.

We're conducting a survey and we'd like your help. Could I speak to the man or lady of the house. (REPEAT INTRO IF NECESSARY)

Could you tell me what neighborhood you live in?

If DOWNTOWN
MT. ADAMS
QUEENSGATE
WESTEND
→ GO TO LEAD-IN

If CAMP WASHINGTON
CLIFTON
CLIFTON HEIGHTS
CORRYVILLE
FAIRMOUNT
MT. AUBURN
UNIVERSITY HEIGHTS
UNIVERSITY OF CINCINNATI
WALNUT HILLS
→ TERMINATE – I'm sorry to have bothered you, we're only interviewing downtown residents. Thank you for your time.

If FAIRVIEW
FAIRVIEW-CLIFTON HEIGHTS
MOHAWK
BRIGHTON

PROBE – Do you live *north* of McMicken Ave or the Western Hills Viaduct?

1. Yes – TERMINATE
2. No – GO TO LEAD-IN

If FINDLAY
OVER-THE-RHINE

PROBE – Do you live *north* of McMicken Ave?

1. Yes – TERMINATE
2. No – Do you live west of Vine St?
   a. Yes – GO TO LEAD-IN
   b. No – Do you live *north* of Liberty?
      1. Yes – TERMINATE
      2. No – CONTINUE

LEAD-IN: Could you tell me how many persons twelve and older live in this household? #_____

GO TO HOUSEHOLD RESPONDENT QUESTIONNAIRE

2. RECORD SEX OF RESPONDENT

(26) 1. Male
2. Female

3. Do you own or are you renting your home?

(27) 1. Owned or being bought
2. Rented for cash
3. No cash rent

4. Are you the head of this household?

(28) 1. Yes – CIRCLE "1" ON Q5, GO TO Q6
2. No

5. What is your relationship to the head of the household?

(29) 1. Head
2. Wife of Head
3. Child of Head
4. Other relative
5. Non-relative

6. How long have you lived at this address?

(30) 1. Less than a year
2. 1-2 years
3. 3-5 years
4. More than 5 years or never moved

7. Where were you born?

(31-32) State: _____ State Code: _____
(33-34) County: _____ County Code: _____

8. Where was your father born?

(35-36) State: _____ State Code: _____
(37-38) County: _____ County Code: _____

9. Now, I'd like to get your opinions about crime. Within the past year or two, do you think that crime in your neighborhood has increased, decreased, or remained about the same?

(39) 1. Increased
2. Same
3. Decreased

(42)
1. Very safe
2. Reasonably safe
3. Somewhat unsafe
4. Very unsafe

12. Would you say, in general, that your local police are doing a good job, an average job, or a poor job?

(43)
1. Good
2. Average
3. Poor
4. Don't know

13. Now, I'd like to ask some questions about crime. They refer only to the last 12 months, between April 1, 1973 and March 31, 1974. During the last 12 months, did anyone break into your (apartment/home), garage, or another building on your property?

1. Yes
2. No

(44) (If YES) How many times? _____

14. (Other than the incident(s) just mentioned) Did you find a door jimmied, a lock forced, or any other signs of an ATTEMPTED break in?

1. Yes
2. No

(45) (If YES) How many times? _____

15. Was anything at all stolen that is kept outside your home, or happened to be left out, such as a bicycle, a garden hose, or lawn furniture? (Other than any incidents already mentioned?)

1. Yes
2. No

(46) (If YES) How many times? _____

16. What was the total number of motor vehicles (cars, trucks, etc.) owned by you or any other member of this household during the last 12 months?

(47)
0. None – GO TO Q19
1. One
2. Two
3. Three
4. Four or more

17. Did anyone steal, TRY to steal, or use (it/any of them) without permission?

force, such as by a stickup, mugging or threat.

1. Yes
2. No

(51) (If YES) How many times? _____

21. Did anyone TRY to rob you by using force or threatening to harm you? (Other than any incidents already mentioned?)

1. Yes
2. No

(52) (If YES) How many times? _____

22. Did anyone beat you up, attack you or hit you with something, such as a rock or bottle? (Other than any incidents already mentioned?)

1. Yes
2. No

(53) (If YES) How many times? _____

23. Were you knifed, shot at, or attacked with some other weapon by anyone at all? (Other than any incidents already mentioned?)

1. Yes
2. No

(54) (If YES) How many times? _____

24. Did anyone THREATEN to beat you up or THREATEN you with a knife, gun, or some other weapon, NOT including telephone threats? (Other than any incidents already mentioned?)

1. Yes
2. No

(55) (If YES) How many times? _____

25. Did anyone TRY to attack you in some other way? (Other than any incidents already mentioned?)

1. Yes
2. No

(56) (If YES) How many times? _____

26. During the last 12 months, did anyone steal things that belonged to you from inside any car or truck, such as packages or clothing?

1. Yes

1. Yes
2. No

(60) (If YES) How many times? _____

Keypunching Note: START NEW CARD

(1-4) Household number: _____
(5) Card number: 2
(6) Person number: 1

---

30. Did you call the police during the last 12 months to report something that happened to you which you thought was a crime? (Do not count any calls made to the police concerning the incidents you have just told me about.)

1. No
2. Yes – What happened? _____

_____

CHECK ITEM A: Was a household member twelve or older attacked or threatened, or was something stolen or an attempt made to steal something that belonged to him?

1. No
(7) 2. Yes – How many times? _____

---

31. Did anything happen to you during the last 12 months which you thought was a crime, but did NOT report to the police?

1. No
2. Yes – What happened? _____

_____

CHECK ITEM B: Was a household member twelve or older attacked or threatened, or was something stolen or an attempt made to steal something that belonged to him?

1. No
(8) 2. Yes – How many times? _____

32a. How many differenct telephone numbers (NOT EXTENSION PHONES) can your household be reached on?

1. One
(9) 2. Two
3. Three or more

08. Other (Specify: _____)
98. Don't know

33. What is the highest grade (or year) of regular school you have ever attended?

(14-15)
_____ 00 Never attended or just kindergarten
_____ 01-08 Elementary grades
_____ 09-12 High school grades
_____ 21-26+ College

34. Did you complete that year?

(16)
1. Yes
2. No

35. What is your date of birth?

(17-22) _____ - _____ - _____
(month) (day) (year)

36. What race are you a member of?

(23)
1. White (includes Spanish)
2. Black
3. Other (Specify: _____)

37. Could you tell me approximately what your family income was for the last twelve months?

(24-25)

1. Under $1000
2. $1000 – $1999
3. $2000 – $2999
4. $3000 – $3999
5. $4000 – $4999
6. $5000 – $5999
7. $6000 – $7499
8. $7500 – $9999
9. $10000 – $11999
10. $12000 – $14999
11. $15000 – $19999
12. $20000 – $24999
13. $25000 and over

38a. Could you tell me what your address is?

(26-31) _____ CHECK CODER:

Census Tract: _____

(32-34) _____ Census Block: _____

(35-36) Cincinnati, Ohio 452

1. Yes – FILL IN CRIME INCIDENT REPORT(S)
2. No

You mentioned earlier that there are _____ members of your household twelve or older. Could you give me JUST their first names and their ages.

IF NONE 12 or 13 – Ask to interview whoever is available. Use PERSONAL SCREEN.

IF SOMEBODY 12 or 13 – Ask household respondent to answer PERSONAL SCREEN for them. Then, proceed to other household members.

SUPERVISOR CHECK ITEM:

Is the address in Census Tract 2, 3.01, 3.02, 4, 5, 6, 7, 9, 10, 11, 12, 13, 14, 15, or 16?

1. Yes – CONTINUE WITH CALL BACKS
2. No – Put in special "outside PD1 file." Do not make call backs.

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CRIME VICTIMIZATION/RDD STUDY

PERSONAL SCREEN

(1-4) Household number: _____
(5) "3"
(6) Person number: _____

(7-10) Date of Interview: - - 1974

(11-15) Time of Interview: _____ AM-1 PM-2

(16-17) Interviewer no: _____

My name is _____ and I'm calling for the University of Cincinnati. We're conducting a survey and I'd like to ask you a few questions.

1. RECORD SEX OF RESPONDENT (Or person being responded for)

(18)
1. Male
2. Female

to be of some concern to people. Within the past year or two, do you think that crime in your neighborhood has increased, decreased, or remained about the same?

(27)
1. Increased
2. Same
3. Decreased
4. Don't know
5. Haven't lived here that long

6. Within the past year or two, do you think that crime in the United States has increased, decreased, or remained about the same?

(28)
1. Increased
2. Same
3. Decreased
4. Don't know

7a. How safe do you feel or would you feel being out alone in your neighborhood AT NIGHT – very safe, reasonably safe, somewhat unsafe, or very unsafe?

(29)
1. Very safe
2. Reasonably safe
3. Somewhat unsafe
4. Very unsafe

7b. How about DURING THE DAY – how safe do you feel or would you feel being out alone in your neighborhood?

(30)
1. Very safe
2. Reasonably safe
3. Somewhat safe
4. Very unsafe

8. Would you say, in general, that your local police are doing a good job, an average job, or a poor job?

(31)
1. Good
2. Average
3. Poor
4. Don't know

9. The following questions refer only to things that happened to you during the last 12 months–between April 1, 1973 and March 31, 1974. Did you have your (pocket picked/purse snatched)?

1. Yes
2. No

(32) (If yes) How many times? _____

10. Did anyone take something (else) directly from you by using

13. Were you knifed, shot at, or attacked with some other weapon by anyone at all? (Other than any incidents already mentioned?)

1. Yes
2. No

(36) (If yes) How many times? _____

14. Did anyone THREATEN to beat you up or THREATEN you with a knife, gun, or some other weapon, NOT including telephone threats? (Other than any incidents already mentioned?)

1. Yes
2. No

(37) (If yes) How many times? _____

15. Did anyone TRY to attack you in some other way? (Other than any incidents already mentioned?)

1. Yes
2. No

(38) (If yes) How many times? _____

16. During the last 12 months, did anyone steal things that belonged to you from inside any car or truck, such as packages or clothing?

1. Yes
2. No

(39) (If yes) How many times? _____

17. Was anything stolen from you while you were away from home, for instance at work, in a theater or restaurant, or while traveling?

1. Yes
2. No

(40) (If yes) How many times? _____

18. Was anything (else) stolen from you during the last 12 months? (Other than any incidents you've already mentioned?)

1. Yes
2. No

(41) (If yes) How many times? _____

19. Did you find any evidence that someone ATTEMPTED to steal something that belonged to you? (Other than any incidents already mentioned?)

1. Yes

1. No
2. Yes – What happened? _____

_____

CHECK ITEM B: Was a household member twelve or older attacked or threatened, or was something stolen or an attempt made to steal something that belonged to hime?

1. No
(44) 2. Yes – How many times? _____

---

22. What is the highest grade (or year) of regular school you have ever attended? (RECORD ACTUAL GRADE)

(45-46)
_____ 00 Never attended or just kindergarten
_____ 01-08 Elementary grades
_____ 09-12 High school grades
_____ 21-26+ College

23. Did you complete that year?

(47)
1. Yes
2. No

24. What race are you a member of?

(48)
1. White (includes Spanish)
2. Black
3. Other _____

CHECK ITEM C:
Did you receive any "YES'S" to the Screen Questions (Q9-Q21) asked of this respondent?

1. No—Conduct PERSONAL SCREEN questionnaire with next household member. End interview if last respondent.
2. Yes—Fill-out CRIME INCIDENT REPORT(S).

POLICE FOUNDATION/UNIVERSITY OF CINCINNATI
CRIME VICTIMIZATION/RDD STUDY

CRIME INCIDENT REPORT

(1-4) Household number: _____
(5) Deck No. 4
(6) Person number: _____

other building on property. SKIP TO Q5a
2. At or in vacation home, hotel/motel.
3. Inside commercial building such as store, restaurant, bank, gas station, public conveyance or station. ASK Q4
4. Inside office, factory, or warehouse.
5. Near own home; yard, sidewalk, driveway, carport.
6. On the street, in a park, field, playground, school grounds or parking lot. SKIP TO CHECK ITEM A
7. Other. (Specify: _____)

4. Did the person(s) steal or try to steal anything from the store, restaurant, office, factory, etc.?

(16)
1. Yes
2. No – SKIP TO CHECK ITEM A
3. Don't know

5a. Did the person(s) live there or have a right to be there, such as a guest or a workman?

(17)
1. Yes – SKIP TO CHECK ITEM A
2. No
3. Don't know

5b. Did the person(s) actually get in or just try to get in the building?

(18)
1. Actually got in
2. Just tried to get in
3. Don't know

5c. Was there any evidence, such as a broken lock or broken window, that the person (forced his way in/TRIED to force his way in) the building?

(19)
1. No
8. Don't know

2. Yes—What was the evidence? Anything else?

(20) a. Broken lock or window mentioned. 1. Yes 2. No

(21) b. Forced door or window mentioned. 1. Yes 2. No

(22) c. Slashed screen mentioned. 1. Yes 2. No

(23) d. Other. (Specify: _____) 1. Yes 2. No

CHECK ITEM A:

Was any member of this household present when this incident occurred? (If not sure, ASK)

6c. Did the person(s) hit you, knock you down, or actually attack you in some other way?

(30) 1. Yes – SKIP to 6g
2. No

6d. Did the person(s) threaten you with harm in any way?

(31) 1. No – SKIP TO 6f
2. Yes

6e. How were you threatened? Any other way? (Mark all that are mentioned.)

(32) a. Verbal threat of rape. 1. Yes 2. No
(33) b. Verbal threat of attack (other than rape). 1. Yes 2. No
(34) c. Weapon present or threatened with weapon. 1. Yes 2. No
(35) d. Attempted attack with weapon (e.g., shot at). 1. Yes 2. No
(36) e. Object thrown at person. 1. Yes 2. No
(37) f. Followed, surrounded. 1. Yes 2. No
(38) g. Other. (Specify: _____) 1. Yes 2. No

SKIP TO 8a

6f. What actually happened? Anything else? (Mark all that are mentioned.)

(39)	a.	Something taken without permission.	1. Yes 2. No
(40)	b.	Attempted or threatened to take something.	1. Yes 2. No
(41)	c.	Harrassed, argument, abusive language.	1. Yes 2. No
(42)	d.	Forcible entry or attempted forcible entry of house.	1. Yes 2. No
(43)	e.	Forcible entry or attempted forcible entry of car.	1. Yes 2. No
(44)	f.	Damaged or destroyed property	1. Yes 2. No
(45)	g.	Attempted or threatened to damage or destroy property	1. Yes 2. No
(46)	h.	Other. (Specify: _____)	1. Yes 2. No

SKIP TO 8a

6g. How did the person(s) attack you? Any other way? (Mark all that are mentioned.)

(47)	a.	Raped	1. Yes 2. No
(48)	b.	Tried to rape.	1. Yes 2. No
(49)	c.	Shot, knifed, hit with	

after the attack?

(62) 1. No – SKIP TO 8a
2. Yes

7c. Did you receive any treatment at a hospital?

(63) 1. No
2. Emergency room treatment ONLY.
(64-65) 3. Stayed overnight or longer–How many days? ___

8a. Was something stolen or taken without permission that belonged to you or others in the household? (INTERVIEWER–If respondent was the owner or employee of a store or other commercial establishment, do not include anything stolen from the business itself, such a merchandise or cash from a register.)

(66) 1. Yes – SKIP TO 8f
2. No

8b. Did the person(s) ATTEMPT to take something?

(67) 1. No – SKIP TO 8e
2. Yes

8c. What did they try to take? Anything else? (Mark all that are mentioned.)

(68)	a.	Purse	1. Yes 2. No
(69)	b.	Wallet or money	1. Yes 2. No
(70)	c.	Car	1. Yes 2. No
(71)	d.	Other motor vehicle	1. Yes 2. No
(72)	e.	Part of car (hubcap, tapedeck, etc.)	1. Yes 2. No
(73)	f.	Other (Specify: _____)	1. Yes 2. No

CHECK ITEM B:

Was a. or b. mentioned in 8c?

(74) 1. No – SKIP TO 12a
2. Yes

Keypunching Note: START NEW CARD

(1-4)	Household number: _____	CHECK CODER
(5)	Card number: 5	
(6)	Person number: _____	FILL IN
(7)	Incident number: _____	

8d. Was the (purse/wallet/money) on your person, for instance in a

and/or
Property: (Mark all that apply.)

(24)	a. Purse	1. Yes	2. No
(25)	b. Wallet	1. Yes	2. No
(26)	c. Car	1. Yes	2. No
(27)	d. Other motor vehicle	1. Yes	2. No
(28)	e. Part of car (hubcaps, tape-deck, etc.)	1. Yes	2. No
(29)	f. Other. (Specify: _____)	1. Yes	2. No

CHECK ITEM C:
Was cash, a purse or wallet taken?

(30)
1. No – SKIP TO 10
2. Yes

---

9. Was the (purse/wallet/money) on your person, for instance in a pocket or being held by you when it was taken?

(31)
1. Yes
2. No

CHECK ITEM D:

Was only cash taken? (Q8f – Cash only)

(32) 1. Yes – SKIP TO 11a
2. No

10. Altogether, what was the value of the PROPERTY that was taken?

(33-39) $ _____ .00

11a. Was all or part of the stolen money or property recovered, except for anything received from insurance?

(40) 1. None — SKIP
2. All — TO 12a
3. Part

11b. What was recovered?

(41-47) Cash: $ _____ .00 (Skip to 12a if cash only recovered.)

and/or

Property: (Mark all that are mentioned.)

(48) a. Purse 1. Yes 2. No

12c. How much would it cost to repair or replace the damaged item(s)?

(63-69) $ _____.00 SKIP TO 13
9999999 DK

12d. How much was the repair or replacement cost?

(70-76) $ _____.00

13. Were the police informed of this incident in any way?

(77)
1. No
2. Don't know
3. Yes

CHECK ITEM E:

Briefly summarize this incident.

_____

_____

_____

_____

14. Were other members of your household present when this incident occurred?

(78)
1. No
2. Yes – Be sure you have an Incident Report for each household member 12 years of age or over who was robbed, harmed, or threatened in this incident.

CHECK ITEM F:
Is this the last incident reported by this person?

1. YES – Proceed to other household members with PERSONAL SCREEN
2. NO – Fill in additional INCIDENT REPORT(S)

APPENDIX C

HOW MANY CALL-BACKS ARE ENOUGH?

As demonstrated in Chapter 4, the RDD sample of the Cincinnati population is representative. Yet the data in Table 28 show several important contrasts in composition of households reached on the initial dialing and those reached only after repeated efforts. Had no call-backs been made to no-answers, the citywide sample would have included too few whites, college graduates, 20- to 34-year-olds, renters, small families, and high-income respondents. To achieve a representative sample, therefore, it is obvious that call-backs are necessary. The question then becomes, "How many call-backs?"

The data in Table 29 demonstrate that if the RDD interviewers had stopped at two call-backs, the sample still would have contained too few representatives of the groups described as missed above.

There are too few households in the three, four, five, and six call-

TABLE 28
CALL-BACK RESPONDENTS' DEMOGRAPHIC CHARA
RDD CITYWIDE SAMPLE

CHARACTERISTIC	PERCENTAGE	
	Respondents reached on first dialing attempt	Respon requi 1-6 call-
Sex		
Males	41.9	44.
Females	58.1	55.
	(N = 1,010)	(N = 6
Race		
Whites and others	71.7	78.
Blacks	28.3	21.
	(N = 1,005)	(N = 6
Education		
8 grades or less	27.0	14.
9–12 grades	49.1	47.
Some college or more	23.9	38.
	(N = 990)	(N = 6

9.6	5.8	χ^2 = 45.38
9.9	8.2	df = 5
28.9	41.4	SIGNIFICANT
15.8	16.3	at .01
17.4	18.4	
18.4	9.9	
(N = 1,001)	(N = 637)	
44.7	33.2	χ^2 = 10.81
55.3	66.8	df = 1
(N = 460)	(N = 340)	SIGNIFICANT
		at .01
51.5	41.3	χ^2 = 19.21
36.7	34.7	df = 3
8.1	19.6	SIGNIFICANT
3.7	4.4	at .01
(N = 365)	(N = 271)	

Persons 12+ in Household

:s	Mean = 2.21 persons	(N = 460)
ks	Mean = 1.91 persons	(N = 340)

t = 3.75 df = 798 SIG at .01

TABLE 29
CALL-BACK RESPONDENTS' DEMOGRAPHIC
RDD CITYWIDE SAMPLE

CHARACTERISTIC	PERCENTAGE	
	Respondents reached by 0-2 call-backs	Respon requi 3-6 call
Sex		
Males	42.6	48.
Females	57.4	51.
	(N = 1,515)	(N = 1
Race		
White	73.5	83.
Black	26.5	16.
	(N = 1,507)	(N = 1
Education		
8 grades or less	23.3	8.
9–12 grades	48.7	46.
Some college or more	28.0	44.
	(N = 1,488)	(N = 1

8.6	3.4	χ^2 = 31.03
9.7	5.3	df = 5
31.8	54.3	SIGNIFICANT
16.1	14.8	at .01
18.2	12.4	
15.6	9.8	
(N = 1,499)	(N = 138)	
41.9	24.7	χ^2 = 8.58
58.1	75.3	df = 1
(N = 710)	(N = 84)	SIGNIFICANT
		at .01
47.5	44.6	χ^2 = 4.13
36.3	31.2	df = 3
12.2	20.8	n.s. at .01
3.9	3.5	
(N = 565)	(N = 70)	

Persons 12+ in Household

Mean = 2.13 (N = 716)
Mean = 1.70 (N = 84)

t = 3.04 df = 798 SIG at .01

TABLE 30
FURTHER BREAKDOWN ON CALL-BACK RESPONDENTS' DEM
RDD CITYWIDE SAMPLE

CHARACTERISTIC	PERCENTAGE		
	Number of call-backs required to		
	0-2	3	4
Sex			
Males	42.6	50.6	40.9
Females	57.4	49.4	59.1
	(N = 1,515)	(N = 92)	(N = 23)
Race			
Whites and others	73.5	79.0	95.5
Blacks	26.5	21.0	4.5
	(N = 1,507)	(N = 92)	(N = 23)
Education			
8 grades or less	23.3	10.6	4.5
9–12	48.7	45.2	45.5
Some college or more	28.0	44.2	50.0
	(N = 1,488)	(N = 93)	(N = 23)

8.6	5.0	0.0	0.0	0.0
9.7	5.6	0.0	11.4	0.0
31.8	54.1	54.5	60.0	28.6
16.1	14.7	13.6	11.4	42.9
18.2	12.8	13.6	5.7	28.6
15.6	7.8	18.2	11.4	0.0
= 1,499)	(N = 93)	(N = 23)	(N = 18)	(N = 4)
41.9	31.4	10.5	26.1	0.0
58.1	68.6	89.5	73.9	100.0
= 710)	(N = 50)	(N = 20)	(N = 12)	(N = 3)
47.5	33.0	64.7	53.0	60.0
36.3	43.2	23.5	0.0	0.0
12.2	20.3	5.9	47.0	40.0
3.9	3.4	5.9	0.0	0.0
= 565)	(N = 41)	(N = 18)	(N = 9)	(N = 3)

Persons 12+ in Household

<s	2.13	(N = 716)
<s	1.94	(N = 50)
<s	1.21	(N = 20)
<s	1.61	(N = 12)
<s	1.4	(N = 3)

Six call-backs	3
	800

fore, that a break point comes after three calls. Balancing the improvement of representativeness of the sample on the one hand with the diminishing returns on the other, it would seem most productive to make between three and five no-answer call-back attempts. As indicated in the text, no optimum limit has been placed on the number of call-backs required to complete all interviews from the household.

APPENDIX D

FINAL DISPOSITION OF ALL DIALINGS IN RDD

It has been pointed out that in random digit dialing, many telephone numbers selected proved to be not in service or to belong to businesses, while others were located in phone booths or in unoccupied dwellings or offices. The distribution of all calls made in the random digit dialing survey is presented in Table 32.

TABLE 32
FINAL DISPOSITIONS OF ALL DIALINGS IN RDD SURVEY

DISPOSITION	PERCENTAGE	
	Citywide Sample	P.D. No. 1 Sample

APPENDIX E

CRIME CODING INFORMATION AND PROGRAM

The kinds of criminal victimizations being measured by LEAA-Census are various forms of common theft and interpersonal assault. The descriptions of these types of crimes differ from those the Federal Bureau of Investigation uses in conjunction with its Uniform Crime Reports (UCR), not only in the labels used, but also in the method of classification.

In the LEAA-Census surveys, the interviewer recorded a complete description of criminal victimization reported during the interview. This description is computer-coded according to the LEAA-Census classifications, based on the presence or absence of certain elements in the incident. Because this description identifies various aspects of the crime, the LEAA-Census classification scheme can be used to show

Type of Crime	Conditions
Assaultive Violence	
With Theft	Theft or Attempted Theft or Commercial Theft [1]
Rape	Rape — method of attack or type of injury
Attempted rape	Verbal threat of rape or attempted rape as method of attack or attempted rape injuries
Serious assault	
With weapon	Weapon present and any injury
No weapon	No weapon[2] and serious injury[3] or
	No weapon[3], other injury[4] and hospitalized Not hospitalized two or more days
Minor assault	No weapon[2], attacked, and minor injury[5] or
	No weapon[2], attacked, other injury[4] and not hospitalized two or more days

No theft and no attempted theft and no commercial theft[1]	8a = 2, 8b = 1 and 4 ≠ 1
Rape – method of attack or type of injury	6ga = 1 or 7a.2a = 1
Verbal threat of rape or attempted rape as method of attack or injury	6ea = 1 or 6gb = 1 or 7a.2b = 1
Weapon present and any injury	6a.3a or 6a.3b or 6a.3c = 1 and 7a.2c or 7a.2d or 7a.2e or 7a.2f or 7a.2g = 1
No weapon[2] and serious injury[3] or	6a = 1 or 2 and 7a.2c or 7a.2d or 7a.2e = 1 or
No weapon[2], other injury[4] and hospitalized two or more days	6a = 1 or 2, 7a.2g = 1 and 7c.3 = two or more days
Weapon present and threatened or	6a.3a or 6a.3b or 6a.3c = 1 and 6d = 2 or
Weapon present, attacked, and no injury	6a.3a or 6a.3b or 6a.3c = 1, 6c = 1 and 7a = 1
No weapon[2], attacked, and minor injury[5] or	6a = 1 or 2, 6c = 1, and 7a.2f = 1 or
No weapon[2], attacked, other injury[4] and not hospitalized two or more days	6a = 1 or 2, 6c = 1, 7a.2g = 1, and 7c.3 ≠ two or more days

PERSONAL AND PROPERTY CRIMES (continued

Type of Crime	Conditions
Attempted Assault, no Weapon	No weapon[4] and threatened or No weapon[2], attacked and no injury
Personal Theft Without Assault	Theft or attempted theft[6]
Robbery	Theft
With weapon	Weapon present
No weapon	No weapon[2] and threatened or No weapon[2], attacked and no injury
Attempted robbery	Attempted theft
With weapon	Weapon present
No weapon	No weapon[2] and threatened or No weapon[2], attacked and no injury
Purse snatch, no force	No weapon[2], not attacked, not threatened, purse taken, and property on person
Attempted purse snatch, no force	No weapon[2], not attacked, not threatened, attempt to take purse, and property on person

No weapon[2], not attacked, not threatened, property on person, and cash or wallet taken	6a = 1 or 2, 6c = 2, 6d = 1, 9 = 1 and 8fb = 1 or 8f = $1-9999
Property Crimes	
No right to be in home, etc.	5a = 2 or 3
Entered and evidence of force	5b = 1 and 5c.2a or 5c.2b or 5c.2c or 5c.2d = 1
No theft	8a = 2
Something damaged	12a = 2
Nothing damaged	12a = 1
Theft	8a = 1
Entered and no evidence of force[7]	5b = 1 and 5c = 1 or 8
Tried to get in and evidence of force	5b = 2 or 3 and 5c.2a or 5c.2b or 5c.2c or 5c.2d = 1
Theft except motor vehicle or attempted theft except motor vehicle	8f ≠ 0 or 8fa or 8fb or 8fe or 8ff = 1 and 8fc or 8fd ≠ 1 or 8ca or 8cb or 8ce or 8cf = 1 and 8cc or 8cd ≠ 1
Sum of stolen cash and property value = $0-$49	8f + 10 = $0-$49
Sum of stolen cash and property value = $0-$9	8f + 10 = $0-$9

Type of Crime	Conditions
$10-$24	Sum of stolen cash and property value = $10-$24
$25-$49	Sum of stolen cash and property value = $25-$49
$50 or more	Sum of stolen cash and property value = $50-$19,998
$50-$99	Sum of stolen cash and property value = $50-$99
$100-$249	Sum of stolen cash and property value = $100-$249
$250 or more	Sum of stolen cash and property value = $250-$19,998
NA amount	Amount of stolen cash NA or value of stolen property NA
Attempted larceny	Attempted theft except motor vehicle
Auto Theft	
Theft of car	Theft of car and no permission or permission, not returned

Theft of other motor vehicle and no permission or permission, not returned	8fd = 1
Attempted theft of car	8cc = 1
Attempted theft of other motor vehicle	8cd = 1

theft.

must be present in order to know if there was a weapon.

ls, broken bones, teeth knocked out, internal injuries, or knocked unconscious.

d severity.

, scratches, swelling.

ttempted commercial theft. Victim must be present.

orce.

Rape	Rape with theft Attempted rape with theft Rape without theft Attempted rape without theft
Aggravated Assault	Serious assault without theft Attempted assault with weapon without theft
Armed Robbery	Serious assault with theft with weapon Robbery, no assault, with weapon Attempted robbery, no assault, with weapon
Unarmed Robbery	Serious assault, no weapon with theft Minor[b] assault with theft Robbery, no assault, no weapon Attempted robbery, no assault, no weapon
Simple Assault	Minor[b] assault without theft Attempted assault, no weapon, without theft
Larceny[a]	Purse snatch without force Attempted purse snatch without force Pocket picking

[a]UCR definition of larceny includes many more types of offenses than the personal confrontation crimes.

[b]Minor is defined to exclude weapons; presence of weapon automatically classifies assault as serious by NCP rules.

crime categorizations to achieve the appropriate UCR counterpart. LEAA publications use crime categories that are comparable with UCR definitions.

It should also be noted that some types of crimes may be classified as such by meeting only one set of two or more possible sets of conditions. When this is the case, the underlined word *or* is used to separate the different sets of conditions which may be met. The word *and* is underlined when there are no alternative sets of conditions and, instead, all of the conditions stated must be present in the incident description for that classification.

The RDD study followed exactly the same procedures. The column labeled "RDD Survey Codes" denotes the location on the questionnaire incident report form of the conditions which must be met for that classification. The source code refers to the circled number to the left of a question on the incident report form and the entry numbers for each source code correspond to the question's precoded response categories. Thus, the source code entries are used as a basis for classifying the description of an incident into a technical definition.

CRIME AND SERIOUSNESS CODING PROGRAM

```
C PROGRAM TO PRODUCE SERIOUSNESS CODES, NET LOSS CODES, AND CRIME CODES
C FROM RDD INCIDENT REPORTS.
C CODES PRODUCED FROM CENSUS CRIME SURVEY DEFINITIONS.
C
C
      INTEGER CCODE, ASSLT, WEAPON, THEFT, COMTFT, FORCE, PROP, SCODE,
```

```
      IF((CARD1(57).EQ.1).OR.(CARD1(58).EQ.1).OR.(CARD1(59).EQ.1))
     #INJRY=1
C CALCULATE CRIME CODE FOR SEX OFFENSE.

    5 IF((CARD1(47).EQ.1).OR.(CARD1(55).EQ.1)) CCODE=6
      IF((CARD1(48).EQ.1).OR.(CARD1(32).EQ.1).OR.(CARD1(56).EQ.1)) CCODE
     #=7
      IF (CCODE.EQ.99) GO TO 10
      IF(COMTFT.EQ.1) CCODE=CCODE-5
      GO TO 70
C CALCULATE STOLEN PURSE, WALLET, AND MONEY CRIME CODE.
   10 IF((WEAPON.NE.-1).OR.(ASSLT.NE.-1).OR.(INJRY.NE.-1)) GO TO 25
      IF(THEFT) 25,15,20
   15 IF((CARD2(8).EQ.1).AND.(CARD1(68).EQ.1)) CCODE=18
      IF(CCODE.EQ.18) GO TO 70
   20 IF(((CARD2(25).EQ.1).OR.(CARD2(17).NE.0)).AND.(CARD2(31).EQ.1))
     #CCODE=19
      IF((CARD2(24).EQ.1).AND.(CARD2(31).EQ.1)) CCODE=17
      IF(CARD1(24).NE.2) CCODE=32
      IF(CCODE.LT.32) GOT TO 70
      GO TO 60
C CALCULATE ASSAULT WITH THEFT CRIME CODE.
   25 IF(COMTFT.NE.1) GO TO 30
      IF((WEAPON.EQ.1).AND.(INJRY.NE.-1)) CCODE=3
      IF((WEAPON.EQ.-1).AND.(INJRY.EQ.1)) CCODE=4
      IF((WEAPON.EQ.-1).AND.(INJRY.EQ.0).AND.(ASSLT.EQ.1)) CCODE=5
      IF(CCODE.LT.6) GO TO 70
C CALCULATE PERSONAL THEFT CRIME CODE.
   30 IF(THEFT) 45,40,35
   35 IF((WEAPON.EQ.1).AND.(INJRY.EQ.-1)) CCODE=13
      IF((WEAPON.EQ.-1).AND.((ASSLT.EQ.0).OR.((ASSLT.EQ.1).AND.(INJRY
     #.EQ.-1)))) CCODE=14
      IF(CCODE.LT.20) GO TO 70
      GO TO 45
   40 IF((WEAPON.EQ.1).AND.(INJRY.EQ.-1)) CCODE=15
      IF((WEAPON.EQ.-1).AND.((ASSLT.EQ.0).OR.((ASSLT.EQ.1).AND.(INJRY
     #.EQ.-1)))) CCODE=16
      IF(CCODE.LT.20) GO TO 70
   45 IF(ASSLT) 60,55,50
C CALCULATE ASSAULT WITHOUT THEFT CRIME CODE.
   50 IF((WEAPON.EQ.1).AND.(INJRY.NE.-1)) CCODE=8
      IF((WEAPON.EQ.-1).AND.(INJRY.EQ.1)) CCODE=9
      IF((WEAPON.EQ.-1).AND.((INJRY.EQ.0).AND.(ASSLT.EQ.1))) CCODE=10
      IF(CCODE.LT.20) GO TO 70
   55 NOI=0
      IF((ASSLT.EQ.0).OR.((ASSLT.EQ.1).AND.(INJRY.EQ.-1))) NOI=1
      IF((WEAPON.EQ.1).AND.(NOI.EQ.1)) CCODE=11
      IF((WEAPON.EQ.-1).AND.(NOI.EQ.1)) CCODE=12
      IF(CCODE.LT.20) GO TO 70
```

```
C CRIMES AGAINST PERSONS COMPLETED.
C CALCULATE BURGLARY CRIME CODES.
C CALCULATE EVIDENCE OF FORCE, 0=FORCE BUT NO ENTRY, 1= NO FORCE, 2=
C EVIDENCE OF FORCE
   60 NOE=0
      FORCE=-1
      IF((CARD1(17).NE.1).AND.(CARD1(18).EQ.1)) NOE=1
      IF((NOE.EQ.1).AND.(CARD1(19).NE.2)) FORCE=1
      IF((NOE.EQ.1).AND.(CARD1(19).EQ.2)) FORCE=2
      IF((CARD1(18).NE.1).AND.(CARD1(19).EQ.2)) FORCE=0
      PROP=-1
      IF(CARD2(61).EQ.2) PROP=1
C PROPERTY DAMAGE.
      NTE=0
      IF((THEFT.NE.1).AND.(FORCE.EQ.2)) NTE=1
      IF((NTE.EQ.1).AND.(PROP.EQ.1)) CCODE=20
      IF((NTE.EQ.1).AND.(PROP.NE.1)) CCODE=21
      IF(CCODE.LT.22) GO TO 70
      IF((THEFT.EQ.1).AND.(FORCE.EQ.2)) CCODE=22
      IF(FORCE.EQ.1) CCODE=23
      IF(FORCE.EQ.0) CCODE=24
      IF(CCODE.LT.25) GO TO 70
C CALCULATE CAR AND OTHER VEHICLE THEFT CRIME CODE.
      IF(CARD2(26).EQ.1) CCODE=33
      IF(CARD1(70).EQ.1) CCODE=35
      IF(CARD2(27).EQ.1) CCODE=34
      IF(CARD1(71).EQ.1) CCODE=36
      IF((CCODE.GE.33).AND.(CCODE.LE.36)) GO TO 70
C CALCULATE LARCENY CRIME CODE.
      IF(THEFT.NE.1) GO TO 66
      IF(NUM.LT.0) GO TO 65
      IF (NUM.GE.0) CCODE=25
      IF(NUM.GE.10)  CCODE=26
      IF(NUM.GE.25)  CCODE=27
      IF(NUM.GE.50)  CCODE=28
      IF(NUM.GE.100) CCODE=29
      IF(NUM.GE.250) CCODE=30
      IF(CCODE.LT.32) GO TO 70
   65 IF(NUM.EQ.-1)  CCODE=31
   66 IF(THEFT.EQ.0) CCODE=32
C CRIME CODES COMPLETE.
C CALCULATE SERIOUSNESS CODE.
   70 IF(INJRY.EQ.-1) GO TO 85
C INJURIES, BRUISES, MEDICAL ATTENTION, HOSPITAL.
      SCODE=SCODE+1
      IF(CARD1(62).EQ.2) SCODE=SCODE+3
      IF((CARD1(64).GE.1).AND.(CARD1(64).LT.99)) SCODE=SCODE+3
```

```
      IF(NTLOSS.GE.30000) NADD=6
      IF(NTLOSS.GE.80000) NADD=7
      SCODE=SCODE+NADD
C WRITE THE CODES ON OUT DEVICE 8
      WRITE(8,1200) (CARD1(I),I=1,15), CCODE, SCODE, NTLOSS
  170 GO TO 1
  175 CONTINUE
 1001 FORMAT(63I1,I2,16I1,I7,9I1,I7,I1,I7,6I1,I7,2I1,2I7)
 1200 FORMAT(15I1,2I2,I8)
      STOP
      END
```

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