

FINAL REPORT  
FOR  
ANALYTICAL STUDIES OF VICTIMIZATION BY CRIME  
USING NATIONAL CRIME SURVEY PANEL DATA

LEAA GRANT # 75SS-99-6013

49663

Yale University

Albert J. Reiss, Jr.  
Principal Investigator  
June 1, 1977

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ACQUISITIONS

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## Introduction

This final report is mainly a summary of previous quarterly reports of progress and of the technical and substantive reports submitted in connection with the proposed work. The report lacks the detail of the technical and substantive reports since these were transmitted previously. The goal of the final report is to summarize progress and accomplishments and problems and issues in three separate areas. First there are logistical problems associated with undertaking over time studies using the National Crime Survey (NCS). Some account of these is given here simply because they should be germane to future users of the NCS and for any attempt to revise the NCS. Second, some of the technical findings are summarized that we believe are important to analytical work using the NCS and for future changes in data collection and analysis of the NCS. Finally, attention is drawn to some of the major substantive findings of previous reports to demonstrate the importance of using the over-time design of the NCS to answer important questions about crime victimization. Each of the main sections is designated appropriately: (1) Logistical Problems; (2) Technical Findings; (3) Substantive Findings. The report covers the original grant period from October 24, 1974 to October 23, 1976 and its extension to April 21, 1977.

### Logistical Problems

There were several major guiding strategies behind the proposal to undertake analytical studies of crime victimization using the over-time data from the NCS. First, there was the central idea that causal phenomena might be better understood with over-time than with cross-section data. Second, there was the important set of problems relating to the ways the Census NCS panel design creates errors that affect the quantity and quality of the data for estimates and analyses. Finally, it was thought that important additional data could be added to the NCS panel from the 1970 Census, particularly the data on neighborhoods, and thus enhance the analysis situational proneness to victimization by crime.

Unfortunately, not all of the logistical problems encountered in this study were apparent either to the principal investigators or to those who had developed the data for the NCS or for the neighborhoods for the 1970 Census. It is to these problems that we now turn.

The Panel Design. The NCS was designed as a panel study of locations mainly for logistical reasons. First, it was believed that reports of victimization were subject to time telescoping effects and that interviews should bound the reporting period to reduce the effect of forward telescoping of crime events into the report period. To do so, the first interview is considered the bounding interview and only the second and subsequent interviews are used for estimation of victimization rates. This bounding procedure requires that households and individuals be followed over time and a longitudinal design is appropriate. Second, there are efficiencies in sampling and field survey operations with a panel design. It should be noted that both of these reasons ignore the possibility that important questions can be answered by following

locations, households, and persons over time. Rather, the decisions rested on finding an efficient design that would give the best estimate of the victimization for successive cross-sections of bounded interviews.

Since the Bureau of the Census which is responsible for designing and executing the survey, in large did not provide for data collection and reporting based on following location, households, and individuals over time, but rather successive cross-section estimation, the collection quarter data types also place important conditions on the creation of a longitudinal file. It is to the problems of design and implementation of it for the creation of analytical over-time files that we now turn.

Confidentiality & Disclosure. The Census Bureau assigns a unique identifier to each location and household, and a specific line to each person. The identification information is available on their data files in a form that makes it possible to identify at least some of the respondents if they have access to or acquire additional sources of information. The unique identifier provides information on the location that could be used in connection with other information on the tapes to track down or by other means uniquely identify a respondent. The rules of the Bureau of the Census do not permit, under law, sharing the identification with public users or research investigators who are not employees of the Bureau. Since part of the goal was to develop a users tape, it was necessary at the outset to eliminate the special identification.

Unfortunately this occasioned some delay not only because it was necessary to create an algorithm to provide identification of each location and household, but to provide new variable information as well on the location characteristics that are part of the original identifier, e.g., a variable of size of place. This turned out to be no small matter and contributed some delay in originally delivering the tapes.

Machine Readable Tapes. A second major problem arose in that the Census tapes had to be made compatible with the IBM data processing system currently available through the Yale University Computing Center. This required the preparation of a test tape as well. After we processed the test tape and found the conversion compatible, the collection quarter tapes were finally prepared by the Bureau of the Census.

The result was that we did not receive the ten collection quarter tapes (July 1, 1972 to December 31, 1974) until June, 1975, some eight months after the beginning of the project. Early analyses of the frequency of multiple victimization within that period of time convinced us we would need an additional year of the crime survey. This required waiting until all of the 1975 information was available to begin matching of the information for that time period. We acquired the last of this information only in May 1, 1976. The receipt of these last tapes occasioned additional delay in processing.

Adequate Tape Documentation. An other major problem faced was the availability of adequate documentaion for the information on the tapes. The Bureau of the Census was only in the process of developing a documentaion manual on the design data collection, and coding of the survey and of the variable information on the tapes when the study began. Most of the information was available to us only in a series of memos internal to the Bureau and these did not provide all relevant information. Thus during the first year of the study we spent considerable time in consulting with Bureau staff and in assembling and making operational that information.

By the close of the project period both a Survey Documentation Manual and a Coding Manual for the variables on the tape were available. The Bureau now plans for continuing update of the manuals since some changes are made from one period to the next. Helpful as are these manuals, anyone working with the NCS is aware that routinely there are working memoranda prepared by staff members of the NCS as well as small studies undertaken to evaluate field procedures, data collection, and survey design. To work effectively requires that one be in a line of communication with the staff to secure these memoranda. Fortunately, we were eventually able to work out such a relationship and it has proven invaluable in understanding many aspects of the NCS. Similarly, it took some time to develop liaison with the NCS but eventually contacts were made that proved most helpful, particularly with Ms. Linda Murphy of the Bureau of the Census Survey Division.

Creating the Panel Data File. Perhaps the most difficult problems arose in connection with creating the longitudinal or panel data file. The Bureau of the Census provided data tapes for collection quarters and our main task was to match locations, households, and persons across collection quarters, whenever elegendible for reinterview.

One of the most difficult problems faced in creating longitudinal data files is the problem of matching locations, households, and persons because of errors in a cross-section. This problem arises from the fact that neither Census nor LEAA originally planned for the data file to be utilized for panel analysis. Thus, error which can be tolerated in a cross-section can't be tolerated in a panel analysis. A simple example from our experience may demonstrate the difficulty faced that had to be resolved. Of the 98,228 location in the original file created from Census's 10 collection quarters, there were 4,192 locations for which household status and household number were inconsistent. For our panel analysis, it was essential that these inconsistencies be resolved. Lacking the possibility of returning to original data sources, we were foreed to indirect means for their correction, viz., consistency in the characteristics of households and persons within households.

To resolve the 4,192 inconsistencies by this means required, however, an examination of each inconsistency and its resolution according to stated rules. Almost all of our manpower for two months had to be given over to that task, including the time of our programmers. We believe

the procedures adapted resulted in a valid and reliable data set in this respect, it does raise questions about whether in the longrun Census and LEAA should not give more attention to preparing the data for panel analysis.

The Bureau of the Census has the capacity to provide a clean set of data for panel analysis if attention is given to the problem when the original data tapes are prepared. Once the data are organized by collection quarter, the capacity to correct panel errors is greatly diminished and there is considerable effort that must be expended for less satisfactory results. Note that it is not being suggested that all items on the survey must be error free, only that the items which create the panels be free from error.

In this connection, attention is drawn to certain practices that might well be changed if there is a serious interest in panel analysis. The first is that changes in sampling status should be better documented in the file. Two examples will be given to illustrate the point. (1) The size of the sample was reduced in 1973 for good and sufficient reasons. We are unable to separate subsequently those locations which were dropped because of decreasing the sample size from those locations which were dropped and added because of other reasons--given errors, etc. (for each collection period, there are new sample cases because of changes in demolition, new construction, etc.). (2) The Bureau drops panel cases in the old PSU's are dropped and substitute cases drawn in the new PSU's. The effect of such substitutions on the panel are considerable, since they substitute unbounded for bounded households and in each case a new household for one that is potentially a continuing household. Such decisions should perhaps not be made without considering their potential effects on the panel.

The Bounding Interview. To control time telescoping, as already noted, only six of the seven interviews taken with a given household respondent are eligible for estimating victimization rates. The first interview is a bounding interview used to control for events that might otherwise be telescoped into the second interview.

Bounding interviews were secured only for Samples 1 and 2 and not for Samples 3 and 4. This means that for some purposes we are unable to assess the effects of bounding, time-in-sample, number of interviews, and similar sources of error on estimates of victimization.

The bounding interviews, moreover, are not processed to eliminate some of the sources of error that are controlled in the regular interviews used for estimation. Thus, the bounding interview not only creates more errors on any variable, but they increase the difficulties of matching cases over time. With a regular program of assessing the sources of error in the NCS, more attention perhaps should be given to sources of error in the bounding interviews and provision made for matching them with first interviews the same as for all other interviews.

Creating the Hierarchical and Extract Files. Major problems arose when information from the cross-section data collection quarters were merged into the longitudinal file, bringing together information on the same locations, households and persons as cohorts that can be followed in time. To undertake the objectives of the survey, it was necessary to create hierarchical files of locations, households, and persons. This turned out to be a more difficult and time consuming task than originally anticipated.

Problems arose owing to the fact that errors in cross-section information are compounded in panel matching. A simple example may illustrate the magnitude of this problem. Of the 98,228 locations in the original file for 10 collection quarters, there were 4,192 locations for which household status and household number were inconsistent. Almost two months were spent in resolving these inconsistencies, much of that time spent in laborious matching routines.

But, as indicated previously, the original file of 10 collection quarters proved inadequate for two reasons. Given panel attrition and turnover, we had too few cases to follow the less frequent types of crime over time. The size of the data base, therefore, was increased to 14 collection quarters. The magnitude of that data base can be expressed as follows. The July 1, 1974 to December 31, 1975 file now contains information on 124,401 separate locations, 159,060 separate households, and 369,932 separate persons. Within the file there is a total of 919,056 person records of variable length. Given reduction in sample size by attrition, replacements, and the rotation design, there is the following distribution of records by location: 1 record, 17,535 locations; 2 records, 19,188 locations; 3 records, 23,810 locations; 4 records, 12,945 locations; 5 records, 12,398 locations; 6 records, 23,820 locations and 7 records, 14,705 locations.

The NCS has three data hierarchies: locations, households, and persons. In addition, the household and persons files have variable length records since there is considerable variation in size and composition of households and their rates of victimization as well as in the length of the crime incident record. Hierarchical files with variable length records pose major logistical problems, the more so when up to seven consecutive cross-section interviews are merged.

Given the nature of these logistical problems, there are special problems of programming and analysis routines adapted to hierarchical files with variable length records. For some purposes, therefore, it has been necessary to create Extract Files that are both hierarchical and rectangular. Three such files were created, one a Household Extract File that merges all records for households. A second is a Person Extract File that merges all records for each person. A Victimization Extract file was created for some additional analyses.

A number of minor problems arose in connection with converting

the data to panel files. For a number of variables, for example, the Census code changed over time. It was necessary to recode the information for these variables since Census did not always adopt new codes that could readily be merged with the old codes. Another kind of problem arose because Census reduced the size of the sample. For panel analysis we needed to separately identify those cases that were dropped to decrease sample size from those which were rotated into the sample for other reasons (replacements, for example). Such problems are not only time-consuming but they are not always resolved.

Programming & Analysis Routines. The hierarchical format compounds the file-size difficulties. The peculiar nature of hierarchy in the Victimization Survey files poses problems that are not resolved by existing software. Essentially, an incident in the NCS file may refer to either a household level or to persons in households. This seemingly minor divergence from the traditional concept of a branching major analysis routines. This caused us to spend considerable time in evaluating statistical packages and in searching for the best way to minimize this problem. It meant also that we were at the frontier of managing and analyzing variable length files.

A major contribution of the proposed study was to analyze victim proneness and multiple victimization. This required following households and persons over time and the panel design of NCS made this possible. But, there never had been a panel analysis for a survey as large as NCS. Although we knew that this might occasion new problems, we did not truly comprehend that not only is panel analysis uncharted in terms of practical problems of data management and methodological treatment (panel analysis has received some theoretical consideration in terms of statistical and experimental design problems), but that panel survey designs are largely uncharted in the areas of data collection and processing. Thus, the NCS panel faces data processing problems that are extremely troublesome.

The problem of developing analytical files from collection quarter files was no simple task since it faced us with a problem of finding statistical routines for minimizing the cost of data analysis. Most statistical packages are for small data bases and the NCS is a very large data base. Most available packages are either unable to handle the analytical problems of panel analysis with the hierarchical arrangement or they are too costly to operate. In addition, a number of packages, such as OSIRIS, have substantial errors. After considerable time spent in search of adequate programs that could be adapted to our use and still run economically, we settled on two such packages. The first is TPL (Table Producing Language) of the Bureau of Labor Statistics; the second is BMD, the Biomedical computer programs developed by U.C.L.A. These are both operational for the data base. In addition, Goodman's ECTA for multiple contingency analysis is operational. Each of these tasks involves more time than anticipated so that at least a year's extra time elapsed to reach full analytical capability. The files and capability we attained at this point reduce the time, however, that any other user



would have to invest in the future.

The magnitude of the NCS panel file means that costs of file construction, data management, and data processing exceeded those originally anticipated. To reduce costs of computer processing, considerably more effort had to be placed on software and the creation of multiple files for specific analytical purposes. These increased costs show up in three ways in our total costs. First, we have to retain a considerably greater computer programmer capability than originally anticipated. Rather than one programmer we had to retain the services of two and these cannot be dispensed with throughout the data processing phase since file construction and programming remain a continuing necessity. Second, the creation of multiple analytical files increases costs since each file must be built with the aid of the computer. Third, given increased volume in total number of cases, any given analytical output involves increased costs or production. Clearly, constructing and using the NCS panel file is both costly and time-consuming.

The Neighborhood Hierarchy. The Bureau of the Census merged their 1970 Neighborhood Characteristics Public Use Tape with each of the NCS data collection quarter tapes, thereby creating a fourth hierarchy--neighborhoods--for the analysis. Unfortunately, the Bureau provides very poor documentaion for the Neighborhood Characteristics tapes and it proved to be impossible to obtain much of the information on their creation and characteristics because of both lack of documentaion and concerns about disclosure of confidential information.

Basically, we learned that the neighborhood concept is a very loose one in Census terminology. Its size can range from whole counties to small areas of a city since it is basically a unit of 12,000 households, more or less. Moreover it is constructed from enumeration districts (ED's) at the time of the 1970 Census. Any new construction subsequent to the creation of the ED is not assigned to an ED.

This latter problem--assigning new construction to ED's and therefore to locations--proved to be especially troublesome. For the first waves of NCS collection quarters, about one in 10 locations is not assigned to a neighborhood. The problem is compounded with each update of the sample so that over the decade perhaps at least one in five locations will be without a neighborhood identification by this procedure.

#### Technical Contributions

Technical Report # 1: "Some Suggestion for Changes in NCS-1 and NCS-2 Questionnaire of the National Crime Survey". (Staff Report, September, 1976).

This report suggests revisions of the current NCS questionnaire and of the data tapes prepared from the questionnaire. The suggestions stem primarily from an interest in expanding the analytical possibilities for

panel or over-time analyses of the data from the National Crime Survey, a major objective of this project. Some of the suggestions are not essential for deriving cross-sectional estimates of victimization by crime, the major objective of the NCS survey.

The report recommends both changes in the regular biennial survey and the use of supplements of the NCS at periodic intervals. Suggestions on coding and editing are based on limited information as to when particular edit procedures take place.

Suggestions for Dealing with Problems of Maintaining Location, Household, Person, and Incident Records Over Time. The logic of any panel analysis requires that once any sampling unit--a location--enters a panel or rotation group for a given period of time, there must be some information on that unit for every data collection point during the entire period of time. A record of the status of the unit at every point that additional observations are made is the minimum standard.

For the first three and one-half years of data collection for the NCS, households, locations, and persons sometimes appear and disappear from a rotation group without information on their status. This creates difficulties not only in matching locations--and therefore households and persons across data collection periods--but also problems in determining reasons for their appearance or disappearance for a rotation group. The following changes are proposed so that cases can be matched across collection quarters and so that their status can be determined for any/all collection quarters.

First, there should be a master list of all location identifiers in a rotation group, and for each collection quarter the identifier for each location should be checked against the master list. If we assume a 0.5% error rate of assignment of location identifier in the field, every collection quarter, about 150 locations, would have such errors that would have to be resolved. This should be fairly easy to do at the clerical stage of editing. Since one can also expect errors to arise in preparing data for processing, there should be a final computer edit to compare location identifier fields with the master list. Where discrepancies arise, it should be relatively easy to resolve them if interview schedules and coding sheets are retained until the computer edit has taken place and discrepancies resolved.

Second, the NCS questionnaire and procedures should include a question or field that provides information on whether a new location enters a rotation group as 1) a rotation in, 2) an extra household, 3) a merged household (if location identifiers are changed), 4) a location dropped to reduce sample size, or 5) an added location due to updating a sampling lists. Such information would greatly facilitate cross-panel editing. Likewise, if merged households are created without changing location identifiers, there should be some way of identifying such mergers.

Third, there should always be one household record for each 6-month panel wave for each location through a full rotation. If a location leaves a panel-rotation group through a household merger or a Type C non-interview, then subsequent records should reflect that this household has been eliminated from the sample for its particular panel-rotation group. If a household enters the panel-rotation group out of rotation sequence, then we should know whether it is an extra or added (due to uplisting) location. Thus, if household records are maintained throughout a rotation, there is an additional edit check (from Collection Quarter 3 on for each rotation) and a fairly straight-forward edit check can be made to insure the location identifiers are maintained throughout the sample. This could be the first clerical edit check on collection quarter data and should not entail significant extra work. To repeat what was noted for locations, each location in the master list should have household information through the full rotation. Each household, once entered at a location, should be retained until replaced by another household, a vacancy occurs, or the location is removed by sample rotation.

Fourth, after the first household interview, a reasons should be recorded for the absence for each person who was a member of an entering location. Once a line number has been assigned under a household interview, there should be some information for that person recorded for each subsequent valid household interview. No person should ever disappear from the file so long as his/her household is retained. Retaining the person line should not add significantly to the number of records, but it does require recording the number of persons 12 years old and over in the household. That is, one cannot simply count the number of person records to get the number of persons 12 years old and over. One will have to count the number present to be interviewed. This means that interview type (SC034) will need an additional category of "not present in household." Moreover greater care should be exercised in comparing the persons line number from one interview to the next.

Fifth, a question should be added ascertaining the reasons why a household member is absent for any subsequent interviews during the rotation period, e.g., died, marital separation or divorce, moved out to establish new household, away at school, etc. It would be helpful if one could avoid vague categories such as "temporarily absent" since they are of doubtful analytical value.

Ordering Incidents by Time of Occurrence. The current procedure asks: "In what month (did this/did the first) incident happen?" The respondent is encouraged to give the exact month. For series incidents, the respondent is asked to indicate only "in what month(s) did these incidents take place?" and then seasons are checked off.

For some purposes of studying multiple victimization, we are interested in ordering incidents in time. The procedure could be changed to permit ordering by time and to relate series to non-series incidents. For series incidents only the month of first incident is obtained. The month of occurrence of the most recent incident (beginning SC108) is

not given. It would be a simple matter to obtain the month of the most recent incident of the series and record that since that is the incident of which information is obtained.

Instead of recording the seasons during which series incidents occur, beginning with the month of occurrence of the most recent incident, check all other months in the reference period for which incidents are said to have taken place. Instead of asking the question of "How many incidents were involved in this series?" one might then ask how many incidents of this kind took place in that month, beginning with the most recent month. One would assume that the respondent might have a better idea of how many such incidents took place in the most recent month than for earlier months and that the procedure of having the respondent estimate for each month might be more reliable than a global guesstimate (as is now the case).

For non-series incidents, the respondent may have more than one victimization in a given month. It would be helpful if these occurrences were simply rank-ordered within the month. With order of occurrence within the month, some inference about clustering to types of crime, etc., is possible.

Further information on series incidents would be helpful in establishing the similarity of the crimes reported together as a series. Therefore, the following suggestions are made:  
For Source Code 107

Present Format:

- c. How many incidents were involved in this series?
  - 1. three or four
  - 2. Five to ten
  - 3. Eleven or more
  - 4. Don't know

Suggested Format:

- c. How many incidents were involved in this series?  
RECORD ACTUAL NUMBER \_\_\_\_\_  
Don't know \_\_\_\_\_
- d. In which month did the last incident occur? (Encourage R to give exact month.)  
\_\_\_\_\_  
Month (01-12)
- e. Was the offender the same in all of the incidents?
  - 1. Yes 2. No 3. Don't know
- f. Did the offender always do the same thing?
  - 1. Yes 2. No (Ask a) 3. Don't know
  - a. If not, how different?  
\_\_\_\_\_
- g. Does this series of incidents go back further than the 6 month period we are talking about?
  - 1. Yes (Ask a) 2. No
  - a. How long ago did they begin?  
\_\_\_\_\_  
(Enter number by months exactly when this series began)

When a household moves into a sample location it is important to identify whether or not the incidents reported occurred while the person was living at the sample residence before moving. The following, therefore, could be incorporated:

For all new sample locations ask:

Have you moved in the past six months?

1. Yes
2. No

For all incidents reported by those who moved to location in last six months ask:

Did the incident that you are telling me about occur while you were living at this address (or before you moved here)?

1. Yes
2. No

This latter question should always be asked for all in-movers to a sample location when they are interviewed for the first time, i.e., when SC 020 is "Replacement household since last enumeration" or "Not in sample before."

Facts Related to the Occurrence of the Crime. Additional items on factors related to the occurrence of the crime would facilitate our understanding of patterns of crime victimization. In some cases the city survey questionnaire provides information not asked on the NCS.

Place of Occurrence of Crime. Form SCS-2, SC 110 & 111 provide information on whether the incident happens "inside the limits of a city, town, village," but the information in SC 111 on place name is not available on public use tapes. Thus we are severely limited in our analyses of whether an incident occurred inside the limits of the place where the respondent lives, it would be preferable to follow the procedure of SC 109 on Form NCS-4 of the city survey. One might add that in general there is insufficient information on place available in the public use tapes.

SC 112 relating to the question of "Where did this incident take place?" is not in sufficient detail to be useful for analytical purposes, particularly if different types of respondents and types of crimes are considered. There are several possibilities to consider, depending upon the kind of information sought.

First, it would be useful to know how far from home an incident occurred. One way of getting such information would be to use a simple question of whether it was less or more than one mile. Another would be to ask for all codes other than 1 and 5 on SC 112 whether the place was in their neighborhood or outside of it, accepting the fact

that people's sense of size of neighborhood will vary considerable. It seems preferable to have a separate question of this sort asked for all locations other than #1 or #5 rather than try to include that information in a more detailed version of SC 112. Thus if a respondent reports the incident occurred in a store, it would be followed with: "Is this store in you neighborhood?" or / and "Is this store within a mile of where you live?"

Second, an objection to the present SC 112 code is that too much is placed within the same code category. For young persons going to school and for school employees, one would like to know more about where their victimizations occur. Apart from crimes that may occur to them at home or near their home, the main categories that should be kept separate are these: (1) inside school (as at present); (2) on the school grounds (now put with others); (3) on the way to school (now included with on the street); (4) on a playground, park, etc.

Third, an early pilot version included the category "inside public conveyance or station." Was that category too small to warrant its retention? It is a useful additional distinction. It would be helpful to know where such incidents are now coded--"on the street"?

Fourth, an early version also included the category "at home of friend, relative, neighbor, vacation home, etc." This is too broad and vacation home should be excluded. Perhaps there should be a category to include home of other people such as relatives, friend friends, neighbors. Are these now included in the "other" category? It seems so since we find that 32% of the incidents reported for Screen Question 32 were said to occur in a vacation home, motel or hotel, 32% also were classified as "other". Since Screen Question 32 specifically mentions the home of a friend or relative or where one was temporarily staying, our guess is that the home of relatives or friends might be useful as a separate category.

Fifth, the "other" category also is fairly sizeable for some major types of crime elicited by other screen questions. The following are illustrative examples:

| Screen Question # | Per Cent "Other" Place |
|-------------------|------------------------|
| 32                | 31%                    |
| 39                | 11                     |
| 40                | 15                     |
| 41                | 12                     |
| 42                | 10                     |
| 45                | 12                     |

These questions often account for substantial numbers of incidents, so that additional categories would have more responses than some of those now included. We note, for example, that vacation home/hotel/motel has only 1.1% of all places of incident codes and office or factory

1.8% while 6.4% are coded "other."

Sixth, it also might be useful to see whether the "on the street" category is all that homogeneous; it includes 28.6% of all our incidents.

Informing the Police. We recognize that SC 181 is designed to handle both personal and household crimes and thus "household number" is the only code available. Yet it is important to know whether the victim or some other member of the household called the police for personal crimes. Perhaps the best solution is to ask which household member called the police. SC 181a could list the line number of the household member who called the police. Whatever format is adopted, an attempt should be made to separate victim calling the police for crimes against the person from other household members.

SC 182 asks the interviewer to mark all reasons that apply to why the incident was not reported to the police. It would help to get the respondent to say which of these reasons was most important, second most important, etc., and provide that information as well. There could be "n" fields for "n" reasons and the fields could be filled successively to the left with the most important reason code in field 1, etc. There could be fewer fields than reasons, e.g., the three most important reasons.

A supplement of the NCS should try to get information on why the police were called (SC 181) as well as why not (SC 182).

Offender Information. Whenever information on an offender is reported for more than one incident ask, "Was this the same person (were any of these persons the same) as the ones already mentioned?" In this way we can tell whether the information on offenders being reported is for different offenders or the same offenders. Naturally, there should be a follow-up to determine for which incidents the offenders were the same and for which different.

Though our guess is that it would be no simple matter to obtain the information, it would be helpful to know from one collection quarter to the next whether any of the offenders had previously victimized the person reporting them on an incident.

To obtain some information on police and offender behavior in relation to the incident, either for all incidents reported (or at least for some collection quarters) the following might be asked:

- a. "Was/were the person/persons who committed the crime present when the police came?" or "Did the person/persons who committed the crime leave before the police arrived?"
- b. "Did the police arrest or take any of the offenders with them when they left?"

These two items will permit us to assess better the information

on the relation between victim and offender and on police discretionary behavior, albeit not highly refined measures.

Activity of Victim at Time of Incident. The activity of the victim at the time of the incident is interesting and crucial in the determination of relative risk. Basic categories of activity at the incident occurred that might be considered are for personal victimizations: Working at one's occupation; working at one's residence; in transit between two places (bus, taxi, walking); relaxing or involved in leisure activities (sitting in bar, watching TV); shopping; in school; on the way to school; outside of school, on school grounds; and for household victimizations: no one at home: at work, school, etc.; away on vacation, holiday; someone at home and discovered it happening; someone at home but unaware it was occurring.

Suggested Changes in Background Items. The question on geographic/residential mobility of household should be replaced. It has been noted that Question 25 (SC 044, 045, 046, & 047) is the standard Census question on geographic mobility. The question is of little utility for analytical purposes in the crime survey and moreover, fails to provide adequate information on length of time at present address. A revision or replacement of this section is recommended since together with the recommendation on locating incidents at present or previous address, it should give us the needed information on residential/geographic mobility. Among question to be considered are number of years lived at the current address and frequency of moving within a given period of time, e.g., five years.

Suggested Changes in Procedures for Coding Information. Given the code changes in January, 1973, some of the suggestions given below are unnecessary. They are included, however, to illustrate a principle: The same code category should retain the same meaning over time if there are additional codes for new categories. When new categories are introduced, they should be given unique (not previously used) codes, provided additional codes are available.

First, the line number of the proxy respondent should be coded in the person record of every proxy interview.

Second, code changes should be minimized and standardized from one another, e.g.:

- S. C. 051 looking for work
- S. C. 049 work at all last week
- S. C. 014 type B vacancy
- S. C. 034 type of interview
- S. C. 050 laid off

Third, those data items that have the potential of allocated values should be indicated, (e.g., month of incident in some cases).



Fourth, the exact date of interview should be recorded since it is of interest in studying recall bias due to backward telescoping from month of interview.

Fifth, data tapes might include information on the interviewer (e.g., sex) as it may affect reporting of incidents.

Sixth, the date of birth, rather than age last birthday, should provide more accurate age data.

Seventh, the industry and occupation codes should all be numeric (i.e., no zero/zero/letter codes).

Finally, we suggest changing SC 020 to: household status; same household as last enumeration; replacement household since last enumeration; previous non-interview; not in sample before.

Technical Report # 2. "Detecting Temporal Trends in Victimization: Intervening Factors in Assessing Rates of Victimization" (Arnold Cowmeadow and Albert J. Reiss, Jr., December, 1976).

Bounding and Type of Interview as Sources of Error. A major use of the panel data gathered in the NCS is to examine changes over time in the variables measured for the on-going sample of households and persons or of other units of observation. Any survey, however, is subject to errors in observation or measurement that can affect estimates over time. In the NCS, one of the major variables under study is rates of criminal incidents or victim experiences that are later classified as victimizations, not the victimizations themselves. Consequently, anything that selectively influences reporting of incidents to a survey interviewer introduces error in estimates of victimization and complicates any analyses of victimization. This report examines two of the important variables that seem to affect incident reporting rates: "bounding" and whether the interview was by telephone or in person (face-to-face).

Bounding. The reporting of events to a survey interviewer is affected by response error known as telescoping. Time telescoping occurs when a respondent reports an event as occurring before the actual time of the event is backward telescoping while an event reported as occurring after the actual time of the event is forward telescoping. The amount of telescoping will depend upon the precision with which an event is placed in time; measures of error due to telescoping depend not only upon precision but the time reference period to which an event will be assigned for purpose of estimation or analysis. If one wishes to sequentially order crime victimizations in time greater precision is required than if one wishes simply to estimate the number of such incidents in a given period of time. Thus both telescoping external to a reference period for which victimizations are obtained and internal to that reference period are critical if one sequentially order incidents, but internal telescoping will introduce fewer errors in assignment to

an estimation period.

To control forward external telescoping, the NCS introduced an interviewing procedure called bounding. The initial interviews at addresses in incoming sample rotation groups were designed to bound victimization reporting for subsequent interview at six month intervals. Any incident reported in the next interview to the bounding interview (or any subsequent interview) that was also reported in the bounding interview is regarded as having been telescoped forward in time and, therefore, excluded from the subsequent interview period. Bounding is thus a procedure to prevent allocation the same incidents in consecutive reference periods by eliminating respondent reports of incidents reported in the previous interview.

The magnitude of forward telescoping is an important technical issue in panel surveys since it is error in survey estimates for a given time period and since the error may vary across time periods, it introduces error in estimates of changes over time. To control the effect of forward telescoping in estimates of victimization, the Bureau of the Census does not use initial interviews at a sample address in an incoming rotation group to produce estimates of victimization. This bounding procedure, however, is only a partial control over forward telescoping since not all households or persons in a sample rotation group are in fact bounded for subsequent interviews. This lack of bounding arises primarily from the fact that households or persons at a sample address move. But it may occur for other reasons as well, such as adding locations to update sampling lists or because of non-interview for an interview period. Since the incidence of residential mobility is quite high, a substantial proportion of households regarded as "bounded" in the census procedure are actually "unbounded". We do not assume in this report that all households or persons in a sample rotation group whose interviews should be bounded are in fact bounded. Rather, an interview is defined as bounded only when it is immediately preceded by an interview. All other interviews are treated as unbounded. We are thus able to examine differences between interviews that are actually bounded and unbounded assuming interviewer reliability in assigning incidents to a reference period.

Type of Interview Procedure. A much debated point in the literature on survey research is whether survey interviewing by telephone is as productive of reliable and valid information as is that obtained by personal interview. From the outset that was a problem for the design of the NCS. While the design provided for contact with a household at a sample address to be made by personal interview, it also provided that interviews with household members who were not present or in some cases all follow-up interviews could be made by telephone. Telephone interviews, therefore, are not conducted for a random subsample of all households or respondents but rather depend upon the logistics of locating and interviewing respondents and to a substantial degree interviewer discretion.

There are two kinds of selective influences in interview procedure that can have important effects on any estimate of victimization rates. They are (1) those that change over time in some systematic way and (2) those that affect socio-demographic sub-populations of the sample differently. The present report is addressed to the first type of selective influence. The problem of interest is change in victimization rates over time, either seasonal or other cyclical trends or quasi-secular trends. The selective influences examined for their effect on reporting rates of victimization are changes in the relative proportion of each type of interview--bounded and unbounded, telephone and personal--over time.

Measuring Effects of Bounding and Interview Procedures. The interview procedure for the NCS is that each location in the sample is visited once every 6 months. All persons 12 years of age and older in the household at that location are interviewed. Details are elicited on any criminal victimization or attempted criminal victimization against either the person or household property in the previous 6 months (not including the month of the interview). Thus a July interview would ask the individual to recall incidents which happened between January and June, inclusive. A major reason that any unbounded interview may be expected to produce more incident reports than a bounded one is "forward external telescoping" of events in time. An incident which occurred prior to the designated 6-month recall period is reported as happening within it.

The initial procedure followed in measuring changes in victimization rates over time was to determine the number of interview recalls for each month and the number of crimes against persons (rape, robbery, aggravated assault and personal larceny with and without contact) reported for each interview. This number of person incidents reported for each month was then divided by the appropriate number of interview recalls to obtain a standardized rate.

Tabulations of data files on all persons who ever report incidents initially disclosed that unbounded interviews yield at least 50 percent more incident reports than bounded ones. We likewise knew that unbounded interviews are more frequent at the beginning of our panel period (July, 1972) than at its close (December, 1975), so bounding was controlled in examining changes in victimization rates. Similarly, our early tabulations indicated there were sufficient differences in the productivity of telephone and in-person interviews to warrant its introduction as a control variable.

The sample population that formed the base for the analysis consisted of all interviews where bounding status could be determined. Ambiguities arose when it was not known whether a person was present at the previous interview. These ambiguities arose especially for the first Census "bounded" interview for samples 3 and 4, samples for which the bounding interview had not been included in the files received from the Bureau of the Census. Lacking the first interview, i.e.,

the Census "bounding" interview, it could not be determined whether or not household composition was the same for the first and second interviews. The Census files contain information on whether the household is the same as the previous interview but does not provide information on whether all household members were present at the previous interview. Out of a total of 919,002 interviews, 75,003, or 8.16 percent, were rejected because of ambiguity in bounding status. The remaining interviews yielded 105,506 incident reports. Since there were 112,623 crime incident reports in the total file, the 7,117 excluded because bounding status could not be determined for the interview represents 6.32 percent of all incident reports. The 843,998 interviews used in the tabulations include recall for six months for each interview. There are a total of 5,063,988 recall events (843,998 x 6).

Both personal and household series and non-series incidents are included in calculating the victimization rate. Only the most recent series incident is included in the victimization count, however, the incident for which the respondent reported detailed information. The incident file included the series and non-series incidents reported as occurring in each month of the six month recall periods, January, 1972 to November, 1975. To calculate standardized victimization rates, the following computations were made: (1) the number of interviews recalling for each of the incident reporting months was tabulated; (2) the number of series and non-series incidents reported in a given month was tabulated for each of the four types of bounding-interview status (bounded personal; bounded telephone; unbounded personal; unbounded telephone) and divided by the number of interviews for each of the four types of bounding-interview status recalling for that month; (3) each of these values was then multiplied by 1,000 to produce an incident reporting rate per 1,000 recall months.

Temporal Changes in Frequency of Interview Types. Interview status is confounded with bounding status. Quite clearly, an unbounded interview is more likely to be personal than is a bounded interview whereas a bounded interview is relatively more likely to be conducted by telephone. This is not unexpected, given the fact that the sample design called for establishing personal contact with a household at a sample address for the first interview in a sample rotation group. Generally, a first interview is required to secure a phone contact. Such first interviews comprise a substantial proportion of all unbounded interviews. Subsequent interview are open to greater interviewer discretion in changing to conduct an interview face-to-face or by use of telephone: they are commonly bounded.

We observed selective Temporal patterns that may affect estimates of victimization rates. There is a pattern of cyclic fluctuation with a periodicity of about 12 months.

Examining a graph for bounded interviews for the period July, 1973 to November, 1975 (the last date of recall), it is clear that bounded interviews peak in frequency during the month of February and reach a

minimum around the month of August. The bounded data before 1973 are excluded from this analysis because before 1973 there are very large number of unbounded interviews with persons entering in a sample rotation group. The distribution of personal and telephone interviews also shows a crude annual periodicity, though it is slightly out of phase with the bounded-unbounded cycle. For personal interviews, the minimum falls around October and November with a peak at about March.

The fact that both bounding and interview status are associated with interview productivity and both show cyclic variation in a complex manner makes it quite likely that victimization rates also have a complex cycle if one does not control for type of interview. Each of the four interview types (bounded and unbounded personal and bounded and unbounded telephone) of interview has a distinct incident reporting rate and differ considerably in their relative frequency with bounded personal being the most frequent (more than 60 percent of all interviews after July, 1973) and unbounded telephone being uncommon.

There is a marked start-up effect for the panel survey evident in the recall data for January 1972 to June, 1973 but substantial variation in the distribution of the four interview types after June, 1973. After June, 1973, the distribution over the four bounding-interview status types is characterized by out-of-phase roughly annual cycles.

The Effect of Interview Type on Reporting Rates. Some of the major conclusions that can be drawn about the effect of bounding and interview status on the reporting of victimization incidents are given below.

First, personal interviews produce about 50 percent more incident reports than do telephone interviews. Overall personal interviews netted 21.4 incidents per 1,000 interview recalls while phone interviews netted only 14.3 incidents per 1,000 interview recalls. This relationship holds approximately for the bounding status groups. For bounded interviews, personal interviews netted 16.8 incidents per 1,000 interview recalls while phone interviews netted only 11.4 incidents per 1,000 interview recalls. Within the unbounded interviews, personal interviews yielded 30.5 incident reports for every 1,000 interview recalls while phone interviews yielded only 22.3 incident reports per 1,000 interview recalls. Only if there is convincing evidence that persons interviewed by phone actually are a non-random subset of persons and households with lower rates of victimization, there must be a strong presumption that telephone interviews are less productive of victimization incidents.

Second, unbounded interviews are about 87 percent more productive of victimization incidents than are bounded interviews. Unbounded interviews yielded in the aggregate 29.0 incidents per 1,000 interview recalls whereas bounded interviews produced in the aggregate only 15.5 incidents per 1,000 interview recalls. For personal interviews, unbounded interviews were 82 percent more productive of incidents

than were bounded interviews (30.5 incidents per 1,000 interview recalls for unbounded compared with 16.8 for bounded). Among telephone interviews, unbounded interviews were 96 percent more productive of victimization incidents than were bounded interviews (22.3 incidents per 1,000 interview recalls for unbounded compared with 11.4 for bounded). The increased productivity of unbounded interviews is somewhat less than for personal than telephone interviews. Whether this is characteristic of a random sample of unbounded telephone interviews compared with personal interviews cannot be determined from the NCS since the telephone and personal interviews are non-random sets. Were it to be characteristic, however, it might explain some of the higher rates of incident reporting for unbounded telephone victimization surveys.

The four types of bounding and interview status differ not only in overall rates, but also in the nature of their temporal trends. For aggregate rates of incident reporting, each of the four bounding-interview status types has a different rate of productivity. Unbounded personal interviews produce the most incident reports and bounded telephone interviews the fewest. For each of the four bounding-interview types, there is an upward slope, indicating an increase in personal victimization rates over time. This trend is most evident for unbounded personal interviews.

Third, short-term fluctuations mark seasonal variation in victimization rates. The minimum victimization rates in January and February are present but not with equal intensity in each of the four bounding-interview status types. It is most marked for the unbounded personal interviews, present in the bounded personal interviews, but present only for the January, 1975 bounded telephone interviews. It is absent for the unbounded telephone interviews. The August and October peaks with a slight dip in September are present for personal interviews, particularly those that are unbounded. But, it is absent for bounded telephone interviews and obscured by month-to-month variation for the unbounded telephone interviews where the sample size is the smallest.

Both bounding and interview status have a substantial relationship to incident productivity rates. Unbounded interviews are far more productive of incidents than bounded interviews and personal more than telephone interviews. The difference in personal and telephone interview productivity rates is substantial for both bounded and unbounded interviews. The bounding procedure followed by the Bureau of the Census in estimating victimization rates clearly has an effect on controlling forward external telescoping as Murphy and Cowan report. However some of the difference between unbounded and bounded productivity rates could be due to re-interview or panel fatigue, an effect that is difficult of separation from the telescoping effect.

Data Report # 1. "Patterns of Offending in Crime Incidents In Size of Offending Groups and Age of Offenders Involved in Major Crime Incidents Reported by Victims in the NCS (Albert J. Reiss, Jr., November, 1976).

Relationship of Offending to Averting Crime Incidents. The relationship between individual rates of offending and whether crimes are averted by the incapacitation of an offender is not a simple one. It seems reasonable to assume that so long as offenders who are incarcerated would continue to commit crimes at some rate were they free, there is an incapacitation effect on the crime rate. The number of crimes occurring for any period of time will be reduced by an amount equal to that of the crimes offenders would have committed were they not incapacitated. The potential number of crimes averted by an offender's incapacitation is  $\lambda S$ , where  $\lambda$  is his pre-incarceration rate of offending and  $S$  the period of in-carceration.

The pre-incarceration offending rate is only an approximation for what may have been an offender's true rate of offending during his period of incarceration, since rates both increase and decrease over an offender's career in crime. This report focuses only on some factors that may reduce the effect of  $\lambda S$ . Any pattern where more than one offender commits the same crime, and especially group or gang patterns of offending, will reduce the incapacitation effect if only one or a few offenders in multiple offending are incarcerated and the group collectively continues to offend. While we do not have information on the persistence of offending when only some of the offenders in multiple offending are incarcerated, the larger the incidence of multiple offending, presumably the greater the magnitude of that effect on the incapacitation effect.

The policies with respect to the incarceration of offenders at any age similarly can have a significant impact on the incapacitation effect. Where rates of offending and of multiple offending are high at younger ages, and their incarceration rates are very low, for example, the effect of incapacitation on the crime rate may be appreciably reduced. The age distribution in offending for single and multiple offenders, therefore, is examined to shed some light on the potential magnitude of such effects.

What we know about patterns of offending suggests there is considerable variation in the size and membership composition of offending groups, particularly for young age offenders. For some types of crime, a single offender committing a single type of crime is the modal occurrence while for others, the modal occurrence involves multiple offenders and/or multiple crimes. Unfortunately precise information on the mix of single and multiple offending in individual rates of offending is lacking because of limited information on their mix in criminal careers. The repeating "lone offender" seems less common, however, than the repeating multiple offender or a mix of individual and multiple offending. Where patterns of multiple offending are involved in individual careers,

the less the companionship or overlap among offenders in succeeding crime incidents, the less the incapacitation effect and perhaps also, the less the deterrent effect of incarcerating any one of those offenders. What may be critical in obtaining a substantial incapacitation effect then is the incarceration of an optimal number of offenders in a network of offenders rather than the incapacitation of single offenders.

The data for this study are the 112,591 crime incidents reported by persons who were interviewed between July 1, 1972 and December 31, 1975. Both household crimes and crimes against the person are included. For series incidents, only the most recent incident is included in the total base of incidents. Whenever the respondent or some other member of the household was present when the crime incident occurred, the respondent was asked for information about the characteristics of the offender. Such information was ordinarily obtained for crimes against persons, though there is variation by type of criminal incident; it is infrequently obtained for crimes against property.

For this report, the information on size of offending group and age of offenders is examined by type of crime. Although the exact number of persons was obtained for each incident, we have chosen to categorize them into class intervals for groups of six or more persons. Where only one offender was involved in the incident, the respondent was asked to estimate the age of that offender and the response was categorized by the interviewer as under 12, 12-14, 15-17, 18-20, and 21 or over. Where more than one offender was involved in the incident, respondents were required to estimate only the age of the youngest and the oldest offender and responses were coded in the same class intervals. This procedure does not permit us to estimate the age of all offenders. For some analyses, therefore, we use a base of incidents rather than offenders.

The major crimes against persons and property are classified into 36 separate types of crime incidents that take into account such factors as whether the crime actually occurred or was only attempted, the occurrence of more than one type of crime in a single crime incident, the means used to commit the crimes, and the nature and value of property stolen. The NCS does not collect information on whether the same offender(s) is involved in incidents or repeated victimization of a person or household within or across reporting periods. We thus do not have a population of offenders where each offender is counted only once but of offender incidents. The magnitude of the error this introduces in estimates of age of offenders or size of offending group is not known. It undoubtedly is less for some types of offenses than others, e.g., it is probably higher for some types of assault where incidents of repeated victimization are separately reported than it is for incidents of rape and attempted rape where multiple victimization is infrequent in the survey.

The validity of offender information in the National Crime Survey is



not known. The estimates of age of offenders may be subject to considerable error but there is no way of estimating its magnitude. Comparisons of records of crime victims known to the police with survey interviews shows reasonably high levels of agreement in reporting crime incidents, though the crimes of rape and assault are under estimated on the survey, particularly where the offender is known to the victim. Thus in the aggregate we have fewer of the incidents where precise information on offenders could be provided and proportionally more than expected, therefore, where the information is estimated or judged.

NCS crime incident reports provide limited information on offenders for all types of crime incidents. The offender was either not known to or reported by the victim in four of every four crime incidents. The limitation is largely due to the fact that the victim is not present in crimes against property only--the so-called "cold crimes"--rather than to any artifact of the survey procedure. Information is available on offenders for most reported crime incidents involving a crime against the person; 94.5 percent of the 18,793 incidents involving some crime against the person had some information on offenders. Considerably less information is available on offenders for crimes of stealth against property only where commonly the crime is discovered following its occurrence. Only 4.8 percent of crimes of burglary and larceny reported on the NCS include information on the offender.

The NCS does not provide information on the age of all offenders when more than three are involved in the same offense. Information is available on the age of all lone offenders and for the youngest and oldest offender in multiple offender incidents.

Multiple Offending in Crime Incidents. From the perspective of victims, a victimization is most likely to involve a single or lone offender since the modal offending group size is a single offender. Almost two-thirds of all crime incidents involve a single offender with little difference for crimes against persons and against property.

When one shifts from a population of crime incidents or victimizations to a population of offenders, however, it is clear that an offender is more likely to be involved in crime incidents with others than alone. There is considerable variation nevertheless by type of crime incident. Only 30 percent of all offenders involved in crimes against persons were lone offenders and it was 32 percent for all offenses. Fewer than one-half of all offenders commit a crime incident alone or with only one other person. The average number of offenders per NCS incident is two offenders per incident. These estimates call into serious question all crime equations estimating an incapacitation effect on the assumption of a single crime averted for each arrest. On the average, one could only assign a half of one crime averted to each person arrested. Of special interest is the fact that more than 1 in every 4 offenders in crimes against the person is involved in a crime incident with six or more persons.

From the standpoint of clearing crime incidents by the arrest of an offender, more incidents involve finding the single offender who committed that incident than in finding one of a group of offenders to clear the crime. But the arrest of a single offender to clear any crime leaves on the average at least one other offender free to commit crime.

There is, however, considerable variation by type of crime. Of all offenders involved in robbery, only one in five (19.6) was a lone robber. Armed robberies have the lowest proportion of lone offenders--15.6 percent--and attempted armed robberies the highest proportion of lone robbers--27.4 percent.

Among 36 types of crime against persons and property, serious assault with theft--a predatory crime--has the smallest proportion of lone offenders. Only 1 in 10 offenders who commit a serious assault with theft was alone when committing the offense. Moreover, these crimes have the highest average number of offenders per incident, 2.9 offenders. The proportion of lone offenders also is low for the predatory crimes of serious assault with theft and no weapon (15.7%) and minor assault with theft (18.5%) compared with all other assaults. These predatory assaults differ from the more common assaults that arise among persons known to one another, reflected in the fact that almost 38 percent of all serious assaults without a weapon and without theft and 36 percent of minor assaults without theft were committed by a single offender.

What is particularly striking is that the crimes of robbery and assault account for a disproportionate number of all offenders in crimes against the person where the offending group is large. While 1 in 4 offenders involved in a crime against the person are reported from groups of six or more persons, almost all of these offenders are involved in robbery or assault. Offenders from groups of six or more persons are uncommon in crimes of forcible rape and attempt to rape or in crimes of larceny with personal contact.

Among the major crimes against the person, forcible rape and attempts to rape have the largest proportion of lone offenders; rape without theft has the smallest number of offenders per incident. Larceny with personal contact--purse snatching and pocket-picking is most likely among the crimes against the person to be a pair offense. Roughly one-third of all offenders involved in these offenses were involved with one other person in the offense.

Age of Offenders. Lone offenders are on the average older than are offenders involved in multiple offender incidents. One in four single offenders is under the age of 18 for all reported crime incidents. But the youngest offender in 53 percent of multiple offenders is under age 18 and the oldest offender in 36 percent of multiple offenders is under age 18. Looked at another way, 6 of every 10 lone offenders is 21 years of age or over as compared with 26 percent of all youngest offenders and 42 percent of all oldest offenders.

There is a considerable amount of variation in the age of offenders by type of crime against the person, however. Offenders in forcible rapes and attempts to rape have the highest average age among all crimes against the person. Bearing in mind that 79 percent of all rapes are by lone offenders, it is particularly striking that 8 in 10 offenders in these rapes are 21 years of age or older when they commit the offense. Only 8 percent are under the age of 18. Where more than one offender is involved in rape, however, younger offenders are more likely to be involved, though this is largely due to their presence in attempted rather than actual rapes. Indeed, it is quite uncommon in actual rapes for a young offender (under age 18) to be involved in an actual rape without an older offender. All of the actual rapes with theft and 94 percent of the actual rapes without theft involved an oldest offender whose age was estimated to be 21 years of age or older. Only about one-half of all oldest offenders in attempts to rape were 21 years of age and older and 35 percent of the youngest offenders were 21 years of age and older.

There is ample evidence that the crime of purse snatching is more of a juvenile offense among crimes against the person and is substantially different from pocket picking. Among lone offenders, only one third of the purse snatchers are 21 years of age and older. Yet despite the fact that purse snatching is more of a juvenile crime than other crimes against the person, more than one-half of all lone purse snatchers are estimated to be 18 years of age and older. One in 6, however, estimated to be under the age of 14. Where more than one offender is involved in purse snatching--about one-third of all purse snatchings--at least one half of all youngest offenders are under the age of 18 and more than 4 in 10 of the oldest offenders are under the age of 18.

Among the serious crimes against the person, there are substantial differences in the age of persons by type of robbery depending largely upon whether or not a weapon is used in committing the robbery. For 38 percent of the lone offenders involved in robbery without a weapon, the offender was under age 18 but for only 16 percent of the robberies with a weapon were offenders under age 18. A similar relationship holds for robberies with more than one offender, though these robberies involve substantially larger proportions of young offenders. There is a substantial age difference between robbery with and without a weapon. Somewhat more than half of all robberies and attempts to rob involve only offenders under the age of 18. By contrast, only 15 percent of all robberies with a weapon and 24 percent of all attempts to rob with a weapon involve only offenders under age 18.

The age of persons involved in assaults varies somewhat with the type of assault. Minor assaults differ from all serious assaults and attempts to assault in that persons involved in minor assaults are on the average younger. About one-third of all minor assaults involve only persons under the age of 18 (lone and oldest offender under age 18). Close to 4 in 10 minor assaults involve at least one offender under the age of 18. By contrast only one in six serious assaults with

theft involve only offenders under age 18.

Reviewing these data on age of offenders involved in crime incidents, it should be apparent that the age at which incapacitation takes place may clearly have an effect on crime incidents averted. A substantial proportion of all crimes against the person, particularly the predatory crimes of serious assault with theft and of robbery, are committed by persons under the age of 18. This age distribution raises questions about the optimal age of intervention to achieve substantial incapacitation effects. In any case, one must be struck by the fact that there are even substantial number of offenders involved in serious crimes against the person where the victim estimates the age of the offenders to be 17 years of age and under. In general, however, the most serious crimes of rape and robbery with a weapon are relatively infrequent for persons under the age of 14, though the proportions are somewhat greater for serious assault with a weapon. Overall, a substantial minority of major crimes against the person involve persons of juvenile court age.

Both the size of offending groups and the age of offenders in offenses against persons have important implications for policies of incapacitation. The size of offending groups in crimes against the person shows that on the average only one-half of an offense could be averted by the incapacitation of an offender. Given the high frequency of multiple offending for some of the most serious predatory crimes, the incapacitation of only a single offender involved in such offenses may have only a relatively small impact on averting crimes by incapacitation. Much will depend, of course, on the rates of individual offending and the mix of lone and multiple offender offenses in those rates, a matter for further research.

Since young offenders are more likely to be in group rather than individual offenses, there may be less deterrent effect from their incapacitation than for older offenders who are more likely to be lone offenders. The pay-off from crimes averted by incapacitation could, therefore, be considerably less for younger than older offenders. The pay-off from incapacitation of younger offenders has to lie more in deterrent effects arising from shortening criminal careers--a long rather than short-run effect on crimes averted by an incapacitation policy

Data Report # 2. "Relationship Between Crime Offense Seriousness and Informing the Police of a Victimization" (William Elliott, March, 1977).

The main goal of this report was to determine whether the seriousness of victimization by crime affects the behavior of victims or other persons to mobilize the police to deal with the crime victimization. The Sellin-Wolfgang measure of seriousness of crime was employed as one measure of seriousness and a seriousness score calculated for each victimization. In addition, the effect of the component elements in seriousness such as injury and loss of property on reporting crimes to

the police was also investigated.

The major findings of the report are summarized below. In general they provide support for the expectation that the more serious the crime victimization, the more likely it is to be reported to the police.

There are major differences in the reporting of series and non-series victimizations to the police. The probability of a household member or someone else informing the police of a series victimization is negatively associated with the probability of being victimized. Series victimizations and they are also less likely to be brought to the attention of the police by some other household member or by "someone else". Moreover, when series victimizations come to the attention of the police, they are almost four times more likely as non-series victimizations to come to the attention of policemen "on scene", i.e., by their presence rather than by being mobilized to the scene. The bulk of this difference between series and nonseries mobilizations, however, is due to series victimizations in only some offense categories: serious assault with weapon without theft; minor assault without theft; attempted assault with weapon without theft; and attempted assault no weapon without theft. These findings suggest that series victimizations differ from non-series victimizations. Series assaults without theft for example, are disproportionately family disputes relative to non-series assault without theft. Because of these potential differences the analysis of the report excludes series victimizations from consideration. The main findings for mobilizing police for nonseries victimizations are the following.

First, the more costly the larceny to the victim the more likely it is that the police will be informed by a household member, someone else, and by the police being on the scene.

Second, the existence of an association between seriousness and informing the police is reinforced by noting that except for auto theft, the crimes most often reported to the police are regarded as serious crimes.

|  | % Not Reporting<br>To Police |
|--|------------------------------|
| Auto Theft, Theft of Car                   | 10.06                        |
| Auto Theft, Theft of Other Vehicle         | 17.17                        |
| Rape with Theft                            | 17.86                        |
| Burglary, Forceful Entry & Something Taken | 24.06                        |
| Serous Assault, No Weapon & With Theft     | 29.11                        |
| Larceny, \$250 or More                     | 29.86                        |
| Serious Assault, With Weapon & With Theft  | 30.53                        |
| Robbery With Weapon                        | 30.57                        |

The fact that auto thefts head the list of crimes most often reported to the police does not necessarily argue against seriousness being

associated with informing the police. Under the Type of Crime classification scheme employed in this study, the amount of property loss associated with thefts and/or burglaries is negligible. Yet, on the average auto theft involves considerable more property loss for the victim than does the average larceny. It is possible that auto thefts may represent the more serious form of larceny victimization. A similar argument can be mustered to understand why serious assault with theft but no weapon is more frequently reported than serious assault with theft and with a weapon. Under the crime classification used in the study, an assault is always defined as serious if a weapon is present and there is any form of injury, whereas if no weapon is present the injury must be considerable to warrant the classification of serious. Consequently, it is plausible that considerably more serious bodily injury is suffered under serious assaults without a weapon than under serious assaults with a weapon which could result in higher reporting.

Third, if we look at the crimes which are least likely to be reported to the police, we obtain a set of crimes that clearly are not very serious. This also lends support to the seriousness hypothesis.

|  | % Not Informing<br>The Police |
|--|-------------------------------|
| Larceny, Under \$10                          | 91.07                         |
| Larceny, \$10-24                             | 85.10                         |
| Attempted Purse Snatch, No Force             | 81.25                         |
| Attempted Larceny                            | 79.49                         |
| Larceny, Amount Not Stated                   | 78.48                         |
| Auto Theft, Attempted Theft of Other Vehicle | 72.88                         |
| Larceny, \$25-49                             | 71.28                         |
| Pocket Picking                               | 71.24                         |

Fourth, if we look at the crime victimizations which are most likely to be reported to the police by a household member, we again obtain a set of serious crimes.

|  | % Reported by<br>Household Member |
|--|-----------------------------------|
| Auto Theft, Theft of Car                   | 81.77                             |
| Auto Theft, Theft of Other Vehicle         | 76.57                             |
| Rape with Theft                            | 75.00                             |
| Burglary, Forceful Entry & Something Taken | 67.47                             |
| Larceny, \$250 or More                     | 61.42                             |
| Robbery With Weapon                        | 57.01                             |
| Larceny, \$100-249                         | 52.67                             |
| Purse Snatch, No Force                     | 49.35                             |

The reporting of a victimization to the police by a household member is in some ways more interesting to the seriousness hypothesis than

is whether the police learned of the victimization regardless of the household status of the informant. One would expect that the likelihood of either "someone else: informing the police or the police being informed by being "on the scene" is related in part to social organization factors (such as, hospital policy requiring that police be informed under certain circumstances; police policy requiring heavy patrol of certain high crime districts; resort policy requiring police be informed of all thefts from guests; and so forth) as well as being related to an uninvolved party's assessment of the relative offense seriousness. Correlatively, when a member of the household informs the police of the victimization, one would expect the evaluation of offense seriousness to be less objective and detached.

Fourth, the Sellin and Wolfgang Seriousness Scale ranking of crimes is definitely related to whether or not a victimization is reported to the police. Goodman and Kruskal's gamma indicates a substantial association between highly ranked average seriousness scores and high informing of victimizations to the police (also low seriousness with low informing).

The report presents a number of conclusions on the relationship of component elements of the seriousness index of crime and mobilization of the police when a victimization occurs.

First, it is evident that the police are more likely to be informed when an injury requires some form of medical attention than when it does not require medical attention. There also is a slightly greater tendency to report even minor injuries to the police than to report without injury cases to the police. Moreover, if the police are informed by someone other than a household member, it is more likely that either the injury required medical attention or the attack resulted in injury. This suggests that household members are more likely to inform the police of a victimization than is someone outside it, and that they are more likely to report victimizations of less serious bodily harm than is some other source of mobilizing the police. Possibly non-household reporters have less opportunity to observe and call the police about less serious crimes or household members perceive the victimization of a member as more serious than a non-member would perceive the victimization.

Second, there is a moderate positive association between the degrees of hospitalization and informing the police. Again, it is also evident that as the victimization increases in seriousness it becomes increasingly likely that it will be reported to the police by someone other than a household member perhaps because in very serious cases, 1) the victim is physically unable to inform the police, or 2) the family is too concerned with immediate medical attention for the victimized member, and/or 3) informants other than household members are in social organization positions in which they are better able or required to inform the police. The police are more likely to be informed of a victimization (given an injury requiring medical attention)

as the degree of hospitalization increases from none to emergency room treatment to an overnight or longer stay.

Third, informing the police is associated with the presence of a weapon to commit the offense.

Data Report # 3. "Analysis of the Socio-Demographic Factors Associated with Repeat Victimization in the Nation Crime Survey Using Maximum Likelihood Estimation on Log-Linear Models" (Arnold W. Cowmeadow, March, 1977).

This report explores the social status characteristics of victim-prone persons, i.e., those persons prone to repeat victimization by crime within a relatively short period of time. Five indicators of social status or position (age, sex, education, family income, and race) were selected together with a survey condition that could effect repeat victimization or victimization proneness (length of time in sample as measured by number of interviews). Victimization-proneness was defined as the reporting of at least one personal victimization or attempted victimization in each of two successive six-month periods. The objective of the analysis was to learn which of the social status factors (independent variables) and combinations of factors were associated most strongly with repeat victimization (the dependent variable). The statistical technique used to test for association (maximum likelihood estimation on log-linear models) is analogous to analysis of variance on categorical data. The analysis was undertaken, in part, to study the feasibility of applying this technique to a large, complex data-set, using a standard statistical computer program, ECTA (Everyman's Contingency Table Analyzer).

First, several associations were found between the social status factors and repeat victimization but these do not operate independent of the interaction effects. These associations are as follows:

- (1). Repeat victimization increases with education.
- (2). Repeat victimization decreases with age.
- (3). Non-whites (blacks and others) have a slightly greater propensity for repeat victimization than do whites.
- (4). Persons from families with low family income (less than \$5,000 per year) and high family income (\$12,000 or more a year) have a greater propensity for repeat victimization than do persons from families with middle family income (\$5,000 to \$11,999 per year).
- (5). Males are more subject to repeat victimization than are females.

Second, we found that repeat victimization increases slightly with number of interviews.

Third, we found there were four statistically significant terms that adequately describe the association between the social status factors and repeat victimization. Each of these four terms involved an inter-



action of two independent variables with the dependent variable. These four terms are, in order of their importance: (1) an age by education interaction effect on repeat victimization; (2) a family-income by race interaction effect; (3) a race by education interaction effect, and (4) a sex by times-interviewed effect.

The nature of these four interaction effects may be summarized in the following way.

The most important of the four terms was the age by education interaction. This interaction shows that young (16 to 24 years old) persons of low education (8th grade or less education) are more likely to suffer repeat victimization than older individuals (25 to 49 and 50 or more years old) of comparable education. For individuals of either high school (12 years of education) or some college (high school diploma and one or more years of college) the relationship reverses. Although the tendency is more pronounced for some college, the likelihood of repeat victimization increases with age for the higher levels of education. This interaction operates along with the main effects of the likelihood of repeat victimization, increasing with education and decreasing with age.

The second most important interaction in the model was the family-income by race interaction. Given that non-whites (blacks and others) show a slightly greater propensity for repeat victimization than do whites, while high income (\$12,000 or more a year) and low income (less than \$5,000 per year) individuals demonstrate a greater propensity to repeat victimization than do middle income (\$5,000 to \$11,999 per year) persons; the interaction that 1) low-income non-whites are more prone to repeat victimization than low-income whites, 2) middle-income whites are more prone to repeat victimizations than middle-income non-whites, and 3) high-income non-whites and whites are equally susceptible to repeat victimization.

The interaction term which was third in importance for the modeling of repeat victimization was race by education. Assuming the main effects of race and education, the race by education interaction indicates 1) whites of low education (8th grade or less) are more subject to repeat victimization than non-whites (blacks and others) of low education, 2) high-school educated non-whites show a greater propensity to repeat victimization than do high school educated whites, and 3) whites and non-whites with some college are equally prone to repeat victimization. It is interesting to note that the race by family-income and race by education interactions demonstrate opposing tendencies except in the categories of high income and high education (where no difference by race is found).

The final and least important interaction term of the four significant second order interactions was the sex by number-of-interviews interaction. The main effects of males being more subject to repeat victimizations and individuals with more interviews being very slightly

more prone to repeat victimization are assumed. Then, interaction is such that 1) males are very slightly more likely to suffer repeat victimization under the condition of 3 only interviews, while 2) females are very slightly more likely to suffer repeat victimization under the condition of 4 or 5 interviews. In all cases these relationships are nearly negligible.

The nature of these four interaction effects was clarified by re-analyzing the data in "modules." The modules were chosen in such a way that most of the interaction terms can be reinterpreted as single-factor effects in one or more modules. Thus a complex interaction effect is partitioned into a number of simple effects. A detailed explanation is included in this report.

While the analysis was generally successful, there were problems because of "thinness" of the data. In particular, zero cells in the original contingency table, due in large part to the small number of repeat-victims, posed a problem. Future analyses should either use a larger sample (several hundred thousand cases) or a less-skewed dependent variable, (that is, with more repeat-victims).

Data Report # 4. "Repeat Victimization of Persons and Households in the National Crime Survey" (Albert J. Reiss, Jr., March, 1977).

This report describes patterns in repeat victimization of persons and households in the National Crime Survey. Multiple and repeated victimization is examined using the panel feature of the NCS. Repeat victimization is described both within a six month period and for two successive time periods. Both series and nonseries victimization are considered in repeat victimization. The major conclusions of the report follows.

First, both persons and households usually report nonseries rather than series victimization when first reporting any victimization. Roughly six percent of persons and of households, however, report some series victimization when first reporting any victimization. One-fourth of all persons when first reporting any series victimization and 29 percent of all households also report one or more nonseries victimizations. Only 1.5 percent of persons and of households first reporting nonseries victimization also reported a series victimization. In the aggregate, persons and household reporting both series and nonseries victimization have the highest levels of victimization.

Second, persons and households first reporting any nonseries victimization are more likely to report being victims of a single criminal act than of a multiple or repeated acts of victimization in the six month period of first reporting. Eighty-seven percent of all persons and 85 percent of all households reported only a single victimization on first reporting of any victimization. Correlatively, more than one in 10 persons and about one in six households reported multiple or repeated victimization when first reporting any victimization.

Within the six month period of first reporting, 2.5 percent of all persons and 3.2 percent of all households report three or more victimizations, a rate of victimization of at least one every two months. Households have somewhat higher levels of multiple or repeated victimization than do persons.

Third, assuming three or more nonseries or a series victimization is a high level of victimization (at least one victimization every two months), seven in 10 high victimization persons and two of every three high victimization households report series victimization. Furthermore, assuming at least one victimization every three months (two or more nonseries or a series victimization) only one in three persons and one in four households report series victimization within the six months of first reporting any victimization. Nonseries victimizations, therefore, is more common among repeat or multiple victims.

Fourth, there are reasonably high levels of repeat victimization. Of those persons and households in sample for two successive interviews (reporting victimization experience for one year), one in four persons and almost one in three households report repeat victimization within the year, a level of victimization of at least one victimization every six months. Despite this level of repeat victimization, in the aggregate, persons and households first reporting any victimization are more likely to report no victimization in the next six months than to report victimization by crime. Only 12 percent of all persons first reporting nonseries victimization and 24 percent of those reporting series victimization report any victimization the following six months. The comparable percentages for households are 16 percent and 27 percent. Series victimized persons are twice as likely as nonseries victimized persons to report victimization in the following six months than do household nonseries victims. Persons and households that first report series victimization and are victimized in the next six months are more likely to report nonseries than series victimization in the following six months. Among nonseries victimized persons and households, the more nonseries victimizations first reported, the greater the propensity to repeat victimization in the next six months. Persons who first reported six or more victimizations had a probability of being victimized in the next six months that is four times greater than that of persons who first reported only a single victimization. Forty-three percent of persons who first reported six or more nonseries victimization reported one or more victimizations in the next six months. Households that first reported six or more victimizations had a probability of being victimized in the next six months that was three and one-half times that of households first reporting only a single victimization. Households with high levels of victimization on first reporting had an even or better chance of being victimized in the next six months.

Fifth, persons and households that first report a series victimization and also report victimization in the next six months are more likely than not to be victimized by the same type of crime in their continuing victimization. This crime specific victimization for

persons is characteristic only of crimes of personal larceny and assault. Victims of assault with theft and theft with personal contact are more likely to be victimized by some other type of crime in repeated victimization. The crime specific victimization for households is characteristic of the crimes of burglary and household larceny. Victims of motor vehicle theft are more likely to be victimized by some other type of crime in repeated victimization. Crime specific repeated victimization is characteristic of the most frequently occurring crimes against the person and against household and much less likely for infrequently occurring crimes.

Data Report # 5. "The Residential Mobility of Victims and Repeat Victims in the National Crime Survey" (Albert J. Reiss, Jr., March, 1977).

This report describes the residential mobility of person and household victims and repeat victims in the National Crime Survey. The propensity of single, multiple, and repeat victims to move is investigated using the panel feature of the National Crime Survey. The residential mobility of single and multiple victims is examined both within a six month collection period and repeat victimization in the next six months. Both series and nonseries victimization are investigated for the propensity to move.

The move-out status of a household is based on a comparison of the household status and number of a household at a given housing unit for two successive interview or data collection periods. Whenever an interviewed household is replaced at the time of the next interview by another household or there is a vacancy at that housing unit, it is defined as a move-out household. Multiple and repeat victimization are measured by the number of victimizations reported. A person or household is defined as a multiple victims if on first reporting any victimization, two or more victimizations are reported. Repeat victims are those reporting one or more victimizations within the six months following first reporting of any victimization. Move-out rates of households and their members are calculated for the unweighted sample of households and persons in any given six month interview or data collection period. The major conclusions of this report follows.

First in the aggregate, victimized persons and households show a greater propensity to move than do nonvictimized persons and households. Within every interview period, the move-out rate is higher for persons and households reporting some victimization by crime than for those reporting no victimization by crime. Regardless of the number of interviews, the propensity to move is greater for victimized than nonvictimized persons and households, though the longer the time in sample, the less that propensity.

Second, somewhat more than two in ten households and somewhat less than two in 10 persons report multiple victimization within the first six months of reporting any victimization by crime. Among multiple victimizations, two victimizations occurs most frequently followed by

the report of series victimization and then three or more nonseries victimizations.

Third, the higher the multiple victimization of persons or households reporting victimizations, the greater the propensity to move within the next six months. There is a 75 percent increase in the move-out rate from persons first reporting a single victimization to persons first reporting four or more victimizations. The move-out rate of households first reporting five or more victimizations within a six month period is twice that of households first reporting only a single victimization. Regardless of prior level of victimization, high multiple household victims show a greater propensity to move than do households reporting only a single victimization; this relationship is not consistent for persons.

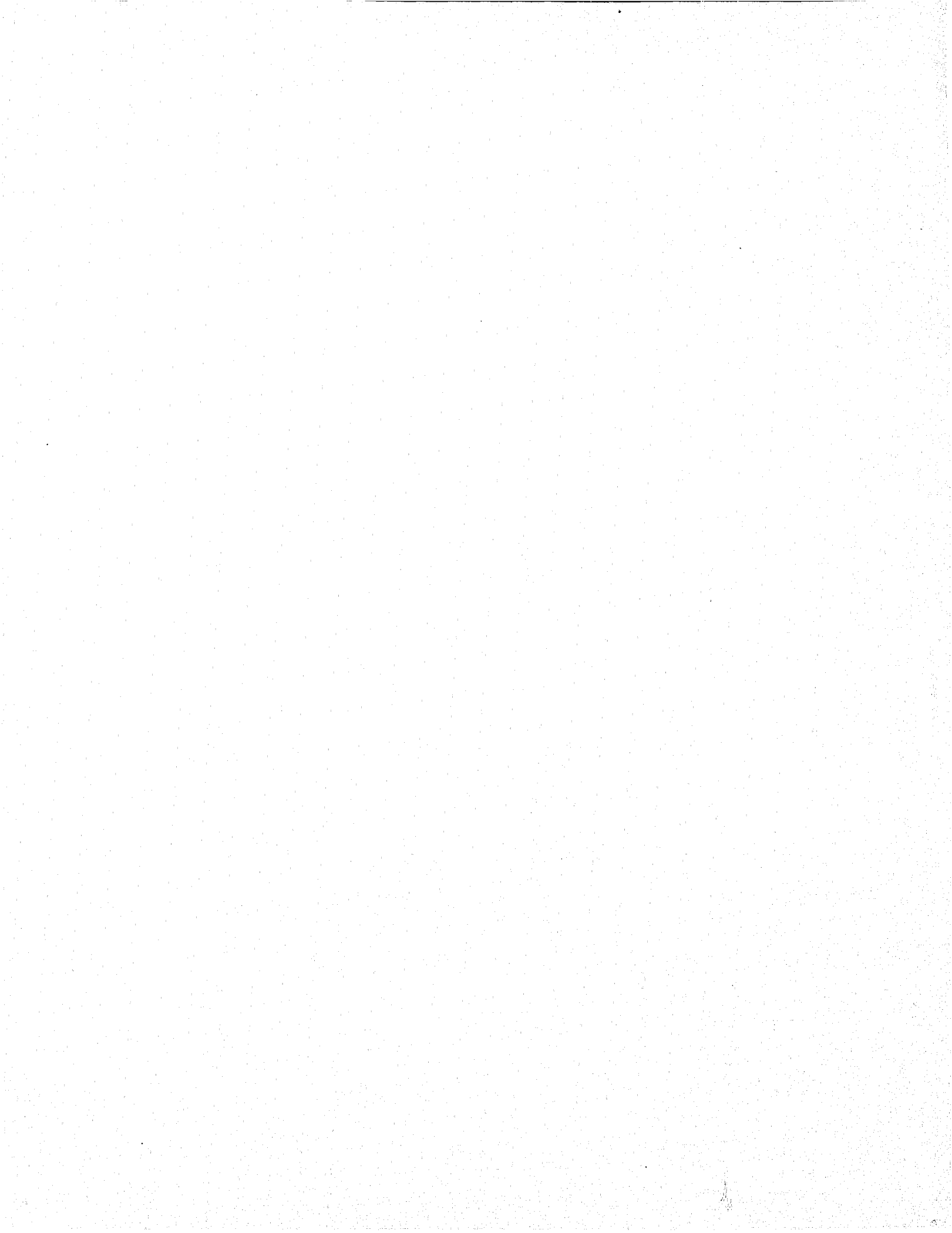
Fourth, even with high levels of multiple victimization on first reporting any victimizations, repeat victimization is much less likely in the next six months than is no victimization. Nonetheless, the higher the level of multiple victimization on first reporting and of repeat victimization in the following six months, the greater the propensity of households to move. High repeat household victims have higher move-out rates than do non-repeat victims. Continuing high victimization increases the propensity of a household to move, so that the higher the level of multiple victimization either within any six months of a year or for the year, the greater the propensity of households to move.

Fifth, the residential mobility rate of persons increases with the seriousness of the crime victimization reported. The residential mobility rate of persons who had some contact with an offender is greater than for persons who had no contact with an offender. The rate of residential mobility increases with the ranked seriousness of the crime. Assaults with theft have the highest rates of residential mobility followed in order by assaults, larceny from the person with contact, and larceny from the person without contact.

Sixth, the household crime of burglary has a somewhat higher rate of residential mobility than do the household crimes of larceny from the household and motor vehicle theft, but the differences in propensity to move by type of crime are smaller for household crimes than for crimes against the person.

Seventh, the rate of residential mobility increases with the level of multiple victimization. While residential mobility increases both with the seriousness of crimes against the person and the amount of victimization, as the level of multiple victimization increases, the rate of residential mobility increases within each major type of crime against the person. As the level of multiple household victimization increases, the rate of residential mobility increases irrespective of type of household crime. The level of multiple household victimization appears more important in the propensity of households to move than

does their type of household victimization. Thus the increased propensity of multiple and repeat household and persons to move is not specific to the four major types of crime against the person and the three major types of crime against households examined in this report.



**END**