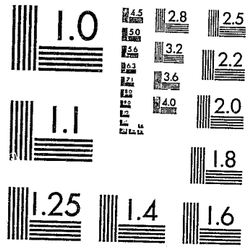

FINAL REPORT

FOR

PANEL STUDY OF VICTIMIZATION BY CRIME

SUBMITTED TO

BUREAU OF JUSTICE STATISTICS

U.S. DEPARTMENT OF JUSTICE

Grant 79-SS-AX-0012

Yale University

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September 1, 1982

Goals of the Study

There were several main goals of this study. Most have been satisfactorily achieved during this grant period. A few modifications were made during the course of completing the research.

The main goals of this research set forth in the grant application were four: (1) to update the longitudinal file by adding 1977 quarterly data to add to the number of rotation groups with six and seven interviews and to document the file for use by others; (2) to continue research into methodological features of the NCS design, including studies of panel attrition effects of selecting household respondents, bounding, and defining repeat victimization by crime; (3) to undertake additional substantive research on victimization by crime using the panel design of the file, including study of the nature and amount of injury and various aspects of victim proneness; (4) to prepare research done in the past as well as research on this grant for publication, preferably as a research monograph. The remainder of this report is devoted to a discussion of how each of these objectives has been implemented during the grant period, and the extension of time to complete work. There is an appendix that includes the major published pieces resulting from this activity and some unpublished work as well.

Before outlining how each objective has been implemented it should be pointed out that research under this grant has inevitably contributed to and been enhanced by parallel research and writing endeavors. The Principal Investigator has also been a participant in the consortium to redesign the National Crime Survey and has done work under a subcontract from BSSR that implements redesign objectives. Although at all times we have taken care to allocate research expenses appropriately to the one or other of these research activities, redesign objectives have been so closely meshed with those of this panel study that some single reports were developed and completed with both objectives in view. This is the case, for example, with the report on bias in household respondent reporting of victimizations which was one of the topics begun as part of this panel study but also became of considerable importance to correcting current estimates of reported victimization by type of crime and for redesigning both the collection instruments and of the procedures for selecting household respondents. A result of these converging objectives is a quasi-experimental study of the effect of selection of household respondents on reporting number and type of victimizations by crime with a collaborative report prepared by David Cantor and Albert Biderman of BSSR and the principal investigator.

Inevitably, also, any research program contributes to and is a valuable resource for adding to our knowledge in related areas of criminology in which the principal investigator works. Thus papers were prepared for conferences and other symposia that were prepared with partial or full support from this grant. Among ones perhaps worthy of mention since they led to publications are the Victimology Research Workshop which was convened to develop a research agenda on victimology under a grant to the MITRE Corporation (NIJ Grant 79-NI-AX0135). The principal investigator participated in this workshop and served as chairman of its sessions. Subsequently the editor's of the Journal of Criminal Law and Criminology publsished the major papers for this conference as a symposium and the principal investigator prepared a foreword to the sumposium publication, drawing upon research that was conducted under this grant. This paper is included in the Appendix.

Another that perhaps bears mention is a piece on crime statistics "What Do We Know About Crime?", prepared for a volume commemorating the Centennial of the Statistical Abstract of the United States. This volume again drew upon work done in connection with the main project, although there was additional support for it. The paper was reviewed by BJS staff prior to publication.

Papers prepared and submitted as reports under this grant were included in a volume sponsored by BJS under a grant to the Social Science Research Council. All of the work for these papers and prior versions of them were submitted under this grant. Work revising and adding to them was done in connection with the current grant and they were published in a final volume edited by the principal investigator and Professor Stephen E. Fienberg of Carneige-Mellon University. The volume is Indicators of Crime and Criminal Justice:Quantitative Studies published by BJS as NCJ-62349, June, 1980.

Documenting the Longitudinal File

Under previous grants a longitudinal file of NCS was created by merging quarterly tapes containing information on the household locations and interviews with households and persons conducted from July 1, 1972 through December 31, 1976. Initially the intention was to merge the 1977 information when it became available but these plans were superseded by ones to develop a longitudinal file as part of the NCS redesign program. An agreement was reached that BSSR would develop a longitudinal file that moved forward from January 1, 1976, a file that could be prepared by program merging of the quarterly tapes. This alternative plan made it unnecessary to merge the 1977 data so the original plan was dropped.

We did prepare, however the documentation for a machine readable tape of the longitudinal file. That documented file (and sample tapes made from it) have been used successfully in work by Stephen E. Fienberg, Willaim Eddy and D. Griffen under a subcontract to redesign the NCS. Their work has been addressed to estimating victimization prevalence in a rotation panel survey. The file has also been used successfully by Daviod Thissen and Howard Wainer, to test a Rasch model that measures and predicts victim proneness.

The file continues to be used in connection with redesign work for the NCS. After considerable delay, the BSSR longitudinal file now appears to be operational and it seems likely that as quarterly tapes from 1978 and later years become available and are merged with that file or users take that program to prepare their own files, there will be less need for the longitudinal file prepared under this grant. Possibilities might be explored, however, of the extent to which the Yale and the BSSR files can be merged. At the present time the BSSR file does not include locations but the possibility exists for merging household and person files.

Continuing Methodological Research on NCS Design

The continuing program of methodological research called for work on panel attrition, selecting household respondents, bounding, and repeat victimization by crime. Papers or publications have been prepared in each of these areas and some work continues at present in connection with the NCS redesign consortium.

Panel Attrition

Traditionally work on panel attrition has focused on loss of respondents from a panel survey due to the fact that interviewers are unable to locate persons who have moved or who for one reason or another were not interviewed for each panel wave. That line of work has focused on ways of estimating the bias that can be attributed to loss of information because of an inability to follow each of the original respondents and to correct estimates for the attrition.

The panels constituted for this study as rotation groups made up of housing units meant that households and persons who moved from a location were replaced by others at the same location who then constituted the interviewed household. In theory replacement households were like those who moved out. One problem set forth for this study was to investigate the

extent to which replacement households and persons are like move-out households and persons.

This problem proved difficult to deal with as we proceeded with the inquiry since we were interested primarily in the effect of replacement on the victimization rate. Measurement in this instance is complicated by the bounding problem and by any time-in-sample effects. Both indeed are aspects of the recall and recounting problem in measuring victimization by crime. In-moving households are not necessarily in sample for the same length of time as are out-moving household so that comparison of their rates for the same length of time in sample is often not possible. We thus have been unable to derive any overall estimate of the extent to which the traditional sampling assumptions about replacement households are reasonable ones. What is clear, though, is that at the time of replacement in the cross-section, the rate of replacement households is on the average appreciably greater because replacement households and persons are unbounded. Though the estimates vary by types of households and persons, the bias of using the unbounded replacements as ff they were bounded results leads to considerable overestimation of the crime rates for each type of crime. All in all for some types of crime they may overestimate as much as one-fifth, assuming comparison with an expected rate from an entirely bounded sample.

It is our understanding that the Bureau of the Census is now working on this problem and some attention has been given to it by the redesign consortium.

Perhaps of considerably more interest and significance, however, is the discovery of other sources of change in the original panel owing to the dynamic processes of any population in time and space and their effect on the victimization rate. There are some major changes in the size and compositon of the population or its constituent subpopulations that have an important effect on estimating victimization rates. Several of these are worth noting.

It is commonplace among sampling statisticians and panel analysts to assume that a panel once chosen can be followed as a constant population of households or persons for at least a limited period of time. Generally speaking, also, births will more or less balance deaths in a population of persons. But where one is interested in estimating rates for both persons and households in a population, the dynamic changes in population may have substantial effects on the estimation of rates quite apart from any losses due to "panel attrition".

Among the major sources of panel change that we have observed as having an effect on the victimization rate, two are especially noteworthy.

The first of these is the selective nature of entry into households. The dynamics of any population of households is such that new persons enter into them for a number of reasons. Among the major ones are (1) the effect of birth or of entry into the population selected, e.g., of becomiong 12 years of age in the NCS household sample and thus becoming eligible for a proxy interview; (2) the addition of persons to a household by marriage, not uncommong among single person households, for example; and (3) the addition of persons to a household for other reasons such as return of a member that was away when the household was first contacted, the addition of persons to a household of unrelated individuals, etc. Each of these sources, however, has a selective effect on the victimization rate. The 12 year olds entering sample households are most likely to report offenses of larceny with and without contact; since their rates are among the high rates of young persons, they add considerably to the pool of larceny victimizations by their accretion to the panel. Those who enter by marriage may have several different and even opposite effects. Since the rate of single persons is greater than that of married persons, it affects the probability of the single person being victimized by crime. At the same time, it adds an additional person who can be victimized. The net effect here appears to be to increase the size of the pool of victimizations. In the aggregate, other accretions to the sample of households

increase the expected number of victimizations also, but it is difficult to know whether this is characteristic of all sources of such addition, given the small numbers involved in many cases, e.g., the return of a family member who has left the armed forces versus one who has lost his/her job.

There likewise are losses from households owing to the dynamics of a population. There are again several major sources of such loss. There are (1) deaths of household members; (2) losses to populations not included in the sample as armed forces persons who reside on domestic or foreign bases; and (3) losses due to the formation of new households. Each of these in turn has an effect on the victimizations reported by households.

The effect of deaths of sample persons reduces the expected number of victimizations to be reported by those households and where the death is of a member of a single person household, that housing unit often is occupied by a household with a different risk of victimization. The exit of household members not only decreases the reporting of victimizations for those households but also affects their probability of being victimized--both higher and lower risk are possible. The departure of young persons from a household, for example may reduce risk of victimization for the household while his/her entry into a single person

household status may increase his/her probability of victimization. The formation of new households has several important effects, one of which is on the base for rates. The formation of households raises important issues of who is to be counted as falling within the original sample of households and persons and as contributing to the panel victimization rate and also of what effect that division has on individual probabilities of victimization.

Our work with the NCS longitudinal file has shown that each of these sources of change is considerable, although we cannot measure each precisely owing to problems of estimating the effect of such changes for moving out, etc. Rather, what we can observe are net effects of such change on the size and composition of the panel over time and net effects on the victimization rate.

The results of that inquiry show that the dynamics of a population can have considerable effect on the victimization rate, especially for subgroups of a population. Within the NCS both incidence and prevalence rates can be affected by such changes.

It is our conclusion that whenever panel surveys of households and persons are employed for estimating rates or for studying substantive problems, such dynamic changes in a

population are likely to be considerable and must enter into considerations of what units and who is to be followed and included within the population for which such rates are estimated. Even then, since such dynamic changes also are consequential for estimating risk, their effect must be taken into account.

Selecting Household Respondents

The NCS has rules for the selection of household respondents. Like many sample surveys, the NCS does not select the household respondent randomly. The interviewer is instructed to select the household respondent from among those eligible for household respondent status. Thes rules give considerable flexibility to the interviewer. Clearly the household respondent is of importance since the household respondent is responsible for answering the control card and survey instrument questions on the housing unit and the composition of the household, answering and household screener questions, and usually serves also as the proxy respondent for persons 12 and 13 years of age residing in the household or for any other household members eligible for proxy interview.

Among the matters that are of special interest in this connection are four: (1) how representative are household

respondents of all members of the household eligible for respondent status? (2) does the household respondent remain constant over the number of interviews that the household is in sample? (3) what is the effect of a household respondent serving as a proxy for a 12 or 13 year old? and,, (4) are there any biases in reporting victimizations as a consequence of household respondent selection? The first two of these questions was investigated in connection with this grant and the latter two in connection with the NCS Redesign Consortium.

Our research discloses that the household respondents selected are not representative of all eligible household respondents. Not all members of a household, of course, are eligible for selection as a household respondent. Any household member 18 years of age and over is technically eligible to serve as a household respondent when more than one member is over the age of 18. In terviewers are instructed, however, to try to interview "...the most knowledgeable household member, that is, the one who appears to know--or might reasonably be expected to know--the answers to the household questions. Most frequently this will be the head of the household or his wife. In all cases, the household respondent must be at least 18 years old, except in those households in which the head, his wife, or all persons are under 18." (U. S. Bureau of the Census, National Crime Survey

Interviewer's Manual, NCS-560 Washington, D. C.: U. S. Dept. of Commerce, Rev. 8/75, p. D2-4). Parenthetically we note that this instruction treats the male as head of household in a married couple household.

There is no simple answer to the question of how representative are household respondents of all eligible household respondents. We begin simply by noting that single persons households have only one eligible respondent and single person households, of course differ from households with two or more membrs. They differ especially in age composition with persons aged 18 to 25 and ages 65 and over being represented disproportionally in all single person households. They differ also in their sex composition with females more often represented than males. Similarly, the marital status composition is different with only a very small proportion of married persons in single person households. We could also show how they differ from all other household size groups and thus they necessarily differ from any other kind of household respondent.

Where they can choose a household respondent, the selection will depend to some extent on who was the household respondent in prior interviews and whether that respondent(s) is available at the time of the visit for current interview. For one of our experiments where we looked at the

characteristics of households who were in sample three or more times, there were eight possible groups of eligible household respondents. Designating a household respondent as HR and a nonhousehold eligible respondent as NHR, one is in either one of these statuses for each of the three interviews. The eight possible combinations then for Interviews 1,2 and 3 are respectively: (I) HR,HR,HR; (II) HR,HR,NHR; (III) HR,NHR,HR; (IV) HR,NHR,NHR; (V) NHR,HR,HR; (VI) NHR,HR,NHR; (VII) NHR, NHR,HR; (VIII) NHR,NHR,NHR.

Even taking into account the fact that persons under age 18 are household respondents infrequently and that single person households are more likely to be headed by females, it is clear that women are much more likely to be selected as household respondents than are men. For our study on household respondent status of the households intervewed at three successive time periods only 34.5 percent of the persons selected as household respondent at the time of the first interview was a male. Men also were much less likely to be selected as a household respondent then were women with 41.2 percent of the men never serving as a household respondent during one of the three successive interviews while only 21.1 percent of the women never served as a household respondent. Men who serve as a household respondent during one or more of the three interviews were less likely than women to have been

selected as household respondent at the time of their first interview. Although 62 percent of all men serving as a household respondent for one or more of these interviews were selected at the time of the first interview, 74 percent of all female household respondents were selected at time of first interview. Excluding households where the same respondent served for three or more interviews--households where 74.4 percent of all household respondents are women--the odds of selection as a household respondent are about equal for men and women at time of first interview. 48.5 percent of men ever serving in one or two interviews and 49.7 percent of the women were selected at time of first interview. Men who ever serve as household respondents, excluding those from single person households and who serve for all three interviews as a household respondent, were somewhat more likely to be selected for the first time in the third interview than are women. Thus 15 percent of these men who served as a household respondent for one or two interviews were selected for the first time in the third interview as compared with 8.3 percent of the women.

Only very small proportions of persons under age 18 ever serve as a household respondent. Yet of persons aged 14 in sample for three interviews, seven percent had been interviewed at least once by age 15. Similarly, of those who

were aged 15 at time of first interview 10.7 percent had served at least once as a household respondent by age 16. The percentage increases with age; of those who were age 16 at time of first interview 15.7 percent were interviewed by age 17 and exactly one-fourth of those who were 17 at time of first interview were interviewed then or during their eighteenth year. Looked at another way, for each age group, the person ordinarily not considered as eligible household respondent were more likely to be selected at time of third than at time of first interview by which time they were on the average more than a year older. Of the 14 year olds, for example, who served as a household respondent either at age 14 or age 15, 21.7 percent were interviewed when first in sample at age 14 and 43 percent were interviewed for the first time during their third time in sample when clearly aged 15. For the 17 year olds moving towards the 18th year age of eligibility, 33 percent were selected at time of first interview and 33 percent at time of last interview. For one reason or another, however, it seems evident that persons under age 18 from stable households that remain in sample for at least three successive interviews have a reasonable probability of being selected as the household respondent, particularly as they approach the age of 18.

That the interviewers follow the prescription of giving preference to selecting the head of the household or the spouse of the head is also clear. As one might expect, given the relatively small average size of households, a substantial proportion of all eligible respondents of households in sample for three successive interviews serve as a household respondent for at least one interview: 69.6 percent. Of these household respondents, 51.2 percent designated themselves as the head of the household and an additional 42.4 percent were designated the wife of the head; in all 93.6 percent met the criteria of the most knowledgeable respondent in instructions to interviewers.

What clearly emerges from these and other comparisons is that while NCS interviewers currently generally implement NCS procedure for selecting household respondents, among those households that remain in sample the longer it is in sample, the more likely the household respondent changes and the more often are selected than is called for by the procedures. Left open is the question of how much of this shift is due to a shift to telephone interviewing, even though it ordinarily is assumed that the household respondent will be interviewed in person. Or, correlatively, given the requirement of a personal interview for household respondents, what are the problems of keeping the household respondent constant? The

latter problem is of special importance since each time the household respondent is changed, that respondent is unbounded as a household respondent and can telescope forward incidents that were unreported by the previous household respondent.

Work on the effects of selecting household respondents was also undertaken in connection with the subcontract with BSSR and in cooperation with them. That work is reported separately. The principal investigator has done related work on the effect of proxy interviewing on victimization reporting and with David Cantor and Albert Biderman of BSSR has done work on the effect of household respondent selection and of the household screener on reporting of victimizations.

Bounding Implementation and its Consequences

Early on we used the longitudinal file to investigate the extent to which the NCS implements its bounding procedures and what are the consequences of failing to do so. That work focused primarily on how the move-out of households and their replacement affected the cross-section estimates in the NCS. What was demonstrated is that there are substantial move-out rates from one time to the next, so that from 17 to 19 percent of all interviews in any interview period that the Census treats as bounded in its cross-section estimates are actually

unbounded since replacement households for move-out households are unbounded. Move-in households, thus, contribute disprortionally to the cross-section estimate. Some attempt was made to estimate the nature of that effect. We determined that while 82 percent of all interviews are actually unbounded, only two-thirds of all reported crime incidents come from interviews that are actually bounded. The fact that roughly one-third of all crime incidents are reported in unbounded interviews is owing to a combined effect of persons and households moving into locations that have substantially higher victimization rates than do non-movers and of unbounding which produces more victim incidents than does bounding of interviews.

We explored further the extent to which we could estimate the unbounding effect. This proved to be an enormously difficult task owing both to initial faulty conceptualization and later to problems in measuring, given changes in that conceptualization of the bounding problem

There has been a confusion of bounding and telescoping effects and it was sometime before we discerned this confusion and its implications for measuring bounding and telescoping effects in the NCS. The problem begins with the way that the first interview is defined as the bounding interview on the grounds that it controls for telescoping effects. It is

commonly assumed on the basis of limited evidence that respondents when first interviewed telescope incidents into and out of the reference period for which they are expected to recall and recount incidents of victimization by crime and that, on balance, forward exceeds backward telescoping. In any case, it is assumed, that excluding the first interview from the cross-section estimates, bounds all interviews. That designation although abstractly true when one follows all households and persons and secures interviews with them in each successive time period ignores both conceptual and operational problems. Not all are of equal consequence, of course, with some contributing only a small amount of error.

Consider first the fact that the bounding procedure depends upon previous reporting of one or more incidents since the only way for interviewers to bound the current recounting of incidents is to check against a control card to determine whether the incident was reported previously. Hence in an important sense <u>only those who previously recounted an incident are unbounded</u>. Our first conceptual clarification thus was that only those who previously recounted an incident are eligable for the bounding procedure. This means, moreover, that the bounding procedure depends upon a repeat victimization rate. The point is not as obvious as might seem to be the case. Several things should be understood from this

conceptual clarification. First, until a respondent first recounts one or more victimizations to an NCS interviewer, there is no way for that interviewer to check a recounting of a victimization against a prior one so as to "bound" the interview. The simple assumption of bounding then rests on the capacity to implement it since without data on prior victimization there is no way to implement the NCS procedure. One simply assumes that if incident was previously recounted to the interviewer, the currently recounted incident belongs to the current reference period for unless there is some previously reported incident(s) with which it can be compared to determine whether or not it belongs to this or a prior reference period it could be a telescoped incident.

A second implication is that whenever one does not recount an incident in a reference period interview, the presumption of bounding is open to question. Thus if one does not recount an incident in any given period and recounts one the following period, there can only be a presumption that it belongs to the current period; if one erred in not reporting it the previous interview and now reports it, telescoping has occurred. Thus telescoping can occur within the prescribed bounding period due to this type of error.

A third implication is that whenever any person or household for whatever reason is a noninterview in a given

period, the interview is unbounded for the next period. Over time this lack of bounding probably introduces some error into the estimates, though it is not as substantial as from some other sources, given the substantial interview completion rates in the NCS. Nonetheless in any household or person interview record there can be substantial unbounding. Someone interviewewd only every other time, for example, will always be unbounded.

The amount of error unbounding introduces into the estimates varies by the type of estimate, e.g., whether a prevalence or an incidence measure. Although it is difficult to demonstrate empirically with the data in our file, the bounding procedure may have more of an effect on the prevalence than the incidence rate. Some investigation of the contribution of bounding and unbounding to the prevalence rate seems worth undertaking. Note that for household prevalence rates, the unbounded household and the unbounded member in bounded households may contribute disproportionally to the household being classified as victimized.

The second major effect we noted is that of time telescoping. What should be apparent is that time telescoping exists for all persons first interviewed not because of the interviewer bounding procedure but because the respondent cannot implement a self-bounding procedure, i.e., place the

events in the first designated six-month reference period by referring to previous self-reports. This can be due to many different reasons related to how recall occurs and the nature of truth telling. It seems reasonable to conclude that the respondent is quite likely to recall having previously recounted an incident to the interviewer and it is that recall and censoring of another recounting that operates as bounding. After the first period interview then, within any subsequent interview, there will always be self bounding whether or not interviewer bounding is possible. Thus we always have in effect two bounding procedures operating after the first interview--a self-bounding procedure and an interviewer bounding procedure. In the first interview we can have only self-bounding and errors in self-bounding are referred to as telescoping (though they might better be designated self-bounding or self-recounting or simply recounting errors within that reference period).

Now the conceptual clarification of this problem leads to a conclusion that interviewer bounding procedures are far less consequential within any interview period than are self-bounding procedures. The main difference between first and second interview with the same respondent then is not one of interviewer but of self-bounding. What reinterviewing may well accomplish is to develop a self-bounding procedure in the

subject. It is the knowledge that one has previously reported the incident rather than the interviewer procedure that leads to increased accuracy in recounting incidents within the reference period, or self-rather than interviewer bounding.

We shall try to provide further argument and evidence for this conclusion.

First, as already noted, interviewer bounding is operative only for those respondents who reported an incident in the previous interview, ie., to repeat victims of crime. Since the proportion of repeat victims of crime is small within a population of victims, the opportunity for interviewer bounding is limited. (1) In the aggregate, victimized persons and households show a greater propensity to move than do nonvictimized persons and households. From six to seven percent more victimized than nonvictimized persons move-out before the next interview and five to six percent of all households. This is true for both series and nonseries victimization reporting. (2) The higher the average level of victimization, the greater the propensity to move before the time of the next interview. There is a 75 percent increase in the move-out rate from persons reporting a single victimization compared with persons reporting four or more victimizations. (3) Of persons reporting victimization, somewhat less than two in ten report one or more

victimizations when next interviewed. The comparable figure for households is only slightly more than two in ten. The annual prevalence rate of households (locations) thus is very much a function of <u>different</u> rather than the same households being victimized in the two six-month periods that make up the year.

To conclude, since the prevalence rate of victimization in any period is on the order of one-fifth of all victimized households and since only one-fifth of those will report a victimization in the subsequent period, the proportion of all interviewed for whom the interviewer bounding procedure can be operative is quite small. Correlatively, the self-bounding procedure is operative for the vast majority of all interviewed persons and households. Just how much telescoping occurs because of lack of self-bounding cannot be estimated reliably from current information.

But there is a second reason to call into question the interviewer bounding procedure and its effectiveness. Much of this has to do with the information available to the interviewer at the time of next interview. If the <u>same</u> interviewer is conducting the interview at both points in time, then the interviewer bounding is subject both to the information on the control card for any crime previously reported and the information that the interviewer correctly or

incorrectly recalls about a previously recounted incident. Where the interviewer changes from one interview to the next, only the control card information is available. Note that although the interviewer often completes an incident form (or is expected to do so) to determine whether the incident is previously reported, in neither instance does the interviewer have the previous incident report available for comparison nor is he or she instructed on what criteria determine whether it is the incident previously reported or a different one. Clearly, all of the information that one has available, lacking the prior incident report, is that on the control card--information that often is little more than a type of incident description. Certainly any detailed comparison of current with previously reported incidents is not possible, except that based on an interviewer's recall.

Now observe how another condition has emerged that limits severly the population of events to which interviewer bounding can apply. <u>Interviewer bounding procedures are germane only when a reported incident falls within the same type of crime previously reported, e.g., a burglary or a robbery. The probability of repeat victimization by the same type of crime in the six months following first reported victimization is quite low, reducing the interviewer bounding procedure to a determination in only a very small proportion of all recounted incidents.</u>

We have not done the detailed field and control card work on this grant that permits us to estimate what proportion of incidents could be compared by interviewers at the time of interview; indeed much depends upon knowing more about the way in which information on type of crime was reported on the control card and lacking that information it is difficult to make a precise comparison. The reason it is important to know what is recorded on the control card is that the algorithm used to classify incidents interviewers report does not necessarily lead to the same type of crime classification as that an interviwer enters in the description on the control card.

We can be fairly certain, however, that such comparisons by interviewers can not apply to more than a few percent of all incidents reported in any cross-section, judging from given the matrix of repeat victimization by the same type of crime during the time a household is in sample.

These clarifications and the conclusion based on them suggest that not much would be lost were one to eliminate the current interviewer bounding procedure and especially were one able to reliably estimate the self-bounding error for the first as compared with subsequent interviews. Nonetheless, were one to move to CATI or some other form of interviewing procedure where comparison can be made among incidents

reported in adjacent interviews and a determination made on comparison of all incidents reported in the two periods of time, there might be some small gain in doing so. All in all, the major problem is how to train in self-bounding and the conclusion seems to be that exposure to recounting crime events within a reference period makes for reduced reporting in the next period, some of which undoubtedly is due to self-bounding, i.e., to a constraint against reporting again those incidents that were previously reported.

The foregoing also suggests that it would be worthwhile learning more about how frequently the interviewer bounding procedure actually is invoked in field settings and what are its consequences for excluding previously reported incidents.

Repeat Victimization by Crime

The NCS reports annual incidence rates of victimizations by crime by cumulating all of the victimizations reported in that year. That measure cannot be construed as a measure of individual risk of victimizaton by crime since all victimizations recounted by any victim are included in the numerator of the rate. The households touched by crime measure similarly does not take into account repeat victimization by crime since a housing unit enters the numerator of the rate

whenever the household respondent reports a household crime or any one of its members reports a person crime. Both of these measures then do not provide information on the incidence or prevalence of repeat victimization by crime.

In a separate paper included in the appendix to this report ("Measuring Repeat Victimization in the National Crime Survey and the Special Case of Series Victimization", Proceedings of the American Statistical Association, 1981 Social Statistics Section, Washington, D. C., 1981, pp. 41-50) the principal investigator reports research on repeat victimization by crime. No summary is made of that research here except to note that two main problems in measuring series victimization by crime were considered in that paper. One is the fact that series victimizations, contrary to expectations, fall off quite sharply from one interview period to the next. The other is that there is great difficulty in defining as well as in counting series victimizations by crime.

Series Victimization by Crime as Repeat Victimization. Subsequent to the work reported in that paper we undertook additional inquiry into series victimization by crime. One of the purposes of that inquiry was to determine whether series victimization is disproportionally concentrated among certain kinds of victims and places which might account for some of the measurement and recounting problems that beset it.

Previous work on series victimization reported that it was disproportionally concentrated in certain types of crime. Among series crimes against the person, the NCS reported in 1979 that 70.4 percent were crimes of larceny without contact and that an additional 21.5 percent were assaults; for crimes against households, 62.5 percent were household larceny and 35.3 percent burglary. (Table 1, Appendix III, Criminal Victimization by Crime in the United States, 1979, U.S. Department of Justice, Bureau of Justice Statistics). Most series victimizations then are concentrated in four major types of crime.

A number of hypotheses have been advanced to explain series victimization. One of these focuses on victims of assault, stipulating that multiple victimization by the same type of crime, or series victimization, is especially likely among women who are victimized by repeated assaults of a spouse or a male companion. Another, somewhat broader, hypothesis holds that a substantial proportion of all assaults is among persons who have a close personal relationship and that one is more vulnerable to assault by persons close to than distant from one. In assaults by strangers, repeated assaults are most likely to occur when one is violence prone or in situations where violence is likely to arise. It seemed like a reasonable strategy then to determine the extent to

which intimate relationships and situational characteristics might account for series victimizations.

We began our explorations, therefore, by examining all series assaults, of which there were 1875 in our longitudinal file. To qualify as a series victimization, the victim must report victimization by the same type of crime three or more times during the preceding six-months and be unable to recount each incident separately for an incident case report. A series assault thus is three or more separate assaults for which the victim states he or she is unable to supply the details to separate them one from another. Among the series assaults reported, as the distribution below discloses, the modal series is 3 or 4 separate assaults. Still, it is apparent that at least one-fifth of all series assault victims report 11 or more separate assaults in the previous six months, or , on the average, at least two each month.

Number of Separate Assaults Reported in Series Assaults

Number of Separate Assaults in Series	Total Number	Total Per Cent
Three or Four	785	41.9
Five to Ten	610	32.6
Eleven or More	394	21.1
Don't Know How Many	83	4.4
TOTAL	1,872	100.0

Although spouse assault numbers among series assaults, it is only a relatively small proportion of all series assaults recounted to NCS interviewers. Only 9 percent of all series assaults involved spouse assault with an additional 4.1 per cent involving a relative. The model assault offender is someone who is known to or who can be uniquely identified by the victim, 50.6 percent, with assaults by strangers comprising somewhat more than a third of all series assaults, 36.4 percent.

Relationship Between Victim and Offender in Series Assaults

Relationship	Number	Per Cent
Spouse	164	8.9
Relative, not spouse	75	4.1
Known, but not relative	933	50.6
Stranger	670	30.4
TOTAL	1,842	100.0

Some further evidence that series assaults recounted in the NCS are not all that common among persons who have a close and intimate relationship is seen below in the distribution of place of occurrence of series assaults. Some caution must be exercised in interpreting place of occurrence data for series victimizations since place of occurrence is recounted only for

the most recent incident in a series. Unless the separate incidents in a series are so homogeneous that the most recent is a close proxy for all others or unless we assume that the most recent incident provides a random subset of heterogeneous incidents in a series, we have a biased subset for characteristics of incidents in a series. We shall have occasion to note below that in the aggregate series incidents are far from homogeneous, even for a given victim. We would expect that series robberies or personal larcenies might vary considerably among those in a given victim's series. They perhaps are most heterogeneous for law enforcement officers who are victims of repeated assaults by different offenders.

Place of Occurrence of Series Assaults

Place of Occurrence	Number	Per Cent
In own home or apartment	275	14.7
Vacation home, Hotel, Motel	4	0.2
Commercial building	284	15.2
Office, Factory, Warehouse	54	2.9
Near own home (yard, sidewalk, etc.)	172	9.2
Street, Park, Playground	663	35.4
Inside school	179	9.6
Other place	239	12.8
TOTAL	1,870	100.0

Only about 15 percent of all assaults occur in ones own home or apartment or a vacation home, hotel, or motel, places

where we shall see that spouse assault occurs more frequently than is the case for all assaults. What is apparent is that the modal place for a series assault is open space such as streets, parks, and playgrounds where more than a third of all series assaults occur. We shall have occasion to note how different types of relationships are related to place of occurrence.

Spouse assault occurs primarily within one's own home or apartment; indeed, it is more than five times that expected from the distribution for all series assaults as the following distribution discloses:

Place of Occurrence of Series Spouse Assaults

Place of Occurrence	Number	Per Cent Spouse Assault	Per Cent All Assaults
Own home, Apartment	133	81.1	14.7
Vacation home, Hotel, Motel	1	0.6	0.2
Commercial building	2	1.2	15.2
Office, Factory, Warehouse	-	0.07	2.9
Near own home	7	4.3	9.2
Street, Park, Playground	9	5.5	35.4
Inside school	-	0.0	9.6
Other place	12	7.3	12.8
TOTAL	164	100.0	100.0

Almost one-half of all series assaults occurring in one's own home or apartment are with a spouse assailant (48.7%);

adding relatives (14.3%) accounts for almost two-thirds of all assaults occurring there. Persons known to the victim account for an additional one-third of all series assaults in one's home or apartment so that only 4.4 percent of all series assaults there are by a stranger. It does appear that repeated assault by a stranger in one's home or apartment is an unlikely event, a not unexpected finding. There appear to be two rather different kinds of series assaults occurring in one's home or apartment--those where women are the victims of a spouse assailant and those in which women recount being assaulted by a relative, friend, or acquaintance who is invited into the home or who is a co-resident in the dwelling unit.

Additional support is provided for this interpretation when we examine the place of occurrence of series assaults where a relative is the assailant. Of the 75 series assaults involving a relative, just over one-half (52%) were in one's own home or apartment and 13 percent were near the home.

Although a substantial proportion of all series assaults occurring in one's own home or apartment are by someone other than a relative who is known to the victim, less than 1 in 10 series assaults by such persons occur in the home. The modal place of occurrence for series assaults by persons known to one is, in fact a street, park, field, or playground as the following distribution discloses:

Place of Occurrence of Series Assaults

By Persons Known to the Victim Who are not Relatives

Place of Occurrence	Number	Per Cent Known Non-Relative	Per Cent All Assaults
In Own Home or Apartment	89	9.5	14.7
Vacation home, Hotel, Motel	2	0.2	0.2
Commercial building	124	13.3	15.2
Office, Factory, Warehouse	34	3.6	2.9
Near own home (yard, sidewalk)	116	12.4	9.2
Street, Park, Field, Playground	309	33.1	35.4
Inside school	150	16.1	9.6
Other place	108	11.6	12.8
TOTAL	932	100.0	100.0

We shall be able to explain why this is so when we look more closely at kinds of victims of series assaults but it should be noted here that a substantial proportion of these victims are school-age children. One can see this also in the proportion of victimizations by non-relatives known to the victim that occur inside the school.

From the standpoint of law enforcement, it should be noted that a very substantial proportion of all series assaults then are ones where the victim has sufficient knowledge to identify one perceived as the assailant. As we noted above, only a third are by strangers. But examination of place of occurrence and relationship to victims makes it

clear that the police are more likely to be reactive to complaints of series assaults than proactive since they ordinarily cannot organize preventive or proactive patrol to deal with such repeated assaults. The main exception is for school children going to school and when inside the school.

Finally, we noted that the stranger series assaults were unusual in terms of their place of occurrence in several respects as the following distribution discloses.

Place of Occurrence of Series Assaults by Strangers

Place of Occurrence	Number	Per Cent Stranger Assaults	Per Cent All Assaults
In own Home or Apartment	12	1.8	14.7
Vacation home, Hotel, Motel	1	0.1	0.2
Commercial building	148	22.1	15.2
Office, Factory, Warehouse	19	2.8	2.9
Near own home (yard, sidewalk)	36	5.4	9.2
Street, Field, Park, Playground	320	47.8	35.4
Inside school	28	4.2	9.6
Other place	105	15.7	12.8
TOTAL	669	100.0	100.0

The modal site for a stranger assault on a victim is not unexpectedly a street, park, playground or other open public place. Yet it should be kept in mind that only just under one-half (48.3%) of all series assaults occur in these places. Bearing in mind that these are series assaults on the same victim, it should be apparent that these are not the ordinary

stranger to stranger assaults of a victim. Who these persons are that experience repeated assaults by strangers in public places and commercial buildings is more apprent when we look at kinds of victims below. Here we would simply note that the series assault victims in these places undoubtedly are different from the conventional view of a victim of a stranger assault.

Although we could describe series victims in terms of the conventional categories of age, race, sex, and other socio-economic characteristics, these turn out to give us relatively little understanding of who are series victims. Apart from the relational characteristics already explored, the major explanatory variable seems to be exposure to opportunities for repeated assaults and that may be more a function of how one spends ones time at work or in some regular pursuit than in how one spends one's leisure or time at home. The major way we have of looking at these work related opportunities from NCS data is in terms of ones occupation or, when not employed, in terms of one's position in the labor force such as whether one is going to school. We examine below the occupational characteristics of victims of series assaults. We begin this examination by presenting a detailed listing of the occupational or labor force status of all victims reporting series assaults by the number of times that occupation was

reported for a series assault. Bear in mind that whenever the same victim recounts a series assault in more than one six-month reference period, we shall be counting the number of times that same victim appeared. Our measure thus is somewhat confounded both by any differences in length of time that different kinds of series victims are in sample and of the propensity to repeat victimization. Our description, nonetheless, may mirror a reality of repeated exposure in daily living over which some kinds of series victims have more control than others. To change one's occupation or employer or the school one goes to is more difficult than to decide whether one will go out at night for a leisure pursuit.

Occupations Represented by a Single Series Assault (61 or 3.3%)

Accountants.....Architects Archivists & Curators.....Chemists Personnel & Labor Rel.....Health Technicians, n.e.c. Bus. & Comm. Teachers.....Col. & Univ. Teachers Kindergarten Teachers.....Technicians, n.e.c Voc. & Ed. Counselors.. ..Designers Musicians & Composers.....Painters & Sculptors Writers/Artists/Ent.(nec).Credit & Collection Mgr. Funeral directors.....Inspectors, ex. const. Mgs. & Supts., Bldgs.....Office mgrs., n.e.c. Sales mgrs, ex.

retail....School Adm., college Hucksters & Peddlers.....Real estate agents/brokers Sales reps. wholesale.....Retail salesworker Serivce/Const. Saleswkr...Billing clerk saleswkr. File clerks.....Mail carrier, P.O. Meter readers, utility....Calculating Mach. Ops. Key Punch Operators.....Ship & Rec. clerks Telephone operators.....Carpet Installers Power line installers.....Furn./Wood finishers Glaziers.....Inspectors, n.e.c. Jewelers/Watchmakers.....Loom fitters Dry wall installers.....Furnace tender, etc. Meat cutters/butchers.....Meat cutters, mfg. Furnace tenders/stoks.....Not spec. operators Fork lift, etc. ops.....Brake ops/Couplers RR switch operators.....Carpenters helpers Timber cutters, etc.....Not specified laborer Bldg. clearners, n.e.c....Dishwashers Food cntr/fountain wkrs...Dental assistants Baggage porters, etc.....Welfare service aides Marshalls & Constables.....sk 2 40 Occupations Represented by Two Series Assaults: 80 assaualts

4.3%

Computer Systems Anal...Industrial engineers Mechanical engineers....Lawyers Librarians.....Therapists Clergy.....Recreation workers Teachers,

ex.coll.,nec..Radio Operators Railroad conductors.....Sales mgrs., retail Ins. agents/brokers.....Sales repts, mfg Cntr clerks, ex. food...Estimators/Investigator Statistical clerks.....Stenographers Stock clerks/storekeep..Misc. clerical wkrs Crane/Derek/Hoist ops...Air cond./refrig/heat Automotive body reps....Heavy equip. mechanics HH appliance mechs.....Mechanics & repairers Molders, metal.....Cloth. ironers/pressers Cutting ops., n.e.c.....Punch/Stamp operators Spinners/Twisters,etc...Textile ops., n.e.c. Parking attendants.....Animal caretakers Stock handlers.....Farm laborers Waiters assistants.....Health aides, ex. nurses Attndnts, pers. serivce.Child care workers

22 Occupations Represented by Three Series Assaults:66 assaults

3.5%

Clinical lab techs.....Psychologists Photographers.....Lodge, Union, Soc. off Cashiers.....Clerical supervisors Teachers aides.....Ticket, etc. agents Carpenters.....Excavating mach. ops Locomotive enginers.....Painters, cons., maint Stationary engineers....Meat packers/wrappers

Weavers.....Welders/Flame cutters Gardners, Groundskeep...Lodging quarters clearners Practical nurses.....Barbers Housekeepers.....Pvt. HH cleaners, etc...

12 Occupations Represented by Four Series Assaults: 48 assaults

2.6%

Health administrators...Purch. agents/buyers Typists.....Electricians Plumbers/Pipe fitters...Printing press ops Roofers/Slaters.....Assemblers Misc. ops.....Vehicle washers, etc. Bartenders.....Recreation attendants

8 Occupations Represented by Five Series Assaults: 40 assaults

2.1%

Bookkeepers.....Receptionists Automobile mechanics....Telephone installers Machine ops, misc.....Delivery/Route workers Freight & Meat Handlers.Bank officers

10 Occupations Represented by 5-9 Series Assaults: 72 assaults

3.8%

Occupation	Number of Assaults
Sewers and Stichers	6
Taxi Drivers & Chauffeurs	6
Physicians, Medical & Osteopathic	7
Construction labs./excpt. Carpenter's Helpers	7
Fire Fighters	7
Armed Forces	7
School Administrators, Elementary & Secondary	8
Checkers, Examiners, & Inspectors, mfg.	8
Garage Workers/Gas Station Attendants	8
Bus Drivers	8

Occupations Represented by 10-49 Series Assaults: 231 assaults
13.3%

Occupation	Number of Assaults
Truck Drivers	11
Janitors & Sextons	11
Officials & Administrators, Public Adm.	12
Waiters	12
Nursing aides, Orderlies, Attendants	12
Registered Nurses	13
Restaurant, Cafeteria & Bakery Workers	13
Sales Clerks, Retail	13
Cooks, except private household	13
Elementary School Teachers	14
Secretaries, n.e.c.	15
Secondary School Teachers	16
Social Workers	18
Sheriffs & Bailiffs	20
Guards	38

Occupations Represented by 50 or More Assaults: 337 assaults
18.1%

Occupation	Number of Assaults
Managers and Administrators, n.e.c.	50
Police Officers & Detectives	287

Labor Force and Related Statuses with 20 or More Assaults: 822

44.1%

Labor Force or Related Status	Number of Assaults	Per Cent of All Assaults
Retired	20	1.1
Unable to Work	22	1.2
Unknown status	89	4.8
Going to School	128	6.9
Keeping House	217	11.6
School Age Child	346	18.5

What seems altogether clear is that a relatively small number of occupational specializations and of statuses of those not employed account for the bulk of the series victimizations obtained in the NCS. One must bear in mind that the statistics given above are for all series assault victimizations reported, representing thus neither victims not risk of victimization for an occupational or status group. Yet we can interpret these to some extent as representing both victims and risk of victimization.

The occupational specialty where members are most likely to recount having been the victim of one or more series assaults is law enforcement. Over 15 per cent of all series assaults were recounted by police and detectives. An additional 2.0 percent were recounted by guards and sherriffs and bailiffs recounted 1.1 percent. In all, more than 18.5 percent of all series victimizations are reported by these law enforcement specializations. One can readily understand why such a substantial proportion of series victimizations are

reported by law enforcement personnel. One reason is that they are exposed to assaults from persons who are the object of law enforcement; many of these probably are attempted rather than actual assaults and often the law enforcement officer may experience little injury. The relatively high frequency of this occurrence leads law enforcement officers to recount them as series since they cannot readily recall each individual incident for recounting to NCS interviewers. Another reason why series assaults recounted by law enforcement officers loom large among all recounted series victimizations may be that they are more skilled in recalling and recounting repeated victimizations. Understandably it may be difficult for them to recount the details of each individual incident since they are exposed to a much larger number of crime events during any six month period. Given the difficulty in separating their own victimizations from those of others, especially if most are not all that serious for the officer, they end up being recounted as series events.

Police are somewhat more likely to report larger numbers of events in their series assaults than are all victims of series assaults as the following discloses:

Number of Victimizations	Police & Detectives	All Series
Three or Four	32.1	41.9
Five to Ten	37.6	32.6
Eleven or More	25.4	21.0
Can't Estimate	4.9	4.5
TOTAL	100.0	100.0

The second occupational group that is worth noting is the occupation of teaching. Among teaching specialties, kindergarten and nursery school teachers at one extreme and college teachers at the other do not seem to report many series victimizations. These groups account for few series victimizations by assault. Although not high, we note that elementary and secondary school teachers accounted for 1.6 percent of all series assaults. Although these teachers may be exposed to a smaller number of assaults within any six month period, they do not seem especially prone to repeated assault of 3 or more victimizations in that period of time.

A substantial proportion of all series victimizations are reported by persons who are not currently employed. We note especially the high proportion contributed by school age children who are ages 12 to 18, since they contributed 18 percent of all series assaults. If we add to these the series victimizations recounted by persons over the age of 18 who are going to school, there are an additional 6.9 percent. All in all then, persons going to school account for one-fourth of all series victimizations (25.4%).

We can understand now the information on place of occurrence of series victimizations by examining the contribution of law enforcement officers and students to that distribution.

Percent Distribution of Place of Occurrence of Series Assaults for Selected Groups

Place of Occurrence	Police & Detectives	School Age Children	Attending School
In own Home or Apartment	0.3	5.2	7.8
Vacation Home, Hotel, Motel	0.3	0.0	0.0
Commercial Building	17.1	2.3	13.3
Office, Factory, Warehouse	2.1	0.3	1.5
Near own Home	0.3	10.7	7.8
Street, Park, Playground	59.3	47.4	37.5
In School	0.3	30.9	18.0
Other places	20.3	3.2	14.1
TOTAL PER CENT	100.0	100.0	100.0
TOTAL NUMBER OF ASSAULTS	(287)	(346)	(128)

Here we can see how risk of victimization by assault is disproportionally concentrated for these groups in streets, parks, and playgrounds when compared with the distribution for all groups given previously. Note how the younger school age children are more likely to be victimized in school than are older ones. Note also how police and detectives and to some extent older students are more likely to be victimized in places other than those included in our classification of

places of occurrence. For the police this may be their patrol car, the station, lock-up and other places where arrest, booking, and arrangements for prisoners are made. We see similar distributions for guards and sheriffs and bailiffs.

Persons who are keeping house and victimized by series assault are most likely to be victimized in their home (53.0%); undoubtedly these are the victims of spouse assault. An additional 19.8 percent are victimized near their home--in the yard or on the sidewalk nearby. Almost three-fourths of those keeping house, then, are victimized repeatedly by assault in connection with their home and its immediate environs. Still, pesons keeping house who are repeatedly victimized by assault seem to run some risk when going about away from home, as reflected in the fact that 13.4 percent of them are victimized on the streets, parks or playgrounds and another 4.6 percent in commercial buildings. All in all, however, the hearth is the more common place for those keeping house and repeatedly assaulted.

It is clear that there are patterns to series assault victimization but one must consider whether given the above patterns these victimizatons should be simply added to those that occur against citizens who are not vulnerable to a crime such as an assault because of the nature of their work. This is the case for law enforcement officers. Just as we separate

out law enforcement related deaths and deaths to law enforcement officers in the line of duty, so we might well report series victimization by assault for law enforcement officers and not include them in the regular series victimizations or merge them with all aggravated or simple assaults.

Examination of the data also makes clear that there are some problems in estimating as well as recounting the individual incidents in series victimization. Overall, the percentage unable to estimate the number in a series was not appreciable: for 3.3 percent of the reported series victimizations for all types of crime the respondent said he/she did not know how many incidents were in the series, i.e., respondents could not place the number of incidents experienced in one of the three class intervals provided respondents. An additional 1.2 percent of the respondents had missing data and it is not known whether this is equivalent to a "don't know" or a failure in procedural implementation by interviewers or other processors of the information provided by respondents. Yet there is some evidence that it is more difficult for victims of some kinds of crime than others to estimate the number of incidents in a series. As the following tabulation discloses, there is a variation from 5.1 percent of all burglaries where the respondent said he or she was unable

to estimate the number in a series to only 1.2 for motor vehicle theft and none for the small number of recounted series rapes.

Type of Crime in Series	No Estimate	Can't Estimate	Total Number	Total Per Cent
Rape	0.0	0.0	37	0.5
Assault	1.1	3.3	1,872	24.5
Robbery	1.6	3.8	184	2.4
Personal Larceny	1.8	2.7	2,317	30.4
Burglary	1.2	5.1	1,188	15.6
Household larceny	1.8	4.7	1,953	25.6
Motor Vehicle Theft	3.7	1.2	81	1.0
TOTAL ALL SERIES	1.6	3.8	7,632	100.0

Generally, series victims of burglary and household larceny seem to have greater difficulty estimating the number of incidents in the series than do other kinds of series victims. Just why this should be so is unclear, since there does not seem to be a common explanation for the observed differences. We are disinclined to attribute this to differences in the homogeneity of the incidents in a series since we have found that in the case of series assaults--where the presumption of high homogeneity among the separate incidents prevailed--that for at least some large categories of victims, such as law enforcement officers, the presumption of homogeneity among the incidents probably was unwarranted. The offender should be different in each incident as should place of occurrence, etc.

Indeed, our place of occurrence for series victimizations being based on the most recent reported incident is perhaps somewhat biased (though much depends upon whether we have in effect a random subset by taking most recent reported incident for all series reports).

Some work needs to be done then to see both whether we can measure the number of incidents in a series more precisely and reduce the proportion who can't estimate the number to a negligible one, e.g., by having the respondent report the time of the two most recent incidents and of the most distant in the reference period. But we need to do much more on how most recent is like all other incidents in the series if we are to include them in more detailed analyses of victimization by crime.

The Statistical Bases for Crime Indicators. Statistical indicators of crime commonly are conceptualized and reported as rates. BJS thus reports rates for victimization by crime against persons for all persons 12 years of age and older in the J.S. and for crimes against household property for all households in the U.S.. UCR reports rates for crime incidents rather than victimizations, including both crimes against persons and organizations, for the entire U. S. population.

Almost all of the attention on crime rates has focused on the accuracy of the measurement of the crime variable in the rate--victimizations for the NCS and crime incidents for UCR. Rarely is attention given to the appropriate base for the rate. The principal investigator prepared for the National Crime Commission in 1966 a demonstration of how the base for the rate was also important in reporting crimes as rates. Attention should focus on the denominator as well s the numerator for the rate. By disaggregating crimes in terms of their kinds of victims, principally persons, household organizations, and all other organizations he demonstrated that the rates for household and other organizations were considerably higher when one selected these rather than persons for rates such as burglary. Moreover, for a crime such as robbery that can victimize either persons or organizations, he demonstrated that the rates were higher for robberies against organizations using organizations as the base for that rate than they were for robberies against persons, using persons as the base for that rate.

This line of inquiry was carried somewhat further in a paper prepared for a conference at the Univesity of Aix-en-Provence and included as an appendix in this final report ("Problems in Developing Statistical Indicators of Crime", December 11, 1981.)

We shall summarize only breifly here the major conclusions of that paper. The focus was on both the numerator and denominator of the rate and their relationship. Considerable attention is given to selecting an appropriate base for a rete and the importance of collecting information on that base and tracking it accurately over time. Since that paper was written, we have seen ample evidence that both UCR and NCS assumptions about the base for the rate made for substantial errors in reporting their respective rates. UCR made substantial underestimates of the change in population over the decade and NCS underestimated the growth in households. These resulted correspondingly in UCR overestimating its crime rates and NCS overestimating its household crime rates. Other issues are considered, including whether some kinds of crimes such as robbery shold be computed both for individual and organizational bases when they are crimes of robbery involving an organization.

Attention also focused on conceptions of magnitude and the selection of a base for a rate. It is pointed out that most persons cannot deal adequately with the magnitudes of rates used commonly in crime reporting, e.g., rates per 10,000 or per 1,000 units of a population of persons, households, or organizations. They are more likely to understand chances, e.g., a 1 in 10 chance, or simple percentages. The NCS

prevalence rate is thus far more intelligible than are its rates of victimization per 1,000 persons 12 years of age and older. One might also point out that in the same reports on Households Touched by Crime issued by NCS, one has an even more complex measure that tries to relate incidents of victimization to the prevalence measure in a ratio of incidents to households touched by crime. Such a measure, it is suggested, is even more difficult to comprehend since it involves grasping the concept of a ratio and of an average for a population of victimized households.

The paper also considers the problem of developing new measures of crime and of bases for rates. It should be noted that the base for the rate in statistics on how much crime is there are collected by a different set of agencies than those who collect the information on crime (or at least in different parts of agencies as in the case of the U. S. Bureau of the Census collecting information on household and population sizes in the Population and Housing Divison and on victimizations by crime in the Crime Surveys Division). Where the statistics are of processing crimes and offenders,as in criminal justice statistics, both the numerator and the base for the rate are collected by the same agency. Where these are separated, it may be more difficult to monitor changes in the base as well as in the numerator. Yet the monitoring of

changes in the base is as critical as that of the numerator if one is interested in changes in crime rates.

Substantive Research

We continued a line of inquiry into substantive problems of victimization by crime. Two were of special interest and work on them is reported here. The first of these is that of victim proneness to victimization by crime and the second that of how consequential is crime to its victims. The consequences of crime for victims was examined in two different ways--in terms of how serious are the consequences of crime to its victims and the other that of the measurement of consequences in the NCS, a measurement as well as a substantive issue.

Victim Proneness. The NCS currently provides relatively little information on individual risks of victimization by crime. This is partly owing to the fact that the unit of NCS reporting ordinarily is a victimization by crime, i.e., an individual's recall and recounting of a crime experience. UCR, correlatively, has focused on the crime incident. Neither treats the individual victim as the unit of analysis so that one can calculate riks of being victimized by crime. There is substantial interest also in not only whether but

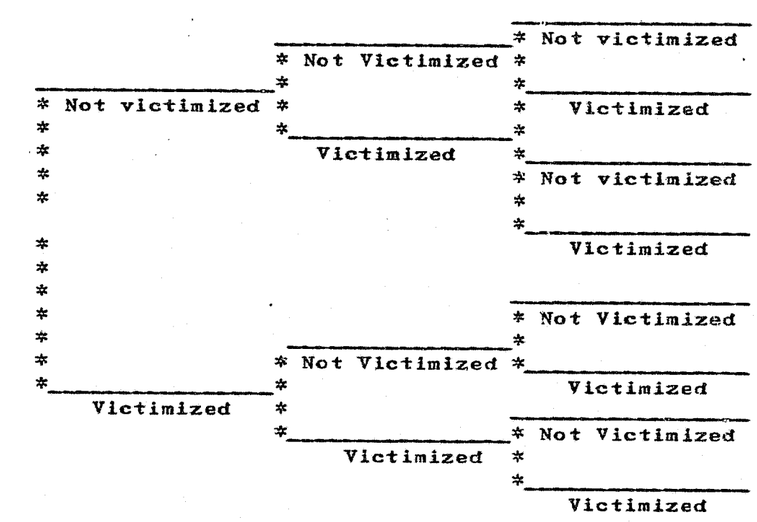
when one will be victimized, i.e., in the frequency of its occurrence and of the time between victimizations. We have done some previous research on that question and in this period looked at some additional features of victim proneness, i.e., whether some individuals are more likely to experience repeat victimization than are others. The victim proneness question is related both to questions of individual risk and to those of explaining differences in risk.

Work on victim pronessess in this period focused on developing and testing a model of victim proneness. In doing so we examined reponse sets by respondents and the effect of time-in-sample on reporting of victimizations.

Developing a Model. In developing a model of victim proneness we began with a simple model that victims who experienced repeated victimization (or multiple victimizations) within a period of time are prone to victimization. Proneness in this sense also implies that households victimized are not part of a random process but rather that something enhances their probability of being victimized.

We began the process of model construction by thinking of each household as involved in a binary outcome of victimized or not in successive six-month intervals of time they are in

sample in the NCS. This is akin to a binary "tree" where the information in the tree are reports of victimizaton from NCS households for each six-month reference period for the time the households are in sample. The tree then is based on the fact that many households were in the NCS sample for several interview periods and that in each of them each household could recount either no or some victimizations. The tree is represented in the diagram below.



Within the longitudinal file, any given household could be in sample for one to nine interviews. Normal panel

rotation allows for seven interviews but the start-up rotation design provided for some to remain in sample for eight or nine interviews. There are from 2 to 512 different patterns of response possible in this tree, given the possibility for a household providing information on their victimization experience for nine six-month periods. We actually output such a tree but this paper is based on a reorganization of those data and a substantial reduction of the information contained in the tree.

By looking at the patterns of responses, several queations can be addressed. One interesting question is whether any of the patterns are significantly more likely to occur than others, and, if so, why? Another question is whether patterns of responses are related to the length of time that a household is in sample. Still another is, what is the total proportion of households that is victimized over time and what proportion is victimized any given number of times? Are there any distinguishable patterns of multiple victimization?

The work reported below focuses on two of these questions: the relationship of reporting victimizations to length of time in sample-- <u>persistence</u>-- and whether certain households may be regarded as <u>victimization prone</u>, <u>i.e.</u>, <u>they report victimizations more often</u> than would be expected by chance.

Persistence in Sample and Recounting Victimizations.

There are several reasons to expect households that report no victimizations will remain in the sample longer than households that report them. The first is that victimization by crime may lead households to change their residence. So, if a household reports a crime in one period, it is less likely to be at the same location for the next interview. We have, indeed shown that is the case in our previous research on repeat victimization by crime with highly victimized households more likely to move than those with low victimization and victimized more likely to move than nonvictimized ones.

The second reason is that it may well be that more of the mobile than the nonmobile households live in high crime rate areas. Some work on crime in urban neighborhoods suggests that norms and informal enforcement of them are weakened or break down when the population in an urban neighborhood is highly mobile. Transient households aid in creating an environment where more crime occurs.

The third reason is that respondents that remain in the sample longer may learn to report fewer victimizations on the Screener Questionnaire in order to avoid having to respond to the questions in the Crime Incident Questionnaire.

The fourth reason is that victims are more likely to telescope victimizations that occurred from a prior six-month reference period into the present one, especially during the first interview and perhaps more for early than later periods that one is in sample if self-and interviewer-bounding are accepted.

There are, on the other hand, reasons to expect reports of victimizxation to increase with length of time in sample. Prime among these is an increased awareness or sensitivity to recalling and recounting victimizations and out of a growing familiarity with NCS questions that lead one to "store" information on experiences between interviews in a way they are more readily recalled and recounted to the survey interviewer.

One way to use the information in the tree to look at this problem of how persistence in sample affects victimization reporting is to compare the number of households reporting victimizations on the basis of their length of time in sample.

Table 1: Proportion of Households Reporting Victimizations in at Least One Period by Length of Time in Sample

Number of Periods on Sample (a)	Number of Households (b)	Proportion Victimized (c)	Standardized Proportion (c/a)	Difference from Last Period
1	39,482	.266	.266	———
2	41,083	.339	.170	.096
3	33,133	.377	.126	.044
4	27,486	.410	.103	.023
5	22,086	.459	.092	.011
6	18,356	.487	.081	.011
7	8,477	.515	.074	.007
8	7,407	.532	.067	.007
9	3,041	.556	.062	.005

Table 1 presents the proportion of households reporting at least one victimization by length of time in sample. It also shows a "standardized proportion" that is obtained by dividing the proportion of households reporting victimizations by the numbmer of periods the contributing households were in sample.

As expected, given the tree structure, victimizations cumulate with length of time in sample: the proportion of households reporting at least one victimization increases with length of time in sample in Table 1, with about 27 percent reporting one or more victimizations for one period in sample and 56 percent of those who were in sample for nine periods. Put very simply, the longer the time any household is in sample, the more likely it is to report one incident. This

finding is congruent with the idea of exposure to risk. Given a constant exposure to risk, the longer the period of time elapsed since last victimization, the more likely one is to be victimized.

It is unfortunately the case that the design of the NCS slices into a household's victimization experience over time at different points in that household's career in victimization. We do not know for any given household what is its experience with victimization prior to entering our sample.

We can "standardize" the length of exposure to victimization for our households by dividing the proportion of households recounting incidents by the number of periods the households are in sample. This give us a crude measure of the average proportion of victimizations contributed by households within each period. In Table 1 we observe that the longer households are in sample, the smaller is the standardized or average proportion of victimizations recounted per period. This means that the longer a household remains in sample, the less likely it is to contribute a victimization incident.

Furthermore, Table 1 presents information on "Difference from Last Period" which shows that for each of the first five periods of time in sample, the average proportion is always

approximately half that of the preceding period. (The relationship does not hold for households six or more times in sample). The change from one time period to the next thus appears to dimish by a constant.

A second way to utilize the data in the tree model is to examine the proportion of households recounting victimizations in just their first time in sample by their length of time in sample. This helps in determining whether the initial or bounding interview predicts time in sample. The relevant information for this examination is in Table 2.

Table 2. Proportion of Households Recounting Victimizations in the First Interview by Length of Time in Sample

Length of Time in sample (a)	Number of Households (b)	Proportion Victimized (c)
1	39,482	.266
2	41,083	.242
3	33,133	.207
4	27,486	.195
5	22,086	.191
6	18,356	.188
7	8,447	.184
8	7,307	.184
9	3,041	.195
Total	200,521	.219

Examination of Table 2 discloses a monotonically decreasing series of proportions of households recounting victimizations in the first interview, if we disregard

households that are in sample for nine interviews. The longer a household remained in sample, the less likely it was to have reported a victimization in the first period. The difference in Table 2 between one and eight times in sample is 8.2 percent. Therefore, nearly a third (30.8%) fewer households that remain in sample until the eight period report victimization in the first period than households that are in only once. Furthermore, only households that persist one or two periods are above the average proportion of victimizations reported for all households.

Several conclusions can be drawn from the information in Tables 1 and 2. Of the four reasons given above as to why one might expect that the longer a household remains in sample, the less likely it is to report victimizations, information in the two tables are consistent with the ideas that households may move because of crime and that households in higher crime areas may be more mobile than those in lower crime areas. The consistency arises from the fact that the tables show that households which persist less often recount victimizations. Although these tables do no dispute the notion that households which remain in sample learn to report fewer victimizations in later interviews--an analysis of recounting between periods would be necessary to support this explanation--neither do they support it. Rather, they suggest that households which

remain in sample start out, so to speak by reporting less. We note there is a special problem in this interpretation, given the way we have sliced into victimization histories of households. An unknown proportion of the households who do not report in the first period undoubtedly were victimized in the previous period and so on; we might thus visualize sucessive cohorts of victimized for which this cross-section is an inadequate representation.

The tables also suggest that while increased sensitivity to recalling and recounting information on one's victimizations by crime, especially on the part of household respondents, may lead to enhanced reporting in later interviews--we have no evidence from these tables that it does--any effect it has is overwhelmend by a tendency to report fewer victimizations.

To sum, then, households persisting in the sample are likely to be households that report less crime either because they are victims less often or because they do not recount their victimizations for whatever reasons. Using the household as a unit for reporting victimizations over time thus may bias rates unless length of time in sample is controlled for. Further analysis of the relationship of length of time in sample to recounting victimizations undoubtedly should pay attention to its antecedents. Among

other important features of a design are to follow individuals and households for longer periods of time and to look at cohorts of respondents.

Learning, Victimization Proneness, and Response Sets. Victimization might just well be a random experience--an outcome of a stochastic process where all households are equally likely to be victimized. Experience and other crime statistics and analyses suggest strongly, however, that some households have a much greater risk of victimization than others. One matter we sought to investigate in this connection is how much greater the risk is for some households than others.

We conceptualized the risk of victimization in terms of "proneness". The term "prone" conjures images of repetition. Someone who is "crime prone" then is repeatedly victimized.

We shall now present a model of proneness in terms of an expected probability of multiple victimization. This model will be used to examine the relationship between reports of victimization in one period of time with reports of victimization by the same household in the succeeding period.

A household was defined as victimization prone in terms of its rate of victimization by crime. The more frequently a household is victimized, the more prone it is to victimizaton

by this definition. We shall confine our exposition and examination of the model to data for only two successive time periods so that a household that is victimized in both periods is operationally defined as victimization prone. The main question to be answered using this definition is whether more or fewer households are victimized than would be expected by chance. If more, then we can say that the excess of victimized households exists because of victmization proneness.

Correlatively, if there are fewer than expected by chance, an alternative explanation is called for. One such alternative is a learning hypothesis that stipulates a household learns from its experiences of victimization by crime to avoid or avert the possibility of future victimizations. This avoidance learning reduces it's chances of victimization by crime in the future. A household's members may not simply learn how to avoid being victimized in the future but it may actively take steps to alter the chances that it will be victimized. Thus one expects that victims may gain some motivation to prevent future victimization that actually alters its risk.

Were proneness and learning the only competing explanations for any difference between expected and observed victimizations by crime our analysis and interpretive problems

would be simplified. Unfortunately there are competitors. An important one is the "response preferences" or "response sets" of respondents in the NCS. Some respondents may prefer not to report their victimizations; others do not recall them at the time of the interview; others may overreport the number of victimizations. If a households members recount victimizations in two successive periods of time, the fact that they recount more in the second than the first period may not be due to a change in actual victimization experience but to improved reporting by the second period or, for example, by a desire to please the friendly interviewer. The design of the NCS also introduces measurement error in collecting information on victimizations which may be related to time in sample; the propensities of people to distort information is well known.

Given the possibility of response sets and other forms of measurement error, the most that can be concluded from our analysis is that any observed effects are due either to <u>proneness and</u> response set effect or to <u>learning and response set effects</u>.

In developing a model of "proneness", the first matter to consider is the probability of a household being victimzed more than once by chance. We can write the probability (p) of victimization (v) in a particular period (n) as:

p(V(n))

This probability can be expressed for a particular period as the number of victimizations reported for a period divided by the total number of households reporting in that period. Thus, for the first interview period, there hypothetically might be 23,000 victimizations and 100,000 interviews, giving a probability of victimization of .23.

If we treat the households for each interview period as either "victimized" or "not victimized", we have a distribution of events that is binary in nature. A binary distribution then can be used to determine the likelihood that multiple victimizations are random occurrences.

Suppose there is a probability p(V(1)) of victimization for the first period that a household is in sample and aprobability p(V(2)) of victimiatin for the second period. Then the probability of victimization (p(V(1) or p(V2))) in <u>either</u> one or the other period is:

$$p(V(1) \text{ or } V(2)) = p(V(1)) \quad p(V(2))$$

but the probability of victimization (p(V(1) & V(2))) in <u>both</u> periods is:

$$p(V(1) \,\&\, V(2)) = p(V(1)) * p(V(2))$$

The extension of these equations for multiple periods are straight-forward. For the probability of victimization in at least one period, assuming that victimizations are independent events, we have:

$$p(V(1) \,\&\, V(2) \text{ or } \ldots \text{ or } V(n)) = p(V(1)) \quad p(V(2)) \quad \ldots \quad p(V(n))$$

and for the probability of victimization in all periods, we have:

$$p(V(1) \,\&\, V(2) \,\&\, \ldots \,\&\, V(n)) = p(V(1)) * p(V(2)) * \ldots p(V(n))$$

If victimization is a random experience and all victimizations are independent events, then actual household victimizations should be reasonably close to those predicted by the models. If repeat victimization is affected by learning, predictions from the original model will be too high, because learning curtails victimization in the second and subsequent periods. If, on the other hand, proneness occurs, predictions from the model will be too low, because they will not take into account the effects of factors that predispose to proneness to victimization by crime.

Unfortunately, we cannot separate the learning from proneness effects in the model above. If empirical data show

less victimization then expected, we can say that there is a "marginal" decrease in victimization due to learning that is the net effect of learning (and/or response effects) after effects of proneness to victimization by crime have been filtered out.

Likewise, if data show more victimization than expected, we can say that there is a "marginal" increase in victimization due to proneness (and/or response effects) that is the net effect of proneness after effects of learning have been filtered out.

The magnitude of this marginal effect is the difference between the observed probability of victimization by crime and the expected probability of victimization under the model. If it is negative, then we shall attribute the effect to "learning" and if it is positive, to "proneness".

The Model Tested with NCS Longitudinal Data. We can apply the model to the NCS longitudinal data and examine whether the victimization reports of households are consistent with a model of random victimization, of learning, or of proneness.

Table 3 presents the total responses for the first two periods for all households in sample two or more periods. As can be seen, 30.55 percent of these households reported

victimizations one or more times during those two periods, while 5.73 percent reported victimizations both periods.

From Table 3, it is possible to calculate the odds that a household reporting no victimizations the first time in sample will report victimizations in the next interview, period 2. This is:

Period 2 = yes : Period 1 = no

Table 3. Victimization Reports in Period Two by Reports in Period One for All Households

Period Two	Period One: Not Victimized	Victimized	Total
Not Victimized	111,845	24,135	135,980
(Percent of Total)	(69.45)	(14.99)	(84.44)
Victimized	15,832	9,227	25,059
(Percent of Total)	(9.83)	(5.73)	(15.56)
Total	127,677	33,362	161,039
(Percent of Total)	(79.28)	(20.72)	(100.00)

chi Square = 4686.5561 P< .001

15,832 : 111,845

which yields a probability of 0.1416.
The similar odds for a household that did report victimization in period one are:

Period 2 = yes : Period 1 = yes

9,227 : 24,135

which gives a probability of 0.3823. The ratio of these two probabilities is 0.3823/0.1416 = 2.70, or in other words, a household that reports a victimization in the first period is 2.7 times as likely to report one in the second period as a household that does not report a victimization in the first period. (This ration also holds reasonably constant when length of time in sample is controlled.)

Table 4. Expected and Observed Probabilities of Reporting Victimization in Periods One and Two by Length of Time in Sample

Number of Periods in Sample	Proportion in Period One (P1)	Proportion in Period Two (P2)	Expected P1 or P2 (P1 P2)	Observed P1 or P2	Expected P1 and P2 (P1*P2)	Observed P1 and P2	Ratio Observed to Expected AND
2	.242	.157	.399	.399	.038	.059	1.56
3	.207	.164	.371	.371	.034	.059	1.74
4	.195	.157	.352	.352	.033	.058	1.77
5	.191	.158	.349	.349	.030	.058	1.93
6	.188	.148	.366	.366	.028	.055	1.98
7	.184	.143	.327	.327	.026	.049	1.88
8	.184	.140	.324	.324	.026	.050	1.94
9	.194	.137	.331	.331	.027	.054	2.00
ALL	.207	.156	.363	.363	.032	.057	.179

We now know that there is a 2.7 times greater chance that a household reporting victimization in the first period will report a victimization in the second period than will a

household not reporting a victimization in the first period. Part of this likelihood is due, however, to structural considerations involving the distribution of victimizations. That is, while there are only two possible events for each period (victimization report or no report), the probability of the two events is not equal, and therefore a household's odds of reporting are not strictly distributed in a binary fashion.

The model presented above takes this into account, by establishing the expected likelihood of victimization. The expected probabilities of victimization for households in sample ach possible length of time are calculated between periods one and two and the displayed in Table 4. The table also displays the observed probabilities for comparison.

From Table 4, we see several things. First, the proportion of victimization reports in both period one and period two tend to decrease (though not monotonically) by length of time in sample. Again we see the phenomenon where more persistent households are less likely to report victimizations, at least in the early periods. The decrease in expected probabilities of victimization is, of course, tied to the decrease in reporting.

The expected probability of victimization in two successive periods hovers around .03, or about one-tenth the

porbability of being a victim in only one or the other period. But, the actual incidence of successive victimization reports is considerably higher than expected. The ratios show that a household in sample only 2 periods is 1.56 times more likely than expected to report a victimization the second period if it reported one in the first period than is a household that did not report a victimization in the first period. As the number of periods in sample increases, so does the importance of the initial report in regard to a second report. A household in three periods in 1.74 times more likely than expected to report victimization in the second period after reporting one in the first period than a household not reporting one in the first period, and a household in sample eight periods is fully 2.00 times more likely than expected.

The initial interview therefore tells us quite a bit about what a household is likely to report in future periods (at least the second one). It suggests that certain households are more likely to contribute to the victimization statistic than others, and that the contribution begins immediately. For more stable households, in terms of persistence in sample, those certain households are up to twice as important as other households. Whether this is due to a response set on the part of the respondent(s), or to salience of victimizatio, or to actual victimization is not

discernable from the tree. But we can say that, on balance, victimization proneness is more likely to occur than learning.

The Consequences of Victimization by Crime. Considerable interest attaches to the consequences of crime for a number of reasons. Among them are first of all the fact that the consequences of crime enter into the definition of types of crime, especially as to their seriousness. Even characteristics of a crime incident, such as a threat of crime, can be seen as serious because of its consequences, i.e., the threat is in some sense consequential to the person who is the object of the threat. Consequences also are of interest since they tell us about individual experiences with crime and their costs to victims—economic, social, psychological. An interest attaches to consequences, also, in terms of their prevention and their melioration by victims and by organizations in society. And finally, interest attaches to them because they tell us about how and why events are processed as they are by both victims and by the system of criminal justice. Victims, for example, are much less likely to call the police for crimes where they experience no injury or loss than for those where they do.

The Seriousness of Crime. A major problem in reporting on crime in the United States is the issue of how serious are the individual crimes that comprise the statistics on serious

crime. Historically, serious crime was conceptualized in terms of the major crimes against persons and their property and institutionalized in Uniform Crime Reporting in two modes as Part I and Index Crimes. They now are virtually identical so that the Index Crimes of Criminal Homicide, Forcible Rape, Robbery, Aggravated Assault, Burglary, Larceny-Theft, Motor-Vehicle Theft, and Arson are treated as Part I offenses,with arson dropped from the crime index . NCS includes simple with aggravated assault and does not collect information on either criminal homicide or arson.

Setting aside issues of the accuracy of information collected and particularly those associated with unreported or the dark figure of crime, there are genuine issues in crime reporting as to how such statistics will be viewed, especially in terms of some criteria of seriousness. Now the major way that seriousness is conceptualized in this country is in terms of legal categorical definitions of crime where consequences for victims are taken into account but where sanctions attached to the crime category are the major indicator of the seriousness attached to that crime. Regardless of the source of the conception of seriousness, however, it is apparent that titles of crimes conjure up images of seriousness. Thus assault conjures up an image of seriousness with assault being generally regarded as more serious than a motor vehicle theft.

And an aggravated assault conjures up an image of seriousness that is more seious than that of simple assault.

The NCS reports both for broad categories of crime: Rape Robbery, Assault, larceny, burglary, and motor vehicle theft and distinguishes them also in terms of other criteria of seriousness, e.g., whether or not force was used, whether or not coercion was used, and whether or not the crime as attempted or completed, although not consistently so. There are no attempted robberies, for example, since these fall into larceny reporting. These distinctions reflect differences in the seriousness of victimizations and crime incidents using legal criteria of seriousness. NCS also reports whether or not the victimization was reported to the police and reasons for not reporting, measures that provide some indication of how serious the victim regards the victimization. Yet this measure ordinarily is not given as a measure of the seriousness of victimization in reporting NCS rates. One does not get, for example, two rates one of which is for all crimes known to the police and another for those not known.

NCS reporting while doing more than UCR in differentiating among the seriousness of crimes in reporting rates of victimization by crime or crime incident rates, nonetheless, could do far more by systematically constructing different kinds of crime rates in terms of their consequences.

The paper on "How Serious is Serious Crime?" included as an appendix to this report draws upon the NCS and UCR in discussing the issue of measuring and reporting on crimes in terms of the seriousness of their consequences. What is clear is that NCS includes a disproportionate number of incidents that have little if any consequences for the victim other than psychological ones. And, although, measures of absolute losses are less satisfactory than relative ones, they do not loom as very consequential in terms of the amount of loss.

This line of reasoning leads to some suggestions about NCS reporting that are not taken into account in the attached paper Four are discussed here: the need to present measures of repeat victimization by crime; the need for measures of psychological consequences; the need for measures of relative loss, and the need for more disaggregated reporting with rates reported in terms of indicators of consequences or indexes constructed from them. Each of these is treated below.

Measures of Repeat Victimization by Crime. The NCS currently has no measure that reports repeat victimization by crime other than its ratio of the number of incidents to the households touched by those incidents. The ratio is a crude indicator of multiple or repeat victimization of households during a year. But as BJS reported in launching its measure of prevalence of crime as households touched by crime, other

indicators of repeat victimzation are essential using a longitudinal file or a panel design. (The Prevalence of Crime, Bureau of Justice Statistics Bulletin March, 1981, p. 3). Although we have not developed precise estimates of such indicators because of problems of estimating for a cross-section based on missing information for individuals in households and for households, such measures are feasible, especially with a panel survey in which households and individuals are followed over time. Nonetheless, we have shown that within any cross-section there is a substantial minority of multiple victims and over time of repeat victims. The aggregate amount of victimization depends in part upon the period for which it is reported. Discounting series victimizations, we have some households in the NCS file reporting as many as 40 separate crime incidents during the three year period the household is in sample.

Psychological Consequences.

The NCS does not routinely collect information on the psychological consequences of victimization by crime. Although survey measures of psychological consequences exist both as subjective and objective measures, they are not altogether adequate. The subjective measures of fear of crime generally do not separate fear of crime from fear of

victimization by crime in a way that clearly distinguishes them and the objective measures are generally based on a few indicators of changes in behavior as a consequence of having been victimization. Moreover, no information is collected on how victimization of others has psychological consequences for persons in the survey.

There is a range of measures of psychological consequences that might be developed, including those relating to other subjective consequences than a simple measure of fear. Among them are measures of the concern for ones privacy and safety. Intensive interviews show that antipathy towards offenders and the system of law enforcement and criminal justice may be more common than is fear of repeat victimization. There are strong emotional reactions of anger and frustration that continue over time.

The range of behavioral measures and the possibilities of constructing an index of behavioral changes as a consequence of actual victimization or of perceived probabilities of being victimized need to be investigated. Bayesian estimates might be one way to approach the problem of subjective consequences of victimization. Again, we call attention to the fact that a panel survey or exploitation of the current longitudinal design will permit us to determine the persistence of psychological consequences over time. There is reason to

expect that the subjective consequences of harm by crime persist over much longer periods of time than do objective ones. But a panel design will permit one to investigate also what accounts for differences in the rate at which psychological and other consequences persist over time and of what accounts for differential rates of change in persistence. The latter may be especially important in aiding victims in dealing with those consequences.

Measures of Relative Loss.

Most of the measures currently used to measure the consequences of crime in the NCS are absolute measures of loss, e.g., the dollar losses from crime, days lost from work, kind of physical injury or loss. There are a few relative loss measures such as amount of loss recovered by insurance payments and whether or not stolen property was recovered.

Yet, it seems apparent that none of the measures currently developed and reported takes into account relative loss in terms of some evaluative criteria of losses. Thus a dollar loss to a person of low income is far more consequential than the same loss to one of high income. Hence the possibility exists of developing measures of consequences in terms of ones income. One can also develop measures in

terms of whether or not the losses have been or can be in any sense replaced. There are symbolic measures of loss because objects are endowed with unique value, e.g., the loss of a gift, wedding or anniversary present, an inherited object, etc.. which invest dollar losses with other values related to the replacement of losses. Some of these measures such as those related to income can be developed with current information from the NCS; others require the collection of new indicators.

What is clear is that the current NCS method of collecting information on the consequences of victimization by crime and of reporting it underestimates the extent and persistence of consequences. This is owing in the first instance to the cross-section collection and reporting of information on consequences of victimizaton by crime. And it is owing in the second instance to the fact that the current design is limited in its capacity to collect information on the consequences of crime.

Work done on the consequences of crime led the investigator to conclude that most crimes do not have very serious consequences and that losses are gnerally neither large and there is a fair amount of recovery of losses by insurance. Yet as the research proceeded it was clear that even with a lognitudinal design where individuals and

households are followed during their time in sample, no provision was made for following previously reported incidents of victimization in the following interview. Hence the panel file could provide information on repeat victimization by new incidents of crime but not of continuing victimization by previously reported victimizations crime.

The failure to follow previously recounted incidents in the next interview period has consequences also for the cross-section estimates. The information a respondent is able to provide on the consequences of a victimization by crime depend upon the time that has elapsed between the occurrence of the incident in the reference period and the time of its recounting to the survey interviewer. The longer the elapsed time from occurence to interview, the more likely it is that the respondent will have the information required for reporting consequences.

A separate paper investigates that issue and it is included in an appendix to this report ("Effect of Time or Occurrence of Crime Events on Victim Reporting of Losses to NCS INterviewers"; Report 5). The basic conclusion of that paper is that the shorter the time interval between occurrence and reporting, the more likely (though depending somewhat on the particular kind of loss or injury) one is to report at the time of interview that the matter is still pending and the

less likely one is to report any recovery of losses. This paper provides information that strongly suggests only a panel design where one follows persons and households can provide accurate estimates of losses and injury by crime and their consequences for victims. This matter is being considered further in the NCS redesign Consortium.

Reporting the Consequences of Crime.

We observed previously that there is a need for both more disaggregated reporting of crime rates in terms of measures of seriousness of crime defined in terms of its consequences for victims. We also have suggested that indexes might be constructed in terms of the consequences of crime.

It would be no simple matter to construct such indexes, given the different and differing standards of value held by victims. We have already noted that objects with monetary value may have symbolic value as well. Perhaps the easiest measure to construct would be one based on dollar loss. If such a measure is to be constructed, however, more information must be secured on monetary losses from the several different kinds of consequences. Thus we have measures of days lost from work but no measure of its economic value. It perhaps would be a fairly simple matter to calculate what a day's work

costs but should one not consider opportunity costs for the housewife, retired, or unemployed as well?

But the most important part of this work will be the development of the measures of crime seriousness. The major measures we have at present are in terms of injury or not for person crimes and dollar losses for property crimes. Yet this distinction is not consistently followed for crimes against persons or property and certainly not in relation to both. Thus there is a reporting of injury and no injury for robbery and assault and it is assumed that all rapes involve injury. But there is no indication of the amount of injury for those injured in the reporting of rates. Reporting on amount of injury is by percentage distribuitions for victimizations, not rates. Similarly, household larceny rates are reported by amount of loss, but no such rates are reported for burglary or motor vehicle theft. Finally, crimes against persons can involve both physical inury and property loss, but we have these combinations reported only as percent distributions, not as rates. One can note for example that the dollar losses are far greater in forcible than nonforcible entry burglaries and that the robberies with injury seem to involve somewhat greater dollar losses than those than do not, but not the rate at which these occur. One would expect, of course, that for some types of crime, the sample cases are too

small to calculate reliable rates, but that is a matter for empirical verification. (Note that in and of itself, small sample sizes tell one something about the probabilities of victimization by that kind of crime and its consequences.

Reporting the Research

Originally it was intended to pull together work done under the several grants into a single monograph. That strategy was altered for a number of reasons. First, the principal investigator found that publication of the results of some of the research was made more meaningful by being addressed to specific audiences, e.g., those developing measures of crime, lawyers who legislate with respect to crime matters, or to specialists in survey design and measurement. A second reason was that the establishment of the Consortium to Redesign the NCS meant that it was often more strategic to feed research results and findings into that Consortium than to spend resources on a monograph. This has meant also another form of publication of findings--the many items and messages contributed to the NCS Redesign Consortium and several papers addressed specifically to redesign issues. Although some of that work was sponsored by the subcontract to Yale for redesign work, much of it was done also in connection with this grant, especially that relating to the programming

of output from the longitudinal file and analysis. There are many intangibles as well, such as the development of expertise on this project that has fed into the redesign efforts. Not the least of these has been the way that work in developing and working with the longitudinal file has led to a redesign effort to create a longitudinal file and collect information in a form that permits cohort and panel analyses.

There follows a list of the published and unpublished reports in the appendix that were part of the current research effort in whole or in part:

"Understanding Changes in Crime Rates", in Stephen E. Fienberg and Albert J. Reiss, Jr. (eds.) Indicators of Crime and Criminal Justice: Quantitative Studies, Washington, D. C.: U. S. Department of Justice, Bureau of Justice Statistics, NCJ-62349, June, 1980, pp. 11-17.

"Victim Proneness in Repeat Victimization by Type of Crime", in Stephen E. Fienberg and Albert J. Reiss, Jr. (eds.) Indicators of Crime and Criminal Justice: Quantitative Studies, Washington, D. C.: U. S. Department of Justice, Bureau of Justice Statistics, NCJ-62349, June 1980, pp. 11-17.

"What Do We Know About Crime?", in Norman Cousins (ed.) Reflections of America: Commemorating the Statistical Abstract Centennial, Washington, D. C.: U. S. Bureau of the Census, December, 1980.

"Public Safety: Marshaling Crime Statistics", The Annals of the American Academy of Political & Social Science, 453 (January, 1981)222-236.

"Effect of Time of Occurrence of Crime Events on Victim Reporting of Recovery of Losses to NCS Interviwers" Report # 5, Panel Studies in Victimization by Crime, Yale University, Institution for Social & Policy Studies, January, 1981

"Foreword: Towards a Revitalization of Theory and Research on Victimization by Crime", Journal of Criminal Law and Criminology, 72 (1981)704-713.

"Problems in Developing Statistical Indicators of Crime", forthcoming in Connaitre la Criminalite: le Dernier Etat de la Question, Aix-en-Provence:Universitaire Presse, 1983.

"Measuring Repeat Victimization in the National Crime Survey & The Special Case of Series Victimization", Proceedings of the American Statistical Association: 1981 Social Statistics Section, 1981:42-50.

"How Serious is Serious Crime?" Vanderbilt Law Review, 35 (April, 1982)541-585.

A Concluding Note

The submission of this final report concludes the statement of work completed under the grant in terms of the original grant objectives. Yet, the body of information that has been developed in the file and in the programmed output is rich indeed so that the principal investigator intends to mine it for information related to redesigning the NCS as a member of the Consortium to Redesign the National Crime Survey and for additional scholarly publication.

Current work will illustrate how this body of information continues to be relevant and in an important sense a continuing effort without drawing upon additional funding. Stephen E. Fienberg has asked whether the work on panel attrition cannot be included in a volume that he is editing for the American Statistical Association and a paper accordingly is being prepared for submission. This is an example of how the rich results continue to feed into scholarly publication. At the present time the principal investigator also is undertaking work on a subcontract with BSSR on redesigning the NCS. Part of that has called for participation in a subcommittee on longitudinal design and another part for developing measures of consequences of victimization. The materials generated under this grant continue to be relevant to that objective and are being mined for that purpose.

From this perspective, the project also represents an investment in a dual sense. It has been an investment in obtaining additional information from the longitudianal file in the form of output that is germane to continuing work on the NCS and continuing scholarly publication. And it has been an investment in the principal investigator so that he may contribute to the many redesign questions that must be dealt with by the Consortium and its research members.

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