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Investigating Repeated Victimization with the NCVS

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Chapter One

Introduction

Repeated victimization has captured the imagination of the criminal justice and criminological community as a tool for understanding the occurrence of crime and allocating crime control and crime prevention resources (Pease, 1998). A decade of pioneering work in England emphasizing repeated burglary victimization has found that prior victimization is the best predictor of subsequent victimization and that this subsequent burglary victiniization occurs a very short time after the first (Pease and Laycock, 1997; Polvi et al., 1991). These findings have been used to allocate police resources to prevent subsequent burglary. They have also lead to the development of theories of crime that emphasize the direct role of entering into the victimization state as the source of subsequent victimization (Sparks, 1981; Pease, 1998). This would occur presumably because the initial burglary provided the offender with information on available goods or means of access.

While the research and subsequent intervention programs are promising there are some reasons to be cautious about the use of repeated victimization as a tool for resource allocation and more so as a means of building criminological theory. With respect to the research on repeat burglary, the data sources used were highly selected subsets of the burglary population. Most of this work is based upon police records which tend to over represent repeaters because they

include a much more heavily victimized segment of the population. There is also some evidence that the victimization survey data used to support theories of repeat burglary overstate the amount of repeat victimization. These potential biases in the data used to explore repeat burglary victimization could result an over estimate of the predictive power of prior burglary on subsequent burglary. This is the central premise of repeat victimization as a tool for allocation and theory building.

The work on repeat assaults is much less developed than that for repeat burglary. There has been some work done on domestic violence and spouse abuse, but this work has not emphasized the repeat nature of the violence. They have focused more on the prevalence of the phenomenon than its persistence overtime. Those studies that have examined persistence have been conducted with highly self selected samples from police records of victim service agency records. Again, this raises questions about the overstating of repeat victimization and the central premise of repeat victim series and programs--that prior victimization is the best predictor of subsequent victimization.

Given the importance and promise of repeat victimization for allocation and theory building, it is important to confirm the assumption that prior victimization is the best predictor of subsequent victimization. The work described here attempts to do this by examining both repeat burglary victimization and repeat assault victimization with data from the National Crime Victimization Survey (NCVS). These data should be less selective than police records because they include events not reported to the police. The survey also employs a rotating panel design which affords some longitudinal data on persons and households. This survey will have a different error structure than the cross-sectional surveys used to examine repeat burglary

victimization. If the results of these analyses are consistent with those found with these other data and methods, then we can be more confident in the worth of repeat victimization for building theory and guiding allocation.

Describing the NCVS as a Source of Data on Repeat Victimization.

The data used in these analyses will be taken from the National Crime Victimization Survey. The NCVS employs a multi-stage cluster sample of 50,000 households and over 100,000 people. It is a rotating panel design. Housing units remain in sample for three years and occupants twelve years of age or older are interviewed at six month intervals for a total of seven interviews. The first interview is used for bounding purposes, not estimation, and subsequent interviews are used to produce annual estimates of the level and change in level of victimization in the non-institutionalized residential population of the U.S. Each respondent receives a personal screening interview which is designed to elicit events that may be crimes within the scope of the survey. One member of the household is designated the household respondent and they are administered a household screening interview that asks about crimes involving the theft of household property. If a positive answer is given to a question in the screener, then an incident form is completed that asks for details of the event including the date it occurred. The information on the incident form is used to exclude out-of-scope incidents and to classify incidents within the scope of the survey.

The analyses discussed below exploit some of the unique advantages of the NCVS for exploring repeat victimization. First, the NCVS collects data at six month intervals over as much as a three year period. This relatively short reference period should provide more accurate data

on victimization experience, while the fact that respondents can be in the survey for three years should offer a more complete view of a given person's victimization history. The cross-sectional surveys with long reference periods that have been used to explore repeat victimi.zation heretofore have none of these advantages. Second, the survey has repeated measurement of independent or predictor variables so that we can assess the effects change in various attributes on the risk of victimization. This is not possible with cross-sectional surveys and it seriously inhibits the ability to test arguments that prior victimization effects subsequent victimization. Without information on changes in other states or conditions, it is difficult to demonstrate that correlations between prior and subsequent victimization are not due to changes in these other states. Finally, the NCVS introduced a procedure in 1992 to collect information on high volume repeat victimizations reported in the survey. This procedure asks the respondent vvhether the repeated victimizations involve the same persons and places or whether the persons and places are different across events. This is a very different approach to exploring repeat victimization than has been used in the study of burglary victimization where inferences are drawn from correlations and not the direct statements of victims, per se. This alternative approach can provide valuable insights into why repeat victimization, and particularly repeat assaults, occur.

The following chapter presents the analysis of burglary risk using longitudinal data from the NCVS. The Chapter 3 contains a descriptive analysis of repeat assault using the data on series incidents from the survey from 1992 through 1995. This chapter supports earlier analyses that suggested that the vast majority of repeat assaults occur in three settings - at work, at school and between intimates. Chapter 4 compares victims of a single assault at work with victims of high volume repeat assault at work in order to identify factors that may explain why people are

assaulted repeatedly on the job. Chapter 5 does the same with single and repeat assaults involving intimate partners. The final chapter summarizes the findings presented in the chapters draws their implications for policy and for further research.

Lessons for Subsequent Uses **of the NCVS for Exploring Repeat Victimization**

In addition to the substantive findings presented in the following chapters;, this research identified a number of niethodological and logistical problems that complicate the use of the survey for exploring repeat victimization. We describe these problems here to assist others who may use the NCVS data longitudinally.

The first set of problems encountered had to do with the use of PROC MIXED in SAS to estimate random effects models. This procedure was not designed for use with files the size of the NCVS. The calculation of random effects requires a great deal of space and processing capacity. These space and processing requirements are exponentially related to the number of values in the random effects variable. Because there were 12,000 households in the burglary analysis, the random effect had I2.OOO values. The main frame computer that we were using could not provide the time and space required. The procedure would not run. Our first response was to seek more space. No matter how much space was allocated the procedure would not run. Our second response was to run the procedure on a subset of data from the file. This allowed the procedure to run but only on a subset of the data that was too small to be useful (n==1000 households). Our third response was to discuss this problem at length with the SAS staff who had written the procedure. They felt that the program would not run with a random effect variable with so many values. They suggested that we sort the file so that housing units were

nested within segments so that there would only be a few housing unit values within each segment rather than a string of 12,000 different values for the housing unit id. This approach allowed the PROC MIX to run within reasonable space and capacity limits.

The second problem occurred with the construction of the person-level longitudinal file. In order to do an analysis of repeated assault victimization, we wanted to build a longitudinal file of NCVS data at the person level rather than the household level as was done for the burglary analysis. It was also important that this file be built with post 1992 data to take advantage of the more complete reporting of the assaults in the new design. This design was fully phased-in in 1993 and therefore we chose to build a longitudinal file beginning with the panel ilnd rotation groups entering in the first collection quarter of 1993 and staying in sample until the fourth collection quarter of 1996. We were able to match three consecutive interviews but we were not able to match the third and fourth collection quarters of 1994 with those of the first and second quarter of that year. After a number of attempts and even attempts at hand matching, we contacted the Census Bureau. The NCVS branch said that it would be impossible to match the data from the first collection quarter of 1995 and aftenvards with data from prior periods due to the phase-in of the new sample. But, they said that there should be no problem matching the 1994 data with that from prior periods. Some time later the Bureau staff called back and said that they had found some problems with the match keys in 1994 and that they would get back to us when they knew what the trouble was. At that point we suspended work on this file and did some additional analyses of the series incident data on assaults. Census is working to resolve the problem.

Chapter Two

Modeling Repeated Burglary Victimization with the NCVS: Giving Content to Heterogeneity and State Dependenlce

Introduction

The fact that people differ with respect to their risk of victimization is not surprising and it has been the major source of evidence for empirical tests of theories of crime (Cohen and Felson, 1978; Hindelang et al., 1978). The fact that some people are repeatedly victimized has received much less attention in both criminological research and crime control policy (Sparks, 1981; Nelson. 1980: Reiss, 1984). A decade of pioneering work in England has reawakened interest in repeated victimization in the United States (Pease and Laycock, 1997; Farrell, 1995; Ellingworth et al., 1995; Osborn and Teseloni, 1996; Forrester et al., 1988; Polvi et al., 1991; Johnson et al.. 1997). This work suggests that prior victimization is a good predictor of subsequent victimization, a finding that has tremendous implications for the allocation of crime control resources.[1] Specifically. it suggests that greater than average resources should be focused on the recently victimized in order to prevent subsequent victimization. This finding is also intriguing in understanding the sources of repeated victimization. It suggests that there may be something about the first victimization itself that predisposes persons to subsequent victimization (Polvi et al., 1991; Forrester et al., 1988). Sparks (1981) has referred to this as "state

[1] The work of Sherman et al. (1989) also suggested that prior victimization in a place will indicate subsequent victimization in that place.

dependence". Once persons enter into the victimization state, future victimizatioris are more likely for them than they are for those not previously victimized, even when the overall rate of victimization is low.

These findings, however, are based in large part on police data and cross-sectional victimization data, data sources that have a number of limitations. Police data are highly selected since a large proportion of victimizations are never reported to the police (Skogan, 1976; Biderman and Reiss, 1967). Moreover. there is good reason to believe that victims who report to the police have much higher rates of victimization than those who do not (Biderman and Lynch, 1981). The volume and nature of repeated victimization may be quite a bit different in this highly selected population than among the general population of victims. The same can be the case with cross-sectional surveys, where recall bias can result in over-representation of repeat victims (Biderman and Lynch, 1981; Bushery, 1981).

This paper attempts to confirm and elaborate the work done in England by using data from the National Crime Victimization Surveys (NCVS). This survey follows housing units for three years, interviewing members of the household at six-month intervals. Consequently, it yields longitudinal data on households that should be less selective than police data and cross-sectional surveys. The survey should also facilitate explaining repeated victimization because it permits the inclusion of time-varying covariates other than victimization, such as change in household size. This allows a better test of the importance of state dependence than is possible with cross-sectional survey data.

What We Know About Repeat Victims

The research on repeat victims has addressed three questions. The first question was whether there is such a thing as repeat victims. Since there is a probability that a certain proportion of the population will be victimized in a given period of time, multiple victimizations during that period can occur simply by chance. One must fit a probability distribution to the data to determine whether there is repeated victimization (Nelson, 1980). The second question was what component of the cross-sectional crime rate is attributable to repeat victims as opposed to one-time victims (Trickett et al., 1992). This is important in order to establish realistic expectations for the contribution of repeat victim programs relative to other components of a crime control strategy. The third question was why certain people become repeat victims and others do not. This question is relevant for both criminological theory and for the targeting of crime prevention resources. Presumably, repeat victims differ in some personal behaviors or characteristics from non-victims and one-time victims. If this is the case, then understanding the ways in which repeat victims differ from others should tell us a great deal about the causes of victimization and we can, in principle, use this knowledge to target intervention. This third question is the focus of this research.

After having demonstrated that repeated victimization was not due to chance, Sparks (1981) suggested that it would be useful to separate possible explanations for repeated victimization into two classes—heterogeneity and state dependence. Heterogeneity refers to the fact that certain people have greater risk of repeated victimizations than others. This greater risk is due to relatively enduring attributes of the victims, such as gender or the kind of work they do.

State dependence refers to the idea that being a victim at time 1 may increase or decrease the chance of another victimization at time 2. Entering into the victim "state" carries a higher than normal risk that a similar victimization will occur in the following period. This could arise, for example, when in the course of one burglary the lock on the door is broken, thereby making it easier to be burgled a second time (Polvi et al., 1991). Similarly, the offender in the first burglary could share information about the contents of the house, which might prompt an acquaintance to enter the house the second time.

The empirical evidence to date is equivocal regarding the importance of heterogeneity and state dependence in predicting burglary risk and particularly risk of repeated burglary. Studies using police data have found a very strong effect of prior burglary on subsequent burglary risk (Pease and Laycock, 1996; Pease, 1998). These data, however, are a highly selected subset of the population of burglary victims and this selectivity could seriously increase the seeming importance of state dependence. The evidence from self-report surveys indicates both heterogeneity and state dependence are important for predicting victimization risk (Osborn and Tseloni, 1996). Much of this evidence, however, comes from cross-sectional surveys that are not particularly well suited to assessing the relative effects of heterogeneity and state dependence. In particular, since state dependence is an inherently longitudinal proposition, it is extremely difficult to assess properly using cross-sectional data.

Evidence from Police Data

People who report criminal victimization to the police are different from those who do not. Reporters are older than those who do not report: they are wealthier and they have more

education (Biderman and Lynch, 1991). Moreover, the events that are reported to the police are different in specific ways from those not reported to the police. The events reported involve more injury and more loss than those that are not (Skogan, 1976). This selectivity could easily affect estimates of the influence of state dependence. If, for example, professional burglars are more likely to hit wealthier victims than their more amateurish colleagues, and they are more likely to share information about the content of households that they have recently victimized, then their victims will be more likely to have repeated victimizations and they will be more likely to be in police records. In a less selected sample of victims than appears in police records, the effects of state dependence will be much less.

Conway and Lohr (1992) lend further support to this contention. They studied police reporting using longitudinal data from the NCVS and found that those who reported at time 1 were more likely to report at time 2. There is state dependence here, but it may relate more to learning how to make a police report than it does to entering the state of repeated victimization. If the risk of repeated victimization were the same across those who reported to the police and those who did not, then Conway and Lohr's finding would suggest that we should iind support for state dependence simply because of the fact that, given victimizations, those who report to the police once will be more likely to report subsequently.

Evidence from Cross-Sectional Surveys

Much of the evidence on repeated victimization and on the sources of repeated victimization comes from cross-sectional victimization surveys (Osborn and Tselorii, 1996; Osborn et al., 1992; Trickett et al., 1992). There is reason to believe that the specific surveys

used in these analyses may overstate both the amount of repeated victimization and the contribution of prior victimization to subsequent risk of victimization. Moreover, cross-sectional designs cannot provide good data on time-varying states (victim during a period) and covariates. This information is crucial to a good test of the effect of entering into the victim state upon subsequent Victimization.

Nelson (1980) analyzed the National Crime Survey City Surveys and found that the distribution of the number of victimizations did not fit a Poisson distribution that assumed independence of victimization events: There were more repeat victims than would have been predicted by chance. Similar findings were obtained using the British Crime Survey (BCS) (Trickett et al., 1992). Some questions have been raised, however, about whether both the City Surveys and the BCS overstate the number of repeat victims (Lynch and Titus, 1996). Because the BCS only collects incident forms on three incidents, analyses of repeated victimization used the number of screener "hits" as a measure of repeated victimization. Screener "hits" refer to the number of times that a respondent said "yes" to questions on the screening interview that are designed to elicit mention of possible victimization events. These "candidate events" are not classified as crime until they have been detailed using the incident form. It is quite possible that respondents could say "yes" to several questions in the screening interview, yet have only one victimization incident because the details of the various incidents reported do not meet criteria.

Both the BCS and the NCS City Surveys also employ a one-year, unbounded reference period. There is good evidence that these designs will include events from beyond the far bound of the reference period (Lynch and Titus, 1996; Neter and Waksberg, 1964). There is also evidence that the events telescoped into the reference period are different from those reported

within the reference period. Events reported to the police or otherwise "rehearsed" are more often telescoped into the reference period than are other events. We know that persons who report their victimizations to the police are more likely to have higher incident rates than those who do not (Biderman et dl., 1967). All of this suggests that cross-sectional designs that employ one-year, unbounded reference periods may have more repeat victims than do designs that employ shorter, bounded reference periods.

More important than the possible overestimation of repeated victimization in cross-sectional surveys is the limited ability of these designs to test the relative contributions of heterogeneity and state dependence. The concept of state dependence has been defined almost exclusively as the effects of entering into the state of victimization. Hence any correlation between victimization across time has been construed as support for the contention that prior burglary victimization per se leads to a higher than normal rate of subsequent victimization (Lauritsen and Davis-Quinnet, 1993; Osborn and Tseloni, 1996; Pease, 1998). Relatively fixed characteristics of respondents have been used to assess the effects of heterogeneity. The correlation of events across time that is not explained by heterogeneity is then construed as the effect of state dependence. This is unwarranted because time-varying states other than victimization status could account for the correlation between victimizations over time. Change in employment, change in marital status or change in residence, for example, coulcl be events that changed the risk of victimization, instead of or in addition to the prior victimization event per se. From this perspective, prior victimization is just one among several changing aspects of respondent experience that should be considered. It is important to introduce these other time-varying states into the analysis to provide a fairer test of the importance of state dependence in

explaining victimization risk. Cross-sectional surveys can provide useful data on relatively static characteristics of persons and places, but they are less able to give valid and reliable information on time-varying circumstances. This is the case because of memory decay and distortion with the passage of time. In view of this, cross-sectional surveys often do not ask for retrospective information on status that could vary over time and hence do not provide time-varying covariates necessary for a fair test of state dependence. Longitudinal designs offer more information on time-varying states and experience covariates and thereby offer a better test of state dependence explanations of victimization.

These specific features of the data used to investigate repeated victimization could account for the startling findings on the importance of prior victimization on the increased rate of occurrence of subsequent victimization. Police data may be so selective that they iidentify a highly victimized population that experiences repeated victimization at higher rates than the general population of victims. Cross-sectional surveys may have the same type of selectivity as police data but to a lesser degree. Both of these data sources have very little infomation on time-varying covariates other than the prior victimization state, such as employment, marital status, and residence. Information on these time-varying covariates is <u>essential</u> for testing the importance of entering into the victim state for subsequent victimization. It is important therefore to examine the issue of repeated victimization with data that does not have the same risk of overreporting repeated victimization and that includes time-varying covariates other than victimization.

The NCVS affords this opportunity. The survey is a rotating panel design of housing units interviewed at six-month intervals over three years. Because the NCVS employs shorter,

bounded reference periods, it is less likely to overestimate repeated victimizatioii. And, since residents of housing units are interviewed seven times, the NCVS can provide information on numerous time-varying states and experiences of the respondents that can be used to test the relative importance of state dependence in explaining repeated victimization. This is not to say that the NCVS is the perfect vehicle for exploring repeated victimization, but thal it employs a different design and has a different error structure than NCS city surveys or the BCS, and can, therefore, provide a very different perspective on repeated victimization. In fact, we find that introducing different predictor variables available in the NCVS leads to very different conclusions about the importance of repeated victimization, about our understanding of why it occurs, and about the merits of various policy options.

Using the NCVS to Study Repeat Victims

The NCVS employs a multi-stage cluster sample of 50,000 households and over 100,000 people. It is a rotating panel design. Housing units remain in sample for three years and occupants twelve years of age or older are interviewed at six-month intervals for a total of seven interviews. The first interview is used for bounding purposes, not estimation, and subsequent interviews are used to produce annual estimates of the level and change in level of victimization in the non-institutionalized residential population of the U.S.. Each respondent receives a personal screening interview which is designed to elicit events that may be crimes within the scope of the survey. One member of the household is designated the household respondent and he or she is administered a household screening interview that asks about

crimes involving the theft of household property. When a positive answer is given to a question in the screener, an incident form is completed that asks for details of the event, including the date it occurred. The information on the incident form is used to exclude out-of-scope incidents and to claisify incidents within the scope of the survey

The NCVS is a l~ngitudinal survey of housing units, but since housing units remain in sample for three years and seven interviews, the NCVS can also be used to obtain longitudinal data on households and people. The occupants of the sampled housing units can and do move, into and out of the units. Approximately 40 percent of the households in the sainple remain in the survey for all seven interviews. Since attrition is not random in the survey, any longitudinal use of these data must take into account the possibly biasing effects of attrition. This problem is discussed in greater detail below.

Longitudinal files of NCVS data can be built at the housing unit, household and person level by linking these units by combinations of control number, household number and person (line) number. Albert Reiss (1977, 1980) was the first to build and use a longitudinal file constructed in this manner. He encountered a large number of problems with matching of units across times in sample. He suspected that many of these problems were due to the fact that the file covered the period 1973 to 1975, the start-up period for the survey. The rotating panel design was not in balance and households were rotated in and out of sample at irregular intervals. It was difficult to determine which housing units should have been in sample for which periods and who should be matched with whom. During the NCS Redesign, Richard Roistacher used the control number method to construct a longitudinal file of households and persons for the period 1976 and 1978 when the design was in balance. Census Bureau staff

checked his matching results using the control card and found that his match rate using the control number was 95 percent. This suggest that the control number method of constructing longitudinal files is adequate.

A household-level longitudinal file was constructed with the public use tapes from the National Archive of Criminal Justice Data for the period 1980-1983. This period was used because during this period the public use tapes still contained the segment-level identifiers. Segments are the last unit of clustering in the sample design. Previous analyses of victimization risk have shown that the segment can be useful for understanding burglary victimization (Lynch and Cantor, 1992). Data from later in the pre-1986 period was not used to construct the file because sample size reductions which took place in 1983 complicate the construction of longitudinal files for the period 1983 to 1986. There is no good reason to believe that the precipitants and context of burglary have changed radically over time.[2]

Potential Contributions of the National Crime Victimization Survey to Understanding Repeated Victimization

The NCVS has several potential advantages for the examination of repeated burglary victimization. First, since it employs short, bounded reference periods, it should not overestimate repeated victimization to the extent that surveys with long, unbounded reference periods would. Second, housing units are followed for three years and this should increase our ability to identify repeat victims relative to cross-sectional surveys that ask people to recount their experience from the previous year. Third, the NCVS records time-varying states and

[2] The redesign of the NCVS has had less effect on the reporting of burglary thian it has for crimes like assault and rape (Persley, 1995; Lynch and Cantor, 1996).

experiences other than victimization. Fourth, a great deal is known about the error structure of the survey and variables can be entered into the analysis to take account of these known sources of error.

The Availability of Repeated Measures in the NCVS

The NCVS is a longitudinal design of housing units, not people or households. **The** housing unit remains in sample for seven interviews but the occupants of that unit can change throughout the time the unit is in sample. This mobility may be less consequential for burglary than it is for other crimes, such as assault, because respondents are still reporting on **the** experience of a particular housing unit, even though the respondent may change. For personal crimes like assault the respondent's referent is his or her own experience, and that will radically change when respondents change. As a result, with care we can use data from all six interviews in the NCVS for the investigation of burglary.

The Availability of Data on Status Changes in the NCVS

The NCVS collects a substantial amount of information on the attributes of housing units and their occupants at each interview, in addition to the information obtained from occupants about their victimization status. Many of these attributes can change from administration to administration and thereby constitute a change in status or circumstances. The variety of statuses that are included in the NCVS are summarized in Table 1. Many of these statuses will not be included in this analysis because they require more complex manipulation of the data than we have been able to accomplish to date. The following statuses will be included in our models: (a)

changes in the number of people in the household, (b) changes in the number of persons other than young children in the household, (c) changes in the household occupying the unit (new household moves into the unit).

Data on Important Sources of Heterogeneity in Burglary Risk

Prior studies of the prevalence and incidence of victimization have identified a number of specific "indicators of opportunity" that appear to predict the occurrence of burglary victimization. Many of these measures are relatively fixed and can therefore be considered sources of heterogeneity in risk rather than a more variable state. While the NCVS does not have a great deal of information that can be used to operationalize opportunity concepts, it included many of the attributes of housing units and persons that have been used in these earlier studies.

Limitations of the NCVS as a Means of Studying Repeat Victimization

Because the NCVS has been the subject of considerable methodological scrutiny , we have some knowledge of the error structure of the survey and how these sources of variability may affect our analyses (Biderman et al., 1986; Biderman and Lynch, 1991). In soine cases we are able to take account of these sources of error in our modeling, while in others we can only acknowledge them and speculate about their effects.

Residential Mobility. Bounding and the Reportinp of Victimization. Neter and Waksberg (1974) noted that in retrospective surveys respondents reported events that were out of the reference period as occurring in the reference period. They referred to this phenomenon as

telesco~ing. In an effort to reduce the effects of telescoping, they advocated bounding the far end of the reference period with *a prior* interview. This practice provided both a cognitive bound and a mechanical bound for the reference period. Respondents had an event to refer to in searching their memories (the initial interview), so that they could determine whether the target event occurred before or during the reference period. In addition, interviewers could refer to events reported in the prior interview to identify events that were reported earlier and exclude these twice-reported events. This type of procedure is employed in the NCVS. The first interview serves as a bounding interview for the second interview and the data provided in that interview is not used for estimation purposes. Every interview serves as the far bound of every subsequent interview.

Unfortunately this bounding procedure was not uniformly applied to the total NCVS sample. Households that move into housing units that are in the NCVS sample will be not have a bound for their first interview, but they will have prior interviews for all subsequent interviews conducted in that housing unit. Since unbounded interviews produce substantially more reports of victimization than do bounded interviews, it is important to take bounding into account in our analyses. This is easily done because households that move into sample units are identified in the data. The problem for this analysis is that boundedness is perfectly inversely correlated with residential mobility (in-movers). This complicates the interpretation of the effect of moving. Moving can have a real effect on the risk of burglary victimization (fewer security precautions owing to unfamiliarity with new residence and its threats, for example) or in-movers can have higher reported rates of victimization simply because they are initially unbounded in the NCVS.

It is difficult to sort out these interpretations with the data from the NCVS. Nonetheless, it is important to keep mover status in the model to control for both of these possible effects (which compete with victim state to explain subsequent burglary events).

Time-In-Sample Bias. Researchers have observed in surveys with longitudinal or panel designs that respondent "productivity" (frequency of reporting key events) decreases with the time that a unit is in sample or with the number of interviews. In the case of the NCVS, respondents report less victimization the longer they are in sample. This phenomenon has been referred to as time-in-sample bias. It has been observed at the level of rotation group, as well as at the individual respondent level (Biderman and Cantor, 1984). It is not clear what the source of this error is, or even whether it is an error. Some have attributed the decrease in reporting over time to fatigue, whereby respondents become increasingly less willing to perform the cognitive task demanded in the interview. Others have attributed it to education, whereby respondents become trained over time to more assiduously perform the cognitive task and to exclude irrelevant or marginal events. Whatever the interpretation of this empirical regularity, it is wise to take it into account in modeling victimization. Consequently, we included a variable in our analysis which indicated the number of times that a unit had been in sample.

Series Incidents. Series incidents refer to victimization events in which there are multiple occurrences of essentially the same type of crime but which the respondent cannot distinguish or date. When this occurs the NCVS interviewer can treat these events as a series incident, in which case an incident form is completed on the most recent of these events and the number of events in the series is recorded. The Bureau of Justice Statistics (BJS) does not include series incidents in their annual estimates of victimization. They exclude series because they do not regard all of

the events in the series as sufficiently similar to the most recent event to use them in annual estimates. This is especially the case for the dating of these events, which is crucial for the production of annual estimates.

Decisions about the appropriateness of including series victimization as repeated victimization can be very consequential for assessing the contribution of repeat victims to the cross-sectional crime rate. This is so because by definition series victimizations include a large number of incidents and can, therefore, contribute a great deal to the cross-sectional crime rates. One series victimization with ten incidents will obviously contribute Substantially more to the crime rate than a single incident or a repeat victim with only two incidents. Analyses that have included series incidents in the cross-sectional crime rates have shown that doing so substantially increases the cross-sectional crime rates and the instability of the crime trends (Biderman and Lynch, 1991). This raises the suspicion that series incidents in a large part may be an artifact of Census Bureau procedures. More specifically, multiple events may be treated as a series event when the respondent can clearly recall and report on these incidents, simply because it is easier for the interviewer to complete a single incident form, as opposed to multiple incident forms. To the extent that this is the case, it is a mistake to exclude series incidents from the cross-sectional crime rate and from the calculation of the contribution of repeat victims to the cross-sectional crime rate.

It is less clear how the inclusion or exclusion of series incidents would affect the modeling of the risk of repeated victimization. For purposes of this analysis, series victimizations are included as the total number of incidents reported by the respondent. This was done because repeat victims are a very small proportion of persons reporting victimization in any

given wave of the NCVS. As a result. excluding them can have a large effect on the number and perhaps, the distribution of repeated victimizations. Moreover, we can be more confident that respondents have had multiple victimizations when they report a series event than we can be that the dating of these events is accurate (Dodge, 1984; 1987). Since our purpose was not to produce annual estimates of the level of crime, we believe that it would be less accurate to exclude series events than to include them.

The specific measures of respondent heterogeneity, changes over time (respondent covariates including victimization in successive periods) and measurement error included in the models are described in Table 1.

Analytical Methods

Addressing the question, "Why do some people become repeat victims and others not?" calls for serious analytical innovation. The reasons for this are twofold. First, the NCVS represents a new and potentially very rich source of information about repeated victimization owing to its longitudinal nature. Second, new statistical techniques and software appropriate for the kind of data provided by the NCVS have recently been developed. The present study is the first to take full advantage of the both the longitudinal character of the data and new analytic strategies to investigate repeated victimization.

Lauritson and Davis Quinnet (1995) conducted a longitudinal analysis of repeated victimization of adolescents and young adults based on the first five waves of the National Youth Survey (NYS). Their analysis dealt with two distinct contributors to repeated victimization,

namely, heterogeneity and state dependence. The effect of heterogeneity on the victimization outcome has been measured by the intraclass correlation coefficient (denoted "rho"), which varies between -1 and +1. Usually rho for individuals will be positive in longitudinal studies because individuals tend to be like themselves from one time to the next; thus, the closer rho is to ' 1 , the greater the force of an "iqdividual differences" or heterogeneity account of repeated victimization.

State dependence refers to the notion that the prior conditions ("states") of persons may affect their likelihood of being in one of several possible current states. However, as used by Sparks (I981) and in subsequent literature, the only state referred to was that of crime yictim, and thus state dependence acquired a very restricted usage that refers to *one* state only: victim state. For example, if a person were victimized in a previous period, he or she might be more likely to be victimized again in the current period, as compared with a randomly chosen person from the same population. The probability of persons being in states "Not a Victim" or "Victim" in the current period is thought to depend on which of these states obtained in the previous period (i.e., it is a conditional probability). Note that state dependence is concerned with person-features that can vary dramatically over time (e.g., from "Not a Victim" in one period to "Victim" in the next period); that is, state dependence is concerned with person features that are heterogeneous over time. In a panel regression context, the degree of state dependence is assessed by examining the coefficient for the indicator of the previous state. The greater the departure of this coefficient from zero, the higher the degree of state dependence. We have stressed that this should be a partial regression coefficient in which the effects of other time-varying covariates related to victimization should be controlled.

Following the strategy developed by Nagin and Paternoster (1991), Lauritson and Davis Quinnet (1995) simultaneously examined both heterogeneity and state dependence in a model of repeated victimization. Owing to the longitudinal design of the NYS (they used five waves of reinterviews), the statistical model included terms that represented both individual respondents (with subscript i) and the time order of responses (with subscript t). In other words, for a given variable the data consisted of a table whose rows represented individual respondents and whose columns represented the times or waves of interview (a so-called "multivariate" or repeated measures data layout). In this layout of the dependent variable (Y_{it}), patterned diffierences among the rows correspond to heterogeneity among persons, whereas patterned differences among the columns correspond to heterogeneity among times (interviews). The statistical model employed for these data allowed for the self-similarity of one person's responses over time by including a random term to represent the unchanging uniqueness of an individual (U_{it}); the whole set of such individual "uniquenesses" was thought of as arising from the random selection of the observed sample respondents from the population of eligible persons. Thus, a given person's response at a given time, Y_{it} contains two random terms, the familiar "error" or "disturbance" teim that reflects the perturbation of an observation by unmodelled influences, denoted e_{ip} plus the tjerm for unique and stable but unmodelled qualities that may characterize a person over time, denoted u, (without a t subscript, implying that u, is the same at every time, or $u_{it} = u_i$ for every t). The latter term is called the <u>random effect</u> for each person in the panel, which gives rise to the name of this model in the econometric literature, the "random effects panel model" (in the experimentall design literature, this is a subjects-by-trials randomized block model, where persons or "subjects" repeatedly observed form the blocks; Kirk, 1995). Both disturbances and random effects are

usually assumed to follow (independent) normal distributions.

An immediate consequence of the data model just described is that the total unexplained variability in responses arises from two sources, the usual random disturbance of observations e_{rr}, or "error," plus the random variability among persons who remain like themselves; over time $u_{,}$, or "heterogeneity." The statistic rho mentioned earlier is just the proportion of total variability that is due to heterogeneity; 1-rho is the proportion of variability among persons fhat is due to unexplained sources. Notice that both sources of variability stand for ignorance: we admit that we really do not know what influences comprise the random disturbance terms and we admit that we really do not know what qualities comprise the random effects that make one person consistently different from another. Notice too that owing to the inclusion of the random effect term u, in every observation, a correlation, invariably positive, is induced among olbservations on the same person at different times: observations of a given person at different times (interviews) are not independent. In the model under consideration this correlation is assumed to be constant and in particular does not vary with the interval between waves: successive observations have the same correlation as observations with far greater separation in time (compound symmetry or equicorrelation model). Should examination of the data reveal a departure from this pattern, then an explicitly time-based scheme for disturbances would be considered, such as a first-order autoregression; the random effect setup would be left intact.

To complete the introduction to the Lauritson and Davis Quinnet model, the explanatory variables, or "covariates," should be mentioned. These variables, generally denoted X_{it}, are regarded as fixed, known quantities (without random elements). As a general matter, such covariates may be of two types. Constant covariates are those that do not vary across

observation occasions (that is, $X_{it} = X_i$) respondent's race is a typical constant covariate. Time-varying covariates capture changing features of the respondent or the respondent's circumstances; for example, a respondent's income might drop sharply at the beginning of a spell of unemployment, then partially recover as substitute work was found, so that a series of income reports would vary with these events. The various elements of the model may now be assembled in equation form for reference:

$$Y_{it} = \alpha + X_{it}B + u_i + e_{it} \qquad (1)$$

The foregoing model equation does not include a term to represent state dependence, i.e., the effect of prior state on the current state. The state dependence model includes the prior state as an explanatory variable, so that the equation becomes

$$Y_{it} = \alpha + X_{it}B + \gamma Y_{i,t-1} + u_i + e_{it} \qquad (2)$$

where $Y_{i\ t-1}$ is the prior state (lagged dependent variable) and y is the corresponding coefficient. When the coefficient y corresponding to prior state is significantly different from zero, prediction of repeated victimization is improved, and as a result the proportion of variance assignable to heterogeneity drops; the difference in rho for equations (1) and (2) is the proportion of total variance in victimization assignable to time-varying covariates, including victim-state dependence. In this way, both the proportion of variance assignable to heterogeneity (rho for the

second equation) and the proportion of variance assignable to time-varying covariates (difference between rhos for the two equations) can be established. There are a great many more technical details associated with appropriate estimation procedures for this model, discussed by Laurjtsen and Davis Quinnet (1995, pp. 153-160), which largely must be passed over here.

One of the analytical problems only partially dealt with by Lauritsen and Davis Quinnet is the nature of the dependent variable. It is a count of events and thus should usually be treated as a Poisson random variable. However, at the time of their work, only repeated rneasures statistical procedures based on the normal distribution model were available. Thus, they applied the recommended variance stabilizing transformation for a Poisson dependent variable, the natural logarithm. In the interim a more flexible and powerful statistical procedure has become available for exactly this problem, the generalized linear mixed model (GLMM). The term "generalized" designates nonlinear models of the type propounded by McCullagh and Nelder (1989), one of which is specifically for the Poisson; the term "mixed" refers to a model that contains both fixed and random effects, such as that for the repeated measures situation we have been discussing. (Note that Lauritsen and Davis Quinnet described their model as "generalized," but in fact they estimated a "general" linear mixed model without a link function using generalized least squares, and even though they did apply a variance stabilizing transformation that is similar in spirit to the Poisson link function, their GLS estimation method is not in general equivalent to the likelihood-based estimation used for McCullagh and Nelder models. The last sentence demonstrates why certain technical details have been passed over.)

The model we propose to apply in the future is the generalized linear mixed model

appropriate for Poisson-distributed data such as a count of victimization events uithin a fixed period. Using the previous notation, our model is

$$Y_{it}=H(\alpha+X_{it}B+u_i) \qquad (1')$$

or when the prior state is included as a predictor,

$$Y_{it} = \mathrm{H}(\alpha + X_{it}B + \gamma Y_{i,t-1} + u_i), \qquad (2')$$

where H() stands for the Poisson link. Models of this type can now be estimated using a SAS macro called GLIMMIX which is distributed on the SAS web page (the breakthrough is the inclusion of random effects inside the nonlinear link function).

One of the specific features of the NCVS is its use of housing unit as the unit of sampling and tracking. This means that housing units are nested within segments in a hierarchical fashion, and further, that individual respondents are associated with housing units in a (possibly) transient fashion. The statistical problems that these features raise are increasingly treated through inclusion of (additional) custom-crafted random effect terms in general and generalized linear mixed models.

Results

The models were estimated beginning with the full model that included all the fixed covariates, all of the time-varying covariates and states, the lagged burglary variable, variables indicating procedural non-uniformities and random effects for household and se,gment. Variables were excluded from the models to determine the best-fit model. Once this model was found, variables were excluded from this model to assess their effects on the overall fit of the model as well as the effects on the covariance due to heterogeneity. (These analyses employed the SAS MIXED procedure, rather than the advanced GLIMMIX procedure. The best fitting model includes the fixed effects of age, race, number of persons in household, marital status of household head, location in SMSA, housing structure, a random effect for housing unit, the lagged burglary variable, the percent change in the size of the household, the time-in-sample variable and whether the household was unbounded (See Table 2). Following a procedure used by Madalla (1983) in equation 2.4, page 39, we computed a psuedo-R squared of .14 for this model. It predicts 14 percent of the variance in burglary victimization. This estimate of the proportion of explained variance is conservative because it does not account for the boundedness of the likelihood for a (0,l) dependent variable. Madalla (1983), eq.2.5, p.age 40) suggests another method for this case. When this method is used the psuedo-R. squared becomes .42. While the former estimate may be low, this latter estimate is suspiciously high. In either case the model predicts burglary victimization reasonably well.

The lagged burglary variable has a significant positive effect on the risk of subsequent burglary victimization, demonstrating victim-state dependence. Prior burglary victimization does predispose the housing unit to subsequent burglary, even when sources of heterogeneity

in risk and some other time-varying covariates are held constant. The effects of specific sources of heterogeneity on burglary victimization are substantially greater than the effects of prior victimization. This finding is consistent with the work of Lauritsen and 1)avis-Quinnet (1995), which is the only analysis of victimization to date that has used longitudinal data. Virtually all of the relatively fixed characteristics of housing units and households (sources of heterogeneity) are related to burglary risk in the manner predicted by opportunity theory and previous work. Housing units in the central city of SMSAs have higher risk of burglary than units elsewhere. Units in multi-unit dwellings are at lower risk than single-family units. Households headed by older as opposed to younger persons have lower rates of burglary; those headed by married persons have lower risk than those headed by unmarried persons; large households are at greater risk than smaller households; householdls with lower income were at greater risk of burglary than those with higher income..

Race of the household head was the only fixed attribute of households examined that was not significantly related to burglary risk. Race did become significant, however, when both central city residence and the marital status of the household head were removed from the model. When this was done households headed by blacks were more likely to be burglarized that those headed by non-blacks. This suggests that the higher risk of burglary for blacks is related to the reduced guardianship that comes with being unmarried and to this greater proximity to offenders in central cities.

Changes in household structure were significantly related to risk of burglary. This was the case for increases and decreases in household size. This suggests that the sheer number of persons in a household may not afford guardianship. Rather the stability of the household.

composition over time may be more important. Changes in composition may require adjustments in schedules to afford guardianship in the household or they may make ownership ambiguous, such that household members do not know or respect what belongs to whom. Similarly, changes in composition can introduce new friend and associate networks to the unit, thereby disseminating information to non-residents that could lead to burglary.

Since many of the most dramatic changes in household size occur when a household moves out of a sampled housing unit and another household moves in, change in household composition can be related to other factors attendant to moving that affect risk, as well as to bounding status, which can affect reporting in the survey. These real and artifactual sources of increases in the reporting of burglary should be taken into account in the model through the inclusion of the variable MOVER. This variable is coded one when the household in sample in the current period is the same household that was in sample in the previous period. This variable should take account of the fact that moving can increase the risk of victimization and of the fact that in-moving households are unbounded and hence will report higher rates of victimization because of the telescoping in of events outside of the reference peiriod. Indeed, in-moving households do have a higher rate of burglary than other households in this model. The effects of changes in household composition, discussed above, are net of moving status and bounding status, however.

Conclusions

This analysis explored the effects of prior burglary on risk of subsequent burglary.

Work done using police data and cross-sectional victimization surveys suggested that prior victimization was a strong predictor of subsequent burglary and that such knowledge could be used to target enforcement and prevention resources. Moreover, this relationship between prior and subsequent burglary victimization was construed as support for explariations of subsequent victimization that emphasized victim-state dependence. That is, the prior burglary, per se, exposed the household to subsequent victimizations. The error properties of both police data and cross-sectional survey data are such that they could resulted in overestimates of repeat victimization and overestimates of the effect of prior victimization on subsequent victimization. This analysis re-examined these findings with data from the National Crime Victimization Survey that has a very different error structure than police data or data from cross-sectional surveys. The major purpose was to see whether prior victimization was an important predictor of subsequent burglary victimization.

The results of this analysis provide some support for the association between prior and subsequent victimization. Having a prior burglary event does predispose a housing unit to subsequent burglaries, even when other attributes of the unit including other time-varying states are held constant.

On the other hand, there are many other attributes of housing units that are better predictors of burglary victimization than prior victimization and which would be a much better basis for the allocation of prevention and enforcement resources. Targeting prevention programs and police patrols toward areas with young persons who are unmarried and that experience frequent or dramatic changes in household composition, for example, would probably be a better allocation strategy than the targeting of prior victims.

While previous empirical work on repeated victimization may have overstated the importance of prior burglary victimization for subsequent risk of burglary, there are some peculiarities of this analysis that may be operating in the opposite direction. Specifically, this analysis examines the effect of the number of burglaries reported during a previous period on the number of burglaries occurring during a current period. Since the NCVS employs a six-month reference period, we are really comparing one six months' worth of experience to a subsequent six months' worth. There is some evidence from police data that repeated burglaries happen in a very short interval after the first incident (Polvi et al., 1991). If this is the case, then the use of a six-month reference period may be too long to detect the full effect of prior victimization (victim-state dependence). A housing unit may experience two burglaries in one six-month period and no burglaries in the next six months. The effect of the lagged burglary variable in this case would be negative, even though the unit experienced repeated burglary victimization. In an analysis such as the one described here, one would need to compare one month of experience, for example, to the subsequent month of experience in order to see whether the lagged burglary variable influences burglary within the current six-month period. Analyses using shorter periods of experience, such as a month, can be conducted with the NCVS, but they will require complex programming to restructure the public use files.

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Table 1: Variables Included in the Model of Burglary Risk by Opportunity Theory Concept

Concept	Variable Name	**Variable Description**
RELATIVELY FIXED CHARACTERISTICS		
Exposure (Age)	Age of Household head	Age in years of the person designated as the household head.
Proximity (SMSA)	Location of Housing Unit	Whether housing unit in the central city of an SMSA. 0 = not in central city 1 = in central city
Proximity (Black)	Race	1 = Black O= Not black
Exposure (Multi)	Housing unit structure	1 = single family home 2 = two or three family home 3 = multi-unit dwelling 4=mobile home or trailer
Guardianship (Married)	Marital Status	0 = single, widowed, separated, divorced 1 = married
Attractiveness (Income)	Income	Household inco:me in ordered categories from 1 to 14
Guardianship (HH12)	Number of Persons in Household	1 = 1 person 12 or older 2 = 2 persons 12 or older etc.
TIME-VARYING COVARIATES		
Guardianship (HH12CHNG)	Subtraction of persons from household	Negative percentage change in the number of persons in the household fr'om tl to t2.
Guardianship (HH12CHPL)	Addition of persons to household	Positive percentage change in the number of persons in the household from tl to t + l.

Victimization (BURGLAG)	Number of burglary incidents	number of burglary incidents at t
MEASUREMENT **ERRORS**		
Guardianship/Exposure Bounding effects (Mover)	Residential Mobility	0 = same house:hold as last interview 1=new household
Response error (Wave)	Time-in-sample	1 = second time interviewed 2 = third time interviewed 3 = fourth time interviewed 4 = fifth time interviewed 5 = sixth time interviewed 6 = seventh time interviewed
Random Effect	Housing Unit	housing unit number

Table 2: Model of Burglary Victimization

Variables	Model
RELATIVELY FIXED CHARACTER -1STICS	
Age of Household head	-.00076*** (-9.91)
Location of Housing Unit	-.O194*** (-6.72)
Race	.0027 (.06)
Housing unit structure	
SFH	.00145 (.25)
2-3FH	-.01214 (-1.76)
3 + FH	-.01796** (-2.84)
Marital Status	.02752** (8.61)
Income	-.000796* (-2.10)
Number of Persons in Household	.00539*** (4.08)
TIME-VARYING STATES	
Subtraction of persons from household	-.04762** (-3.76)
Addition of persons to household	.02668*** (4.37)
Number of burglary incidents	.00074*** (4.68)
MEASUREMENT	

ERRORS	
Residential Mobility	-1.039 (-7.12)
Time-in-sample	
First	.0135** (3.15)
Second	.0156** (3.62)
Third	.0135** (3.15)
Fourth	.0082* (1.92)
Fifth	.0019 (.44)
Housing Unit Random Effect	
REML Log Likelihood	-6270.46
AIC	-6273.46
SBC	-6286.67

Chapter Three

Understanding High Volume Repeat Assaults

Introduction

Criminal victimization is a rare event. In a given year only about 13 percent of the population twelve years of age and older report becoming victims of common law crimes (Dorsey and Robinson, 1997). Being victimized more than once in a given time period, say a year, is rarer still. Much less than one percent of the population twelve and older claim to be victims of repeat victimization. The number of persons who are victimized many times, e.g. six or more, in a given unit of time are very rare indeed.[3] While these high volume repeat offenders are rare they are interesting for a variety of reasons. First, because their victimization is so much more frequent than that of non-victims and one-time victims, the factors that contribute to victimization risk may be more pronounced in this group than in other groups with less intense victimization experience (Sparks, 1981). This should help identify patterns in social structural variables and situations associated with victimization and thereby facilitate the building of theories of crime risk. Second. these high volume repeat offenders contribute a great deal to the cross-sectional crime rates (Trickett et al., 1992; Hope, 1994). If this is the case, identifying these individuals and understanding the source of their extreme vulnerability can help in the targeting and structuring of intervention programs.

[3] Some multiple victims experience many different crime types - they are robbed on one occasion and raped on another. However, most repeat victims experience the same type of violence for each incident (Reiss, 1980). This is the defining characteristic of a series victim in this paper.

While the study of high volume repeat victims may be beneficial, it is complicated by some conceptual and methodological problems. Specifically, police data on repeat victimization is highly selective both in terms of who calls the police and in terms of what types of events are the subject of calls (Skogan, 1976; Conway and Lohr, 1993; Biderman and Lynlch, 1981). This selectivity can offer a very misleading picture of the extent and nature of high volume repeat victimization. Self-report victim surveys are less selective'in terms of who and what is reported, but burden and cost limitations in these surveys lessens their ability to provide detailed and precise information on high volume repeat victims (Dodge, 1984; 1987).

In 1992, the National Crime Victimization Survey (NCVS) revised their procedures for handling high volume repeat victimization. Prior to 1992, if a respondent reported thee or more incidents in a six month period, interviewers could treat them as a "series incident" and simply note the number of events and collect detailed information on the most recent of these events only. After 1992, treatment as a series required six or more incidents. These changes made it more difficult for interviewers to treat these events as a "series incident." Moreover, information was collected on the high volume events that were treated as a "series incident" and those that were not so that the selectivity or interviewer discretion in determining which events could be treated summarily was apparent. Both of these changes should make series incidents more representative of high volume repeat victimization. Finally, some information was collected on the relationship among incidents included in a series incident. This information along with the data routinely collected in the NCVS can provide a much more detailed picture of the volume and nature of high volume repeat victimization than has been available heretofore from the NCVS. Moreover, they can, to some degree, let us establish linkages across individual events in a series of repeat victimization.

The first of the follou.ing sections describes the series incident procedure employed in the NCVS traditionally and the specific changes introduced in 1992. In the second section we review some of 'the work done on high volume repeat victimization using the NCVS. These earlier studies characterized high volume repeat victimization as routinized, low intensity conflict centering around work and domestic disputes. In the third section, we revisit these characterizations of high volume repeat victimization and test their accuracy by using the newly available data on these events. In the fourth and final section we suggest what more might be done to understand why these events occur and why they persist.

Accommodating High Volume Victims in Self-report Surveys

Respondents with a large number of repeat victimizations pose a problem for self-report surveys in that reporting the details of a large number of events is burdensome for interviewers and respondents. This burden can adversely affect response rates and general participation in surveys. Moreover, the quality and utility of information on high volume events is often called into question. Some portion of high volume victimizations are subject to "flabby" recounting, as in the case where a policeman reports being assaulted "everyday". If the interviewer is diligent this results in a report of 180 assaults in a six month period. This number is not very precise. It could be 30 or 8 or 100. It is a chore to recount in great detail a large number of similar events. Accommodating high volume victim, then, requires that the burden be reduced and that good and detailed information can be distinguished from approximations.

Self-report surveys of victimization have taken various approaches to obtaining high quality information on the experience of high volume victims without unduly buirdening them. The most common solution is to ask very detailed information on only a subset of the events reported by high volume repeat victims. The British Crime Survey (BCS), for example, only

collects detailed incident information on the first three incidents mentioned. The National Crime Victimization Survey (NCVS) employs a "series incident" procedure. Interviewers are free to treat high volume victimizations as a series incident if 1) there are six or more incidents reported, 2) these incidents are similar in their specifics and 3) the respondents cannot provide details on all of the events. If these conditions are satisfied, then the interviewer records the number of events in the series and obtains the specifics on the latest event in the series.

These various approaches to the handling of high volume victims trade-off the detail and quality of information for reductions in burden. For the purpose of producing annual estimates of level and change, this may not be a bad trade-off. Series incidents are relatively rare in the NCVS and there is no a priori reason to believe that the contribution of series crimes to annual estimates should vary dramatically over time. Hence change estimates may be particularly immune from whatever error the series procedure introduces.

More recently, however, repeat victims seem to be attracting more attention as objects of study and as a focus of policy (Pease, 1998). Trickett and his colleagues found that more than forty percent of the difference in areal crime rates in England were due to repeat victims (Trickett et al., 1992). These findings raise the possibility that substantially reductions in crime could occur by understanding and targeting repeat and high volume victims. This possibility made series procedures and other accommodations used in self report surveys less acceptable.

The study of high volume victims requires detailed and reliable information on each and every victimization events that befall these victims. Series incident methods and other procedures give up much of that information. They can also confound interviewer discretion with real differences in the nature of events. It is unclear, for example, whether the reporting of an event as a series incident is done to reduce the burden on the respondent or to make the interviewer's life simpler. Finally, none of these summary procedures used (and for that matter,

the interviewing procedures more generally) make provision for associating events when more than one crime is reported in a given interview. Sparks (1981) suggested that entering into the state of victimization at time one will predispose you to victimization at time two. This can occur, for example, when a burglary at time one leaves you with a broken lock **that** facilitates entry at time two. Others have seized upon this notion of "state dependence" (Lauritsen and Davis Quinnet, 1995; Pease, 1998). This scenario assumes that the events share **the** same place; that entry involved the same door; and other interconnections between events in the series of repeat victimization. These commonalities across events are crucial to our understanding of why victimization occurs repeatedly to the same victim. Respondents in virtually all major data sources are never asked outright about the association of one event with another.

This paper takes advantage of the changes in the procedures in the NCVS to shed some new light on high volume victims. As we noted earlier, in 1992 the NCVS changed their requirements for using the series incident procedure. These changes 1) increased the number of events required from three to six, 2) explicitly asked respondents whether the events were similar and whether they could describe them individually, 3) asked respondents whether these events were committed by the same offender or occurred in the same place, and 4) asked whether the conditions that precipitated these events were still going on. These changes permit the disentangling of the use of interviewer discretion from differences in the nature of events. We are able to tell for the first time how the high volume events reported as a series differ from those that were reported on individually. This, in turn, pennits the use of the series data with some sense of how representative it is of the experience of high volume victims.

The data resulting from these new procedures will also allow us to determine whether events in a series share common persons or events and thereby provide a better idea of why these

repeat victimizations occur. If, for example, we know that assaults on the job occur in the same place and involve the same people then we can infer 'that this may be a personal dispute among co-workers. This would be quite different from the assaults on the job that policemen encounter wherein the people and places involved can vary from event to event. This largely descriptive and typological approach to understanding repeat victimization extends the work done by Dodge (1987) and compliments the sophisticated modeling work that has been done more recently (Trickett et al., 1995a; Osborn and Tseloni, 1996). Finally, these new data make it possible to estimate the duration of repeat victimization which was not possible heretofore.

Series Incidents and the Universe of High Volume Repeat Victiimizations

Prior to 1992 it was not possible to determine what portion of the high volume repeat victimization reported in the NCVS was treated as series incidents and why. The rules for treating an incident as a series were in the interviewer's manual but they were vague and there was no way to check whether the interviewers were uniformly following these procedures. As a result, it was impossible to tell how much of the high volume repeat victimizations were reported as series events and how those high volume events reported as a series differed from those reported on individually. In 1992, questions pertaining to the eligibility of events for treatment as a series were added to the interview. What was formerly buried in the interviewer's manual was now explicitly asked of respondents. More importantly, this information is in the public use file so that users of these data can determine how many potentially eligible repeat events were accepted for treatment as a series and why those rejected were rejected.

Interviewers are instructed to ask the following questions:

4.0 Altogether how many times did this type of incident happen during the last six months?

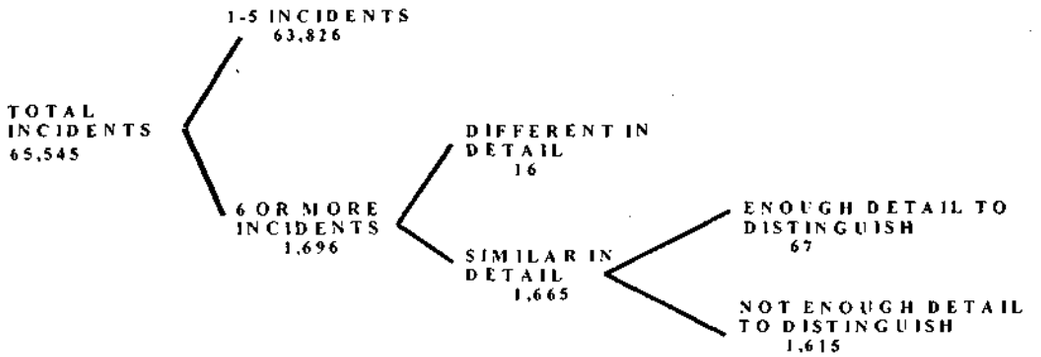
Chcck Item B
How many incidents?
1-5 incidents (not a series)
6 or more incidents (go to Check item C)

Check Item C
Are these incidents similar to each other in detail or are they for different types of crime?
(Ask if not sure)
Similar (fill in Check Item D)
Different (not a series)

Check Item D
Can you recall enough details of each incident to distinguish them from each other? (If not sure, ask.)
Yes (not a series)
No (is a series , ask Q5a.)

Only events that involve six incidents or more, for which the incidents are similar and the respondent cannot distinguish individual events are treated as a series. Throughout the period 1992 to 1995, 1,696 events involved six incidents or more (Figure 1). Of those **1,615** were treated as series incidents. So the vast majority of very high volume victims are included in the NCVS as series incidents. Under one percent are reported on as individual incidents. We know then that we can use the data on series incidents in the NCVS to say something about high volume victimization.

Figure 1. 1992-1995 NCVS Series Incidents Determination



Identifying Types of High Volume Repeat Victimizations

Dodge (1987) examined series incidents in the NCS largely for the purpose of determining if series incidents were due to interviewers' unwillingness to collect data on a large number of incidents from a given respondent. To do this he first identified respondents who had initially reported series incidents from January to June of 1985. He then re-interviewed these individuals using two separate surveys modified from the NCS. He broke the series crimes into those who report five or fewer incidents on re-interview and those with six or more incidents. Dodge found that for most series incidents of five events or less respondents could give detailed information on all of the incidents, if interviewers asked them to. This was not the case for the majority of incidents that included six or more victimizations. By restricting the use of the

series incident procedure to events with six or more crimes, Census removed much of the interviewer discretion that produced "False" series, i.e. events treated as a series not because they were high volume or not recalled in detail.

Dodge's work also suggested some explanations about why these high volume victimizations stay in dangerous situations. He found that the majority of series incidents were either disputes between acquaintances, work related or, to a lesser extent, occurred at school. Most of the work related victimizations involved police officers. Those occurring between acquaintances were seen primarily as domestic violence and those at school were regarded as bullying.

Dodge's work provides very useful insights into the source of high volume victimization. He provided a tentative answers to the question of why high volume victims stay in the situations that repeatedly place them at risk. The answer is powerful institutional arrangements. In the case of the police. for instance, their work role requires that they intervene in confrontations that the rest of us would avoid. The result is frequent assaults. Children under 16 must attend school so confrontations at school are difficult to avoid. To be sure, some persons in these institutional arrangements can avoid these situations, but participation in these institutions predispose one to the risk of high volume victimization. As long as the respondent stays in these institutional arrangements, the repeat victimization will continue.

Dodge's useful description and classification series incidents helps to sharpen our analytical focus. The clustering of high volume victimization in these activity domains suggests that our search for understanding should focus on these domains. What is it about policing or school that predisposes someone to repeated victimization? Are some participants or some

situations in those domains more predisposing than others?

Newly available data generated by the 1992 revisions to the NCVS (obtained largely as a result of Dodge's studies) allows us to extend his work and to begin to answer some of the questions raised in Dodge"s earlier papers (Dodge, 1984; 1987) Specifically, we are now able to 1) check the integrity of his typology to see if it conforms to the interpretations that he attributed to these classes, 2) to elaborate that typology, if necessary, and 3) to suggest factors that predispose participants in those institutional domains to high volume victimi;r,ation that can, be pursued in multi-variate analysis.

Dodge's typology was based upon a very small sample of series incidents in the NCVS. It also included a large number of relatively low volume victimizations, i.e. 3 to' 5, that Dodge showed to be treated as a series incidents largely for the convenience of the interviewer. This analysis includes a much larger sample of series incidents accumulated between 1992 and 1995. Also the new procedures in the NCVS define a much more homogeneous set of high volume victims than did the procedures in effect at the time of Dodge's work. Moreover, Dodge (1984) characterized series based upon an analysis of narrative descriptions provided by Census interviewers. These descriptions can vary substantially across interviewers. The procedure introduced in 1992 employed a set of structured questions to determine if the events in the series had all the attribute of, for example, domestic violence, i.e. that the offender was a spouse and the same person in every event in the series.

For purposes of this analysis we are focusing on assaults which are very prevalent among high volume repeat victims, much more prevalent than they are in victimization generally. Other types of serious crime such as burglary, rape and auto theft are relatively rare among reported

high volume repeat victimizations. Series incidents involving theft are prevalent but they are not addressed here because they include a large number of diminimus incidents and the victims have very little information about the offender and other particulars of the incidents. Table 1 presents the distribution of crime type for series incidents as reported prior to Dodge (3+ events), in Dodge's 1987 reclassification study (6+events) and in the current NCVS series determination procedure. The more restrictive definition of series vicrtimization used post 1992 in the NCVS results in an even greater representation of violence relative to theft in high volume repeat victimizations.

Table 1. Distribution of Crime Type for Series Incidents

Type of Crime	NCVS 1992-95 N=1615 Percent	Dodge 1987 N=72 Percent	Prior to Dodge N=I85 Percent
rapelsex aslt	1.8		
sex thrts	1.2		----__
robbery	3.5	4.2	3.2
aggrav. aslt	10.0	4.2	4.3
simple aslt	45.7	27.8	21.I
then	30.1	43.1	52.0
burglary	7.4	18.1	16.2
vehicle theft	.4	2.8	3.2
Total	100.0	100.0	100.00

The differences observed between the old and new procedures are due in a part to raising the requirements from 3 events to 6. This can be seen by comparing the middle column of Table 1 to the far right column. Still more of the difference may be due to the introduction of the new screening interview in the NCVS in 1992. This new screen substantially increased the reporting of crimes of violence generally and especially simple assault. The increases in reporting were less dramatic for theft, burglary, motor vehicle theft and robbery (Kindermann et al., 1997).

These effects are consistent with the differences observed between the NCVS column and Dodge's column in Table 3.

Revisiting Dodge's Typology

Based on his analysis of narratives Dodge (1987) identified three modal types of high volume victimization—-those that occur at work, those at school and domestic violence between acquaintances. From his field test of these events Dodge was satisfied that the occupational series victimizations were predominantly law enforcement officers, that events between acquaintances were largely domestic disputes and those at school were instances of bullying. We revisit Dodge's typology and use data from the new series incident procedures in the NCVS to find out more about the events in each type of series. For example, are series crimes at work mostly due to police encounters with citizens or to domestic disputes that simply occur on the job? On the basis of this information we may see patterns that suggest further elaboration of Dodge's typology.

Table 2. Alternative Classification of Violent Series Crimes, Dodge 1987

Classification	Cases	3 or more	6 or more
Line of Duty	23	44.4%	44.0%
Domestic Violence	10	19.4	19.9
School Violence	13	25.0	31.9
Other Violence	6	**11.6**	3.9

The first step revisiting Dodge's typology is to differentiate series incidenits by activity at the time of the incident. Dodge identified institutionalized activities of domains such as work, school, and home (implied by the term domestic) as the locus of most series incident assaults.

Now that the activity at the time of the victimization is reported in the survey, we can more systematically classify the full sample of series incidents for years 1992 though 1995. Table 2 presents the distribution of series incidents by institutional domain and number of incidents in the series as reported in Dodge's study. The distribution series incidents by activity at the time of the incident from the post-1992 NCVS is presented in Table 3. Although the two procedures and the classification are too different to permit detailed comparisons, the distributions of assaults across activity domains is similar in the two tables. The vast majority of high volume repeat victimization happens while the victim is at work, at school or at home. Almost 96 percent of the series incidents reported in Dodge's study occurred in these domains and approximately 77 percent in the current survey. There is then something about 'these institutional settings that encourage victims to stay and be victimized again.

The new NCVS procedures also permit the identification of leisure as another activity category or institutional setting. These incidents account for over 16% of the series assault incidents. We will examine these domains individually in the following sections. The big difference between Dodge's findings and those presented here is the substantially, lower proportion of school violence in this study. It is not clear why this occurs.

Table 3. Distribution of Assault Incidents by Activity

Activity	N=899 Percent
work	42.
school	12.1
leisure	16.4
home	23.0
other	5.2
don't know	.6
Total	100.0

High Volume Victimizations at Work

Dodge's work suggested that the bulk of repeat assaultive victimizations at work involved police officers. Since their job is to intervene in disputes and confrontations, it is not surprising that they are "hit, kicked or punched" or threatened on a regular basis;. This analysis seemed to answer the question of 'vhy, in the work setting, would some one tolerate repeated victimizations. The answer was that it is inherent in the work role. This answer was so plausible that other types of explanations were essentially dismissed. Personal vendettas by co-workers or disputes over work-related issues and even domestic disputes that spill over in to the work place were not considered. It is useful to revisit this type of high volume repeat victimization to see if most of the events contained therein conform to this characterization.

From the limited information on occupation contained in the post 1991 instrumentation, it does appear that a large portion of the assaults at work involve law enforcement related-occupations, i.e.. police officers or security personnel (Table 4). Approximately 44 percent of high volume assaults at work involve police officers and security personnel. A rather significant proportion of high volume victimizations, however, involve medical, mental health workers and teachers (20.3 percent taken together). It is plausible that these occupations also have order maintenance components that put them in harms way routinely. Rossi (1973) referred to these occupations as "dirty workers" because they dealt with socially marginal populations and had

Table 4. Distribution of Assault Incidents at Work by Profession

Occupation	N=383 Percent
medical	8.3
mental	5.2
teaching	6.8
law	44.3
retail	9.9
transportation	1.8
other	23.4
Total	99.7

some responsibility for controlling these populations. So an emergency room nurse will be hit by frightened or intoxicated patients on a daily basis. Less amenable to this type of explanation is the victimization of retail workers which accounts for just about 10 percent of high volume assaults at work. It is also somewhat surprising that transportation workers are involved in such a small proportion of high volume victimization at work (<2%). Prior work on the prevalence of victimization at work suggested that persons with work environments similar to those of transportation workers were at high risk of victimization on the job (Lynch, 1987; Collins, Cox and Langan, 1987). It is quite possible, however, that occupations can have high prevalence without being disproportionately the object of high volume repeat victimization.

To the extent that vulnerability to high volume victimization is due to the order maintenance role required in some occupations, we would expect that few events would involve the same persons. The stereotypic police encounter is brief and impersonal. The high volume is a result of their need to intervene in different disputes. Other explanations for the concentration of repeat victimization in law enforcement occupations are possible that have nothing to do with the order maintenance requirements of the role. For example, it is possible that the stress attendant to law enforcement jobs can promote assaults among co-workers on the job. It is also

possible that this same job-related stress can result in domestic violence among police officers that takes place on the job. In both of these scenarios the assaults are job-related due to stress on the job, but it is not a direct result of the order maintenance aspects of the job per se.

We would assume that if the high volume repeat assaults among police officers were due the order maintenance aspects of the job then the offender would be different across all of the events in the series. If these other explanations were more appropriate, then offenders should be the same across events. Moreover, the relationship between victims and offenders should also differ. If the assaults are the result of the occupational role, then the offenders should b'e strangers more often than not. If the offenders are co-workers or intimates then the order maintenance role not the source of the repeat assaults.

When we differentiate high volume assault victimizations at work by the occupation of the victim and whether the events involved the same offender, we see that those involving police officers almost never have the same offender (Table 5). Only 1 percent of high volume assault victimizations reported by law enforcement officers had the same offender in all of the events. In contrast, about 20 percent of the victimizations of persons in occupations other than policing had the same offender committing the acts. This supports the theory that high volume repeat victimization among police officers occurs because of the order maintenance requirements of the job. It also suggests that repeat assaults on the job in other occupations is due to the role and not personal vendettas. Finally these findings indicate that there is a substantial mino:rity of the high volume repeat assaults at work that may be personally motivated (or at least farniliar) and not simply the result of the occupational role.

Table 5. Assault Incidents by Occupation and Person/Place

Occupation	Offender Across Events		Total
	Other	Same Person	
dirty workers	68	10	/8
	87.2%	12.8%	100.0%
law	168	2	170
	98.8%	1.2%	100.0%
retail / transportation	38	7	45
	84.4%	15.6%	100.0%
other	65	25	90
	72.2%	27.8%	100.0%
Total	339	44	383
	88.5%	11.5%	100.0%

Chi-Square	Value	df	Asyrnp. Sig. (2-sided)
Pearson	42.131[a]	3	.000

[a]. 0 cells (.O%) have expected count less than 5. The minimum expected count is 5.17.

These data support Dodge's earlier classification of work related repeat victimization—the work role is a powerful force for keeping victims in high risk situations. These data also suggest that it may be useful to differentiate this class of events further into those that occur to law enforcement officers on the job, those that happen to "dirty workers" and those that occur in other occupations that do not have (to the same degree) an occupational role that requires social control activity. The law enforcement role most clearly fits the stereotype of risk just being part of the job, nothing personal. This seems to be less the case for "dirty workers" and even less the case for retail workers and other occupations. In these cases, it may be much more personal or, at least familiar, event. Subsequent efforts to understand why some role incumbents become subject to high volume repeat victimization and others do not should differentiate these sub-classes of occupations. Presumably, attributes of the occupational role would be more powerful determinants of repeat victimization for police officers than for the others identified here.

High Volume Victimizations at Home

High volume victimizations at home account for 21% of the series assault incidents (Table 3). Dodge's work suggested that the vast majority of high volume assaults incidents between acquaintances are spousal or partner assaults. In contrast to high volume victimization at work, these events would be highly personal where the offender is the same in all instances. The setting is usually the home and the offender and the victim are in some sort of relationship. In general. assaults between acquaintances conform to this image of domestic violence at home.

Approximately 86 percent of all high volume repeat assaults occurring in the home are committed by the same offender (Table 6). In only one case was the offender unknown to the victim. However, only 64 percent of these events involved spouses, ex-spouses or boyfriends (Table 7). In the remainder the offenders were somewhat evenly distributed among neighbors, parents, siblings and other relatives. There is a substantial amount of high volume repeat assaults at home that involve relatives who are not partners. This class of violence among non-strangers at home could be sub-divided further to distinguish partner assaults from other non-stranger events in the home.

It is not surprising that in repeat assaults between partners the offender tends to be the same across all events most of the time (93percent). It is unexpected that 86 percent of the series incidents involving persons known who are not intimates would also have the same offender across all events. One would have thought that several siblings may prey on another or in child abuse more than one adult would assault the child, but this does not appear to be the case. These assaults, like the partner assaults, involve the same victim and offender . Just over 66% of all home assault victims reported that they did not suffer any physical injury.

Further, when the victim did suffer injuries, partners or intimates were responsible almost 3 out of every four times (Table 8).[5]

Table 6. Home Assault Incidents by Acquaintance and Same Person

Offender Across Events	Victim / Offender Relationship		Total
	Other	Acquaintance	
other	***1***	22	22
	70.0%	11.2%	14.0%
same person	3	175	175
	30.0%	88.8%	86.0%
Total	10	197	207
	100.0%	100.0%	100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	2/.343[b]	1	000

[a]- Computed only for a 2x2 table

b- 1 cells (25.0%) have expected count less than 5. The minimum expected count is 1.40.

Table 7. Home Assault Incidents Involving Acquaintances

Offender Across Events	Victim / Offender Relationship		Total
	Other	Partner	
other	13	9	22
	13.7%	8.8%	11.2%
same person	82	93	175
	86.3%	91.2%	88.8%
Total	95	102	197
	100.0%	100.0%	100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	1.1/ID1	1	279

0 cells (.O%) have expected count less than 5. The minimum expected count is 10.61.

5. It is interesting to note that about one half of all high volume repeat victimizations involving partners result in injury, but these series events are excluded from BJS annual estimates of victimization rates.

Table 8. Home Assault Incidents by Partner and Injury

Injury	Victim / Offender Relationship		Total
	Other	Partner	
No Injury	85 61.6%	53 38.4%	138 100.0%
Injured	20 29.0%	49 71.0%	69 100.0%
Total	105 50.7%	102 49.3%	207 100.0%

Chi-Square	Value	df	Asymp. Sig. (2-sided)
Pearson	19.569[O]1	1	000

[b]. 0 cells (.O%) have expected count less than 5. The minimum expected count is 34.00.

Home assault cases are not reported a little over half of the time. The overwhelming reason given by the victim was that the incident was a private or personal matter (Table 9).

Table 9. Reason Assault Was Not Reported To Police by Activity

Reason for Not Reporting	Activity						Total
	Work	School	Leisure	Home	Other	don't know	
Kep lo uniciai	**35"** 30.6%	33 32.4%	*2* 2.0%	5 4.5%	1 3.2%		**97** 18.3%
Private Matter	27 14.8%	13 12.7%	23 23.0%	38 33.9%	9 29.0%		110 20.8%
Minor /Kid Stuff	18 9.8%	31 30.4%	26 26.0%	4 3.6%	3 9.7%		82 15.5%
Not Clear Crime	5 2.7%	2 2.0%	1 1.0%				8 1.5%
Couldn't Id Off. /Get Property / Too Inconv	61 33.3%	14 13.7%	30 30.0%	32 28.6%	7 22.6%	1 50.0%	145 27.4%
Police Not Effective / Biased	11 6.0%	1 1.0%	8 8.0%	15 13.4%	7 22.6%	1 50.0%	43 8.1%
Didn't Want Off. In Trouble	2 1.1%	1 1.0%	1 1.0%	5 4.5%	1 3.2%		10 1.9%
f ear reprisal	3 1.6%	5 4.9%	7 7.0%	11 9.8%	3 9.7%		29 5.5%
Residue		2 2.0%	2 2.0%	2 1.8%			6 1.1%
Total	183 100.0%	102 100.0%	100 100.0%	112 100.0%	31 100.0%	2 100.0%	530 100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	169.215[a]	40	ı000

[a]. 27 cells (50.0%) have expected count less than 5. The minimum expected count is .02.

There is, then, a large proportion of high volume repeat victimization that involves assaults among family members and friends. Within this class of events about one-half are assaults by intimate partners. These events, in turn, seem to correspond to the stereotype of domestic violence. About one-half of these events involve injury and approximately one-half are not reported to the police largely because it is considered a personal matter.

High Volume Repeat Victimization at School

Dodge also noted[6] that a substantial portion of high volume repeat victimization occurs in school situations. The image evoked by his description was that of bullying where a victim becomes the prey of another student or a group of students. The offender singles out the person and then repeatedly humiliates that individual through assaults and other acts of dominance. Whatever their psychic costs, most of these acts involve minimal injury or loss. A large proportion of series incidents at school conform to this stereotype.

Virtually all of the high volume repeat assault victimizations at school involve non-

[6]Victimizations of teachers are included in victimizations at work rather than victimization at school.

Table 10. School Assault Incidents by Acquaintance and Same Person

Offender Across Events	Victim / Offender Relationship		Total
	other	acquaintance	
other	9 15.0%	51 15.0%	60 15.0%
same person	1 2.0%	48 2.0%	49 2.0%
Total	10 • 9.2%	99 9.2%	109 100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	5.436"	1	020

b- 1 cells (25.0%) have expected count less than 5. The minimum expected count is 4.50.

strangers and specifically other students and friends (90.8%; Table IO). These eyents take place in the context of familiarity and, thereby, may be less uncertain and less threatening than events involving strangers. About half (49/109) of all high volume repeat assault victimizations at school involve the same offender which further conforms to the image of bullying (Table 10). Nonetheless about one half of these series incidents have different offenders across events. This suggests that high volume repeat victimization at school may be due in almost equal measure to vendettas and less personal violence. The less personal violence could be the result of gang activity, ethnic or racial conflict or some other conditions that would bring different acquaintances together. It is unlikely that gang activity could account for all of these repeat assaults with different offenders since only twenty percent of the series incidents victims report that the offenders were members of a gang.

In the mean high volume assaults at school are not the serious in terms of durable harms. Less than ten percent (7/109) of these events are classified as aggravated as opposed to simple

assault. Only about 5 percent of series assaults at school were reported to the police, while 30 percent were reported to school officials and 21 percent were considered too minor to report to anyone (Table 9). Only about 20% (21/109) of the school related series assaults reported an injury.

High volume repeat assault at school seem to include two different types. One is similar to bullying in that the same offender commits all of the assaults against the individual. The source of the repeated assaults seem to be personal. The second type involves different offenders. Presumably this type of repeat assault has less to do with the relationship between the victim and the offender than per se. More work needs to be done to describe these types more fully and especially this second subtype. This work will need to await the accumulation of more series incidents from the post 1992 survey since the number of series incident assaults will not support extensive analysis.

High Volume Repeat Victimization at Leisure

Dodges typology did not distinguish events at leisure in part because the information to do so was not available at the time. Since the information identifying activity at the time of victimization became available in the NCVS, a number of analyses have shown that activity specific analyses provide a new view of factors affecting risk (Lynch, 1987; Collins et al., 1987). It may be enlightening therefore to examine high volume repeat victimization in the leisure domain. Leisure is an amorphous activity that is best described as any activity that is not at work, at school or at home. It includes activity such as shopping, going out to dinner, travel other than going to work or school and the like.

It is not immediately apparent what sort of institutional arrangement there would be in leisure activities that would cause a person to remain in a situation that resulted repeatedly in victimization. Leisure activity is almost by definition discretionary such that the person can opt out of activity if they want to.

About sixteen percent of the total high volume repeat assault victimization occurs while the victim is engaged in leisure activity (Table 3). Our expectation was that high volume repeat assaults at leisure would be largely impersonal violence It should involve different offenders over time and these offenders should be strangers. If there was any common thread which linked the events together it would be the location. The expectation was that victims frequent dangerous place in their leisure activity and the result is repeated assaults.

The data do not conform to these expectations. About 42 percent of the high volume repeat assaults involve the same person in all of the events, while only 25 percent of the series have all the events occurring in the same place (Same Both plus Same Place; Table 11). Seventy-three percent of these assaults involve persons that the victims knows and only 27 percent involve strangers. All of this suggests that high volume repeat

Table 11. Leisure Assault by Acquaintance and Same PersonPlace

Same Person I Place	Victim / Offender Relationship		Total
	Other	Acquaintance	
same both	2 1.4%	19 12.9%	21 14.3%
same person	1 .7%	40 27.2%	41 27.9%
same place	7 4.8%	9 6.1%	16 10.9%
mix person/place	12 8.2%	20 13.6%	32 21.8%
different person place	14 9.5%	8 5.4%	22 15.0%
other	1 .7%		1 .7%
residue	3 2.0%	11 7.5%	14 9.5%
Total	40 27.2%	107 72.8%	147 100.0%

assault victimization at leisure is less a situation of frequenting dangerous places than it is of leisure pursuits being the occasion for people who know each other to engage in or continue their conflict. Consistent with this point is the fact that about fifteen percent (22/147) of the high volume repeat assaults occurring at leisure involve the same offender in each event in the series and the offender in each event is a partner, i.e. spouse, ex-spouse or boy/girl friend. Leisure activity becomes the venue for continuing a domestic assault. Additional evidence is shown by the fact that only 30% of these cases are reported to the police with almost 23% stating that they did not report because it was a personal or private matter (Table 9). These events should be classified with domestic assault.

The leisure domain is not particularly useful for understanding high volume repeat victimization . Since leisure activity is largely discretionary , it is not clear what keeps victims in the situation that results in assaults. If we exclude the obvious domestic violence, the

ovehhelming majority of high volume repeat assaults at leisure still involve victims and offenders that know each other.

Dodges Typology Revisited

The analysis of these new data confirm and elaborate Dodge's characterization of high volume repeat victimization. The vast majority of high volume repeat victimization occurs at work, at school and in the context of domestic assault.

The new data also afford some refinement of this tripartite typology. Repeat victimization at work is not just the province of police officers, although they do account for a large number repeat victimization at work. Other occupations such as medical, mental health workers, and teach'ers are well represented among the high volume repeat victims. There is also a substantial representation of retail sales and service workers among the high volume victims.

Some of the newly available data suggest that these different occupation-related victimization may be usefully distinguished. The police and security personnel are the object of high volume repeat assaults because their job requires that they intervene in dispute that other citizens would avoid. This is consistent with the fact that most of the events in the series reported by police officers involve different people and different settings. It is nothing personal, its their job. The situation is a bit different for dirty workers. Their repeat victimizations involve the same persons, but to a greater extent the same places than police officers. The prevalence of repeat actors and places in the victimization of "dirty workers" raises the prospect that the repeat victimization may be more personal or location oriented. This, in turn suggests that strategies for self-protection on the job may be quite different for law enforcement as opposed to "dirty workers".

The newly available data demonstrate convincingly that Dodge's assertion that domestic

violence accounts for a large portion of high volume repeat victimization. Almost one-half of all of the high volume repeat assaults occurring at home involved the same offender and that offender was overwhelmingly a spouse, ex-spouse or boy/girl friend. Equally prevalent relatives among these high volume repeat assaults are assaults by siblings, parents and other known persons. It is interesting to note that a relatively small proportion of partner assaults occurred in settings other than the home (Table 12). Some occurred at work and some at leisure, but the vast majority take place in the home. This suggest that examining domestic violence under the "home" domain may not be completely appropriate. Rather, domestic violence, should include violence among non-strangers regardless of the location of the crime. This provides for a more homogenous typology which can be decomposed further into partner, other relative, and other non-relative acquaintance violence.

Table 12. Partner Assault Cases **By Activity**

Activity	N=155 Percent
work	7.7
school	.6
leisure	16.1
home	65.8
other	7.7
don't know	1.9
Total	100.0

Finally, Dodges classification of high volume repeat assaults in school as bullying is consistent with the new data. About one-half of these assaults involve fellow students who are known to the victim and most of the series have the same offender for all of the event in the series. In most cases all of the events are simple assaults that are reported to school officials, if they are reported at all. The other half of the repeat assaults at school are quite different from the bullying stereotype. Although the offenders are not strangers, it is not the same offender across

all of the events in the series. This suggests that the roots of these conflicts are not in personal disputes among individuals. More attention must be given to describing this subclass of repeat assaults at school.

Finally, the leisure domain, new since Dodge's work, does not appear to provide a unique institutional arrangement that encourages repeat victimization. The overwhelming majority of high volume repeat victimization at leisure appears to be anj extension of personal disputes. They involve the same person and that person is usually an intimate or an acquaintance. They do not seem to be a function of frequenting the same place.

The Duration of High Volume Repeat Victimization

We know almost nothing of about the duration of repeat victimization—how long these series of victimizations lasted.[7] Implicit in Dodge's discussion is the presumption that these high volume repeat victimizations on the job or between intimafes are chronic. They have been going on for quite some time and they will continue to do so. Prior to the redesign of the NCVS, the survey included no information about when these events began and whether the events were still continuing. The redesigned survey asks respondents when the first event in the series occurred and whether they are still going on. This information can be used give us some idea of the duration of events.

Interpreting these data is complicated first by the fact that we know when the series began and we know if it ended, but we do not know when it ended and second, because the time between the beginning of the series incident and the interview is determined in part by the sample design. If you are interviewed in September and the series began in August there will be

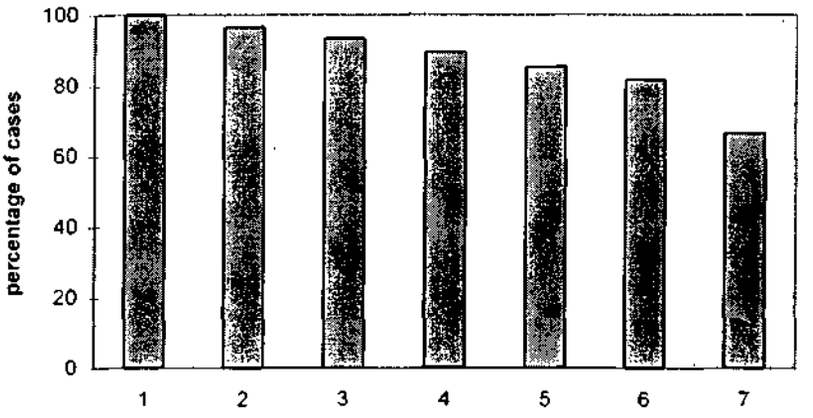
7. The number of events in a series is another way to think about the persistence of an event. In some ways this is a better measure of duration than time to desistance. A series that involves six events over six months is quite different from a series that includes 100 events in six months. Unfortunately, estimates of the exact number of events in a series have been found to be unreliable (Dodge, 1987). Respondents seem to be giving approximations of the number of events that occurred.

only one month in which the series could end. If you were interviewed in July there would be two months in which the series could end. So any assessment of duration we must take account of the time that has elapsed between the beginning of the series and the interview.

It is possible to model these data to estimate the duration of events , if we take account of the time between the interview and the beginning of the series. If we know that an events began that one month ago and that it is no longer going on then we know that it has lasted less than one month. This gives us an estimate of the probability that a high volume repeat victimization, will last a month or less. Similarly, if the series began two months ago and it is no longer going on then we know that the series lasted two months or less. Since we know the probability of termination within a month, then the probability of lasting more than one month but less than two months is difference between the probability of termination in the first month and the probability of terminating by the second month. The probability of terminating in the third month would be the difference between terminating by third month less the probability of terminating in the first or the second month, and so on.

The results of this calculation are presented in Figure 2. They suggest that almost two-thirds of these high volume repeat victimizations have lasted six-months or more. This is consistent with the assumption that these are chronic conditions. They also raise the question of why do some of these events of continuous duration stop.

Figure 2. Percentage of Cases Continuing After Each Month



Time in Months (l=Time 0)

The model described above provides a reasonable estimate of duration given that the NCVS data is cross-sectional.[8] The methods used, however, are somewhat cumbersome for describing differences in duration across types of series incidents. These methods require a large number of cases to give stability to the distributions over months of the reference period. Consider that simply to estimate duration across activity domains would require a 24 cell table- four activity domains by the six months of the reference period.

Given the limitations of these data the best way to assess duration may be to simply distinguish events that persist over long periods of time from those that end in a relatively short

[8]This model will work if the probability of termination is monotonically related to the passage of time--the longer the time between the beginning of the series and the interview, the greater the probability of termination. Unfortunately recall bias is also related to the passage of time and will affect the number and nature of events reported in a given month of the reference period, and thereby the modeling described above. If an event that is continuing is more likely to be recalled and reported overtime than an event that has stopped, then the events reported after six months will include a higher proportion of these continuing events than an events reported after only one month has passed. This could mean that as time passes continuing events could be better recalled. This results in two problems for the modeling described above. First, a larger number of enduring events we be recalled and reported as time passes between the beginning of the series and the interview. So these more continuous events will be over estimated as a proportion of high volume victimizations. Second, the estimate of the probability of termination in these more distant months of the reference period may be underestimated. This would result in a negative probability when the percent terminated in month 6 is less than the percent terminated in month 5. The net result is an over estimate of the duration of high volume repeat victimizations.

period. We can distinguish with some accuracy events that began six months ago (or more) and that are still going on. These victimizations can be considered chronic. We can also distinguish events that began in the last three months and terminated some time during that period. These events are more episodic. While this is not as precise as the exact duration of a series incident, it does provide some idea of the relative duration of events and how duration is distributed across series events.

Table 13. Persistence of Assault Cases **by Activity**

Activity	Duration If Repeat Victimization			Total
	Episodic	Chronic	Else	
Work	1/ 4.8%	194 54.5%	145 40.7%	356 100.0%
School	23 22.1%	25 24.0%	56 53.8%	104 100.0%
Leisure	13 9.7%	33 24.6%	88 65.7%	134 100.0%
Home	35 18.1%	52 26.9%	106 54.9%	193 100.0%
Other	6 14.0%	7 16.3%	30 69.8%	43 100.0%
Don't Know	2 40.0%	1 20.0%	2 40.0%	5 100.0%
Total	96 11.5%	312 37.4%	427 51.1%	835 100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	102.373a	10	000

[a]. 4 cells (22.2%) have expected count less than 5. The minimum expected count is .57.

The duration of these events differs across activity domains. High volume repeat assault victimization at work are disproportionately chronic events (Table 13). Over half (54.5 percent) of series incidents at work began six months ago or more and are still continuing. In contrast, only 24 percent of the high volume repeat assault victimizations at school are chronic, 25 percent at leisure and 27 percent at home. These high volume repeat victimizations are more episodic at

school where 22 percent terminate within three months of beginning, 18 percent at home, 9.7 percent at leisure and only 4.8 percent on the job.

When we examine the more refined typologies we see that law related professions have the most cases classified as chronic (57.1%) (Table 14). Dirty workers follow next with 51.4% and then retail/transportation with 40.5%. Acquaintance related assault cases are far below any work related typology at,32% chronic assaults and partner is even lower at 22.1 % chronic high volume repeat assaults.

Table 14. Persistence of Assault Cases by Work Typology

	Duration of Repeat Victimization			
Occupation	episodic	chronic	else	Total
dirty worKers	6	54	45	105
	5.7%	51.4%	42.9%	100.0%
law	4	89	63	156
	2.6%	57.1%	40.4%	100.0%
retail/transp	5	30	39	74
	6.8%	40.5%	52.7%	100.0%
other	27	73	120	220
	12.3%	33.2%	54.5%	100.0%
Total	42	246	267	555
	7.6%	44.3%	48.1%	100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	30.191[a]	6	.000

[a]. 0 cells (.O%) have expected count less than 5. The minimum expected count is 5.60.

It is unclear why the work environment yields more chronic high volume repeat victimizations than other domains. Perhaps the impersonal violence that characterizes occupations engaged in social control is less traumatic than the more personnel violence that characterizes high volume repeat victimizations in other domains. The oft heard excuse "It's my job." suggests that people are willing to endure more on the job than they are in relationships. This tolerance for enduring victimization on the job may come from a more extreme self-selection process for occupations than for relationships. Those pursuing police work, for

example, know that it will entail some violence while this is less clear for persons in romantic relationships or friendships. In the latter, violence' is perceived to be an aberration not an expectation.

All of the foregoing explanations for the duration of high volume repeat victimization assume that duration is determined by the willingness of either or both parties to end the interaction. For the victim the factors that keep the person in a situation are finally out weighed by the abuse and they take action. For their part offenders must stop either in response to the reaction of the victim (or others) or of their own volition. One of the most common actions that victims can take is to call the police. The question then becomes whether calling the police is related to the duration of high volume repeat victimization? The subsequent question is does differential calling of the police explain the differences in duration across domains?

Overall there is negative but not statistically significant relationship between the duration of a series of high volume repeat victimization and reporting to the police. About 53 percent of the events lasting 3 months or less are reported to the police and 63 percent of the series that began six months or more ago and are still going on are reported (Table 15). This tendency for chronic events to be reported to the police more often than episodic events is reasonably constant across activity domains, with the exception of leisure (Table 16). Among events occurring at work 68.8 percent of the episodic events are reported to

Table 15. All Events Reported by Duration

Reporting to Police	Duration of Repeat Victimization		
	Episodic	Chronic	Total
Not Reported	50	189	239
	20.9%	79.1%	100.0%
Reported	43	120	163
	26.4%	73.6%	100.0%
Total	93	309	402
	23.1%	76.9%	100.0%

Chi-square	Value	df	Asymp. Sig. (2-sided)
Pearson	1.625b	1	*202*

b. 0 cells (0%) have expected count less than 5. The minimum expected count is 37.71.

the police and only 50 percent of the chronic events; at school 13.6 percent of the episodic events are reported and 0 chronic incidents; at leisure 23.1 percent of the episodic events are reported to the police and only 21.9 percent of the chronic events. The big differences in reporting of chronic and episodic events occurs in events at home where 70.6 percent of the episodic events are reported to the police but only 26.9 percent of the chronic events.

Table 16. Activity Reporting By Duration of Event

Activity		Duration of Event		
		episodic	chronic	Total
work	Not Keportea	5 50%	96 950%	101 1000%
	Reported	11 103%	96 897%	107 1000%
	Total	16 779	192 923%	208 1000%
School	Not Reported	19 4329	25 568%	44 1000%
	Reported	3 1000%		3 100.0%
	Total	22 468%	25 53.2%	47 100.0%
Leisure	Not Reported	10 286%	25 71.4%	35 100.0%
	Reported	3 300%	7 70.0%	10 100.0%
	Total	13 289%	32 71.1%	45 100.0%
Home	Not Reported	10 208%	38 79.2%	48 100.0%
	Reported	24 632%	24 36.8%	38 100.0%
	Total	34 395%	52 60.5%	86 100.0%
Other	Not Reported	5 500%	5 50.0%	10 100.0%
	Reported	1 333%	2 66.7%	3 100.0%
	Total	6 462%	7 53.8%	13 100.0%
don't know	Not Reported	1 1000%		1 100.0%
	Reported	1 500%	1 50.0%	2 100.0%
	Total	2 667%	1 33.3%	3 100.0%

These results suggest that reporting to the police may contribute to the termination of high volume repeat victimization in some institutional settings but not in others. Reporting to the police is not related to duration on the job or at leisure. Reporting to the police is negatively correlated with duration in the case of domestic violence. Reporting to the police increases the chance that the high volume repeat victimization will end sooner.

Much more work must be done before we can begin to make statements about what does and does not shorten the duration of high volume repeat victimization. Multi-variate modeling of NCVS data will allow us to test the relative explanatory power of the various explanations offered above as well as many more.

Series Incidents, Repeat Victimization and Events of Continuous Duration

Series victimization has been considered a methodological artifact not worthy of inclusion in annual estimates of crime rates. These events were presumed to be too trivial and the information on them too suspect. The implicit argument has been if these were real crimes then the victim would do something to get out of the situation. There are a number of reasons for reconsidering this appraisal of series victimizations. First, these arguments were not particularly persuasive when they were first advanced. There are plenty of single victimizations with the same level of durable harms as series victimizations that are included in the NCVS annual counts. Second, recent interest in repeat victimization has renewed interest in series victimization. The inclusion of these high volume repeat victimizations can radically affect our assessment of the importance of repeat victimization. Hence, there is more reason to reconsider our treatment of series incidents in the NCVS. Third, Richard Dodge's work on series victimization has removed a great deal of the interviewer discretion which made it difficult to understand exactly what a series incident was. Methodological change in the NCVS based on Dodge's work have reduced this ambiguity further. Finally, more information on events reported in a series are available that allows us to evaluate these events and to better understand why the occur and why they end. For all of these reasons, it is time to return to series victimizations to see if they have something to tell us about high volume repeat victimization.

With respect to the question of whether or not these events are a crimes worthy of inclusion in annual estimates of crime. There is no reason to exclude these events. As we noted above, there are other single victimization events that with similar amounts of durable harm that

are included without question. What makes these events suspicious is the fact that they are not so memorable that each event in the incident can be distinguished in their particulars. But we know from the recency slope in the NVS and from other retrospective surveys that recall of events and their particulars affects all types of events (Biderman and Lynch, 1981). This does not lead us to exclude that which is mentioned.

What seems to cause particular skepticism about series incidents is that they are considered so routine or familiar to the victim. Can crime which almost by definition is "random", undeserved and shocking be so routine and still be a crime? The short answer is yes.

The forgoing analysis shows that much high volume repeat victimization captured as series incidents is chronic. Much of it occurs among familiars and very often involves the same person in every event. Much of it is taken as the cost of fulfilling other related obligations. These incidents are comprised of events that have low levels of durable harm. These events blend into life for some people. Just because these events have become routine does not mean that they are not every bit a crime as those events that are aberrational for most of us. The routinization of these events should be the object of study not grounds for ignoring them.

Part of the reluctance of the Census Bureau and the Justice Department for including series incidents is their suspicion about the estimate of the number of events in a series. Flabby estimates of how many crime events are in a series can substantially inflate the crime rate, This is too great a weight to put on flabby numbers. These legitimate misgivings could be lessened simply by assuming that all series incidents have six crime events. This would minimize the impact of flabby numbers and still not omit these routine, but otherwise crime-like events.

What ever is done for the purpose of estimating crime rates these events of continuous duration are suitable objects for study in their own right. The victims of series incidents are clearly repeat victims (even though the sheer number of events in a series may not be known precisely.) They may be different from other repeat victims by virtue of things other than the volume of their repeated victimization, but we do not know that they are. This should be the

object of study not conjecture. The information available on series incidents in the NCVS offers a unique opportunity to understand repeat victimization. The detailed information available on victims and events is unparalleled as is the fact that respondents are asked whether the repeat victimizations are related in some ways. The information on duration is also unique and promising.

The work done to date on series victimization tells us more than much of the more sophisticated modeling of other self report data on repeat victimization. We know from Dodge's work and the work presented here that high volume repeat victimization is clustered in institutional domains. Much of it occurs in occupations involved in social control, in intimate relationships and at school. These arrangements keep people in situations where they are victimized. Presumably, when people leave these states or positions the victimization will stop.

We know that the nature of these repeat victimizations differs across institutional settings. Much of the high volume repeat victimization at work involves different strangers while the domestic violence obviously does not and the incidents at schools involves acquaintances. From this we would expect that the factors that cause persons to become high volume repeat victims will differ within these institutional domains. For the police officer it may be that some officers have assignments, e.g. vice, that encourages repeat assaults and others do not. For high volume repeat domestic violence being married may be less conducive to repeat violence than living together. We know now to look for different things in different domains.

We also know that the duration of these high volume repeat victimization differ. Some last six months or more and others end in one. The question is why do they end? We know that it varies by domain. Occupation related incidents seem to be more chronic than domestic violence incidents or school incidents. In some domains, calling the police seems to make a difference in duration while in others it does not. In the case of duration, changes in the institutional context may be more important than differences across individuals. Here a change in the work assignment of an individual may cause the repeat victimization to end. Many more

factors must be considered as explanation for the different duration of high volume repeat victimization.

The simple descriptive analyses done by Dodge and those presented here are meant to be exploratory and suggestive. They are meant to pique interest and prose questions for more sophisticated analysis. The work done here suggests three specific types of analyses that can contribute to our understanding of high volume repeat victimization and repeat victimization more generally. First, high volume repeat victimization must be compared to less victimized groups in an effort to explain the high levels of victimization. High volume repeat victims should be compared to non-victims, the singly victimized and low volume repeat victims. The patterns that we see here with respect to domains may be similar to those for the less victimized and therefore not a factor in distinguishing these two groups. Second, if domains continue to distinguish the more from the less highly victimize, then comparative analyses should be done within domains. We know that police officers are more likely to have high volume repeat victimization than college professors, but what is it that distingui'shes among the high and low volume police officers. With so many important characteristics in common other factors must emerge to different high volume repeat victims from those colleagues less victimized. This could be done in the domestic violence domain as well. Third, the question of duration should be the subject of multi-variate analysis. What are the factors that contribute to the termination of high volume repeat victimization? Is it the escalation of durable harm to a point that it can no longer be tolerated? Is it a change in location, a change in occupation or a change in occupation status? Much of the data for exploring these issues is not in the NCVS , but creative use of what is in the survey will substantially narrow the range of factors that should be considered.

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Chapter Four

Explaining High Volume Repeat Victimization on the Job: Going Postal and Threatening Work Environments

Introduction

The concept of repeat victimization has captured the imaginations of scholars and policy makers because of the potential of this phenomenon to promote both our understanding of victimization and the policies that may reduce it (Pease and Laycock, 1996; Sparks, 1981). Empirical investigation of repeat victimization has been limited and somewhat selective. A number of analyses of repeated burglary of housing units have been done, as well as analyses of victimization more generally by location or "hot spots" (Polvi et al., 1991; Sherman et a]., 1989; Pease, 1998). Studies of spouse abuse and domestic assaults have also addressed the issue of repeated assaults. Most of these analyses, however, have been restricted to events reported to the police or to a victim services agency, which is a highly selected subset of crime events. Other analyses of repeat victimization that employ victim survey data include events not reported to the police, but most of these analyses have addressed burglary and not assaultive crimes (e.g., Osbom and Tseloni, 1996; Pease, 1998). Aside for studies focusing specifically on domestic assault, there have been very few analyses of repeated assault victimization using victim survey data.

Dodge (1987) examined high volume assault victimization using "series incidents"

reported in the National Crime Victimization Survey (NCVS). He found that most "series incidents" involving assault occurred on the job or involved intimate partners. These analyses did not receive much attention because it was not exactly clear how well "series incidents" reflected high-volume repeat victimization. Also, Dodge's work was largely descriptive. It did not contrast "series incidents" or "series victims" with persons who were less frequently victimized. Knowing that a large number of repeat victimizations befall police officers on the job, for example, may not mean much if a large number of the singly assaulted are also law enforcement personnel.

This paper addresses the issue of repeat assault victimization by returning to the NCVS and the series incident data. By taking advantage of changes in NCVS procedures introduced in 1992, we will have a much better idea of how well the series incidents reflect the population of high-volume repeated assault victims (Lynch and Planty, 1998a; Rand and Taylor, 1995). This paper takes issue with the point-in-time logic that has been applied to the study of repeat victimization. We propose instead to study patterns of victimization or "events of continuous" that comprise a component of repeat victimization. Moreover, we will attempt to identify factors that differentiate high-volume repeat victims from those less frequently victimized by comparing the singly victimized with those reporting series victimizations. Finally, we will give this analysis sharper focus by examining only victims who experienced their victimizations at work. Prior analyses have shown that "domain-specific" models of victimization provide very different results than those that examine more global classes of crime (e.g. assault) (Lynch, 1987).

Series Incidents and the Universe of High-Volume Repeat Victimizations

Prior to 1992, it was not possible to determine what portion of the repeat victimization universe was reported as series incidents and why. The rules for treating an incident as a series were in the interviewer's manual, but they were vague and there was no way to assess whether the interviewers were uniformly following these procedures. As a result, it was impossible to tell how much of the high-volume repeat victimizations were reported as series events and how those reported as a series differed from those reported on indiiiidually. This made it very difficult to [1] determine whether a series incident was a case of high volume repeat victimization or simply an attempt by interviewers to reduce their workload or the demands they placed on the, respondents.

In 1992, questions pertaining to the eligibility of events for treatment as a series were added to the interview. What was formerly buried in the interviewer's manual was now explicitly asked of respondents. More importantly, this information is in the public use file so that users of these data can determine how many potentially eligible repeat events were accepted for treatment as a series and why others were rejected.

Interviewers are instructed to ask the following questions:

4.0 Altogether how many times did this type of incident happen during the last six months?

Check Item B
How many incidents?
1-5 incidents (not a series)
6 or more incidents (go to Check item C)

Check Item C
Are these incidents similar to each other in detail or are they for different types of crime?
(Ask if not sure)
Similar (fill in Check Item D)
Different (not a series)

Check Item D
Can you recall enough details of each incident to distinguish them from each other? (If not sure, ask.)
Yes (not a series)
No (is a series , ask Q5a.)

Only events that involve six incidents or more, for which the incidents are similar and the respondent cannot distinguish individual events, are treated as a series. Between 1992 and 1995, 1,696 events involved six incidents or more (Figure 1). Of those, 1,615 were treated as series incidents. So the vast majority of very high-volume victims are included in the NCVS as series incidents. Less than one percent are reported on as individual incidents. Series incidents, then, can be used to speak to the universe of high-volume repeat victims.

"Going Postal" Versus "Threatening Work Environments"

Despite the interest that repeat victimization has engendered, many of the basic definitional issues that Sparks (1981) raised remain unresolved; indeed they are seldom discussed. For example, the number of events required to make someone a repeat victim has never been established. Nelson (1980) and others have shown that the distribution of victimization events is not what we would expect if each event were random. While this establishes repeat victimization exists, it does not address whether there is a qualitative difference between two victimizations or five in a given unit of time. It is likely that high-volume repeat victims are different in important ways from persons with only two victimizations in the same time period.

This conceptual ambiguity complicates the interpretation of these data on series victimization. Dodge's (1987) work suggests that respondents may not be able to report accurately on exactly how many incidents may have occurred in the series. Moreover, there is implicit in the fact that official reports exclude series incidents belies an assumption that series incidents are different from single incidents. Perhaps it is the routineness of the crime - i.e., it happens so often that the respondent cannot report on individual events. If this is the case, can it be a really crime, since we assume that most respondents would find the stereotypic crime a memorable event.

This suspicion of series incidents is unwarranted. What is considered a flaw for the purpose of computing crime rates is an advantage for understanding the occurrence of crime and particularly for understanding a large part of repeat victimization. The suspicion of series incidents comes from the point-in-time orientation of our statistical systems. These systems appropriately count individual events. Consequently, it is difficult to accumulate or associate those events across time and establish patterns across events. One attribute of a pattern is the frequency of the event over time. Another aspect could be the similarity of events or the sequence of events over time. A burglary followed by another burglary, for example, may be different from an alternating pattern of burglaries and assaults. Similarly, a pattern where all the incidents involve the same persons are different from those that have different participants.

While some of this identification of patterns has been done with point-in-time events, especially for events that share locations, much of the information required to do this is simply not available in most of our statistical data (Reiss, 1980). For example, our ability to link events by person or place is limited. The information required to make such a match is often not

collected at all or is collected in a way to make the match uncertain. When one aspect of the events is certain (e.g. location), others are not (e.g. persons involved)(Sherman et al., 1989). Moreover, there is no provision in these data for the participants to put the events into a larger context that would associate them as a pattern. For example, we would never know that two assaults were part of a dispute over unionizing the workplace of the offender and victim. The assaults are part of this larger pattern of disputing. This kind of contextual information is not in police records or asked about in victimization surveys.

For the purpose of establishing patterns of repeat victimization, we would argue that series incidents are superior to data systems based on a point-in-time logic. Because of the changes made in 1992 to the series incident procedure, we know the victim is exposed to a high volume of repeat victimization. That is one aspect of the pattern. In addition, we know whether the events were similar and whether they involved the same persons and places. These too are attributes of patterns of repeat victimization. Being able to establish these patterns is far more important than correctly counting the number of repeated point-in-time events.

Given that series incidents establish a pattern, what does this pattern mean? We argue here that the pattern of victimization identified by the series incident procedure constitutes a threatening work environment. The victimizations are frequent and they occur on the job. The fact that respondents cannot remember the individual details of the events is due to the fact that these events have been routinized—i.e., accepted as part of the work environment. The fact that this routinization has occurred makes them different from other more point-in-time crimes, but not less important. Threatening work environments can be considered crimes of continuous duration. While victimization may not occur every day, it occurs with enough frequency that the

threat of victimization is continuous. Workplaces with patterns of high-volume repeat victimization are in a state of continuous assault and the people in these environments **will** respond accordingly.

Studying threatening work environments is quite different from studying victimization at work (Lynch, 1987; Collins et al., 1987). There can be a large number of victimizations at work that are not part of the pattern of victimization. Convenience store clerks may be repeatedly victimized, but it will not be at the same volume as in a threatening work environment and it will not become routine. The respondent victim will be able to recount the details of each event. It will still be considered an aberration and not "just part of the job."

While studying series incidents may not be ideal for studying crime at work or even repeat victimization at work, it is well suited for studying that component of repeat victimization that creates a threatening work environment.

Theories of Repeat Victimization and Dangerous Work Environments

There are few theories of repeat victimization. One of the more enduring theoretical frameworks was introduced by Sparks (1981). He suggested that factors that encourage repeat victimization could be divided into those that were the result of the heterogeneity of risk across people or groups and those that were due to entering into the state of victimization initially. This simple distinction has guided much of the empirical work done on repeat victimization to date (Lauritsen and Davis-Quinnet, 1995). Researchers have tried to establish the relative contributions of state dependence and heterogeneity. Heterogeneity explanations posit that

relatively enduring characteristics of victims such as gender or race predispose persons to different risks of victimization. These differential risks would explain why some persons are victimized twice or three times in a given period and others are not victimized at all. State dependence explanations emphasize the fact that the first victimization predisposed the person to the second and the third (Polvi et a]., 1991; Pease, 1998). This could occur where the first burglar discloses the content of a household to a colleague, who later burgles the same house a second time. Absent knowledge of the contents obtained from the first burglary, the second would not have happened. This distinction between heterogeneity and state dependence is useful because the policy implications are quite different. Heterogeneity explanations would direct prevention and enforcement efforts to broad classes of people with a relatively high risk of victimization. State dependence explanations would suggest that prevention and enforcement efforts should be focused on the previously victimized persons or places.

For all its utility, the simple state dependenceheterogeneity distinction is relatively content-less. It is useful for organizing specific explanations, but is not particularly suggestive of same. Moreover, the distinction between what is a state and what is so enduring as to be a source of heterogeneity is not entirely clear. Is an occupation a state or a source of heterogeneity? How about marital status? Finally, the work done to date has focused only on the state of victimization and not other time-dependent processes that may affect the risk of victimization at time one and at time two. So the addition of an adult household member may result in burglary victimization because more people become aware of the property in the household (Lynch and Berbaum, 1998). Similarly, the ejection of a roommate may result in a burglary motivated by revenge. So changes in the household composition over time, and not the prior victimization,

may account for repeat victimization of the unit.

General theories of repeat victimization, then, are of little help in suggesting explanations for repeat assault victimization. The more specifically focused empirical work on repeat victimization also does not seem particularly appropriate. Much of this work has addressed repeat burglary victimization that, on its face, is very different from assault. Burglary usually involves little contact between the victim and the offender (Shover, 1991; Cromwell et al., 1991). Target selection is largely a function of information flows concerning the availability, and desirability of goods, the risk of apprehension, accessibility of the targets and the like (Bennett and Wright, 1984). For the bulk of burglaries, the entire process of target selection is universalistic and impersonal. A target that the offender comes across with the desired characteristics will be victimized. Assaults, it seems, would be more particularistic. There is necessarily contact between the victim and the offender. The targets chosen are more often selected because of who they are and not because they have a given set of characteristics. Consequently, factors like the interaction between the victim and the offender prior to the victimization or the relationship between the victim and the offender would play a much larger role in determining who will be assaulted and when than would the information flows that seem to affect the occurrence and distribution of burglary. By extension, the work on repeat burglary would seem to have little to tell us about high-volume repeated assault.

The largely descriptive work that Dodge (1984; 1987) has done on assault, together with the domestic violence literature, does suggest an organizing principle in the search for explanations of high volume repeat assault Victimization. The fact that he found most series incidents in the NCVS involved intimate partners or occurred on the job suggests that high-

volume repeat assault victims are attacked repeatedly in part because they cannot leave the situation that predisposes them to victimization. Spouse victims may not leave because they are invested in the relationship or have children that cannot easily be moved. Those assaulted on the job may endure repeated assaults because they need that job to sustain themselves and their families. The search for explanations for high-volume repeat assault, then, would focus on encumbrances that discourage victims from leaving the situation that precipitates assault, as well as factors that would encourage victims to leave those situations.

The search for explanations of high-volume repeated assaults would therefore begin by identifying those factors that expose persons to high risk and then identifying those factors that encourage or discourage leaving that situation. Because we are addressing high-volume repeat victimization at work, the nature of encumbrances may be quite different from those affecting high-volume repeated spousal assaults but the general concept of encumberences may still be useful.

Following Dodge (1987), we know that some occupational roles will predispose role incumbents to higher risk than others. Certainly, the very high rates of series incidents for police officers suggests that persons perfonning occupational roles that require maintaining order will be repeatedly victimized. Maintaining order can lead to disputes and disputes lead to assaults. Other occupations cany these obligations to a greater or lesser degree. Security guards, school teachers, and hospital orderlies will also have some order maintence responsibilities. The greater the order maintenance responsibilities, the higher the volume of repeat victimization and the more threatening the work environment.

Occupational roles can put an individual in proximity to disputes in other ways. Highly

competitive work, where performance standards are clear and rules of conduct ambiguous, can give rise to disputes among co-workers. These disputes can become assaults.

There are other attributes of the occupational role that can expose persons in that role to repeated victimization. Jobs that involve contact with other persons (either customers or co-workers) are more likely to result in assaults than those that do not (Lynch, 1987). Persons who work alone may be at greater risk than persons whose job requires working in groups. These attributes of the occupational role make people vulnerable not because they put them in proximity to disputes, but because they either leave workers accessible or do not provide the guardianship that could protect them from assault.

Even within high risk jobs that require maintaining order, there is variability in terms of the volume of assault victimization. For example, some police officers become repeat assault victims and others do not. This could be due to the fact that certain assignments (e.g., undercover operations, drug operations) can bring an officer to places where disputes and assaults occur with great frequency. If an individual were not assigned to these positions, they would not frequent these places. This is not an attribute of the occupational role per se. Not all people in the job face these risk, just those who are required by the position to come to dangerous places.

Differential risk of assault on the job can be due less to the nature of the job than to the manner in which role incumbents fulfill that role. Some have alleged, for example, that women may be less aggressive or less provocative than male officers in confrontations with citizens (Martin, 1975). The greater aggressiveness of male officers could encourage repeated assaults by citizens.

All of these factors-the occupational role, the specialty within that role, and the manner in which role incumbents execute that role-can affect the risk of assault on the job. Previous work examining the incidence of victimization at work found that the attributes of the job were more important predictors of victimization than the attributes of the employees themselves (Lynch, 1987; Collins et al., 1987). It remains to be seen whether this is the case for repeat victimization at work.

Given this enhanced risk due to the occupation, what encumbrances cause some people to stay in these situations and others to leave? Certainly, the marketability of a person in a particular job would affect their willingness to stay in the face of repeated assaults. Persons with other job opportunities would presumably leave for something that involved less risk or for some other assignment within the occupation that involves lower risk. On the other hand, employees may assume the risk when they accept the job. Indeed, some may thrive on that risk. Police work necessarily involves higher rates of assault than other jobs, so an officer must be prepared to accept that as part of the role.

The decision to leave risky situations can also be affected by the seriousness of the assaults that occur. An officer gravely wounded in his first encounter may take steps to reduce his risk in the future, whereas another officer victimized by a lesser assault may not take similar steps because the particular of victimization did not exceed the benefits of the job or his expectations of risk.

While we know very little about what predisposes persons to repeat assault victimization on the job, the framework described above provides a useful starting point for our empirical investigation of this issue. The specific hypotheses that we derived from this framework and that

will be tested are presented in Table 1.

(Insert Figure 1)

(Insert Table 1)

Testing Models of High Volume Repeated Assaults

The model will be estimated using data from the NCVS. Specifically, we will compare those persons reporting one assault at work during the last six months with persons reporting a series victimization during the last six months. Those reporting a series victimization will be considered high-volume repeat victims and those reporting only one victimization will be the "singly victimized." The occupational role, attributes of the occupational role, attributes of persons in those roles, and attributes of the crime events will be used to predict who will be a high-volume repeat victim and who will be singly victimized. The relative predictive power of these variables will indicate which of these factors encourage high-volume repeat victimization. These results, in turn, will be used to inform the theoretical issues raised in the previous section.

The specific variables and indicators included in the model are described in Table 2 and discussed selectively below.

The dependent variable in the analysis is the threatening work environment measured by whether the respondent reports a series incident at work in a given interview. Persons indicating that they had six or more incidents that they could not distinguish were considered as having a threatening work environment. Those who reported only one victimization during the prior six

months were regarded as not having a threatening work environment.

The determination of series or non-series is based upon a given six-month period covered by the interview. No effort was made to link interviews over time. This means that persons who report series incidents in their first interview, but only one incident in their second interview, can appear in the file twice--once as a person with a threatening work environment and once as person without such an environment. This can reduce the discriminating power of the models and the lack of independence between observations can affect standard errors. If the survey responses are an accurate reflection of the volume of victimization at work at that time, then using them in this fashion is appropriate. Moreover, if people move between these statuses with great frequency, then models based on static individual characteristics should not be too discriminating.

A series of dummy variables was used to indicate attributes of the occupational role. Three occupations were identified as having major order maintenance responsibilities—law enforcement officers, medical practitioners, and teachers. Each of these groups was identified by a dummy variable indicating membership in that occupation (membership=l, else=0). Several other occupations were identified because they exposed their members to risk or they failed to provide guardianship, and not because of the order maintenance requirements of the role. These occupations include persons in retail sales and those in transportation occupations such as taxi or bus drivers. These occupations require contact with the public, mobile work places that are not easily protected, and the transport of money. All of these characteristics can expose an individual to assaults (e.g., robberies attempts that end in assault). A dummy variable was introduced for each of these occupations.

Several variables were included in the model measuring the dangerousness of the environment in which the work is performed. These attributes of the environment are not part of the occupational role per se. While the job may put the person in these situations, not everyone in the occupation functions in the same environment. Central city location is one of these variables. The crime rate in central cities is much higher than elsewhere, so persons working in central cities should be at greater risk than persons performing the same task elsewhere. Persons who work in the central city of an SMSA were scored 1 on this variable and others were scored 0. The same logic was used in the case of persons who work nights as opposed to days and those who work alone as opposed to in groups. Persons who work at night were scored as 1 and persons not working at night were scored as 0. Those working alone were scored as 1 and those working with others were scored as 0.

There are several individual attributes that can affect their performance in the occupational role. Two items were used to measure the marketability of the individual and thereby their ability to leave the job. These items were the educational attainment and income of the individuals. Education was scored "1" if the respondent had more than a high school education and "0" if they had a high school education or less. Income was coded in fifteen ordered intervals. Two other items were used to measure the social integration of role incumbents—whether the respondent was married and how many times he or she had moved in the last five years. Married respondents were scored "1" and all others were scored "0". The number of times moved was simply the actual number of times the individual reportedly moved.

Three different attributes of the event(s) were included in the model--the degree of durable harm, the degree of threat, the public nature of the event, and the relationship between

the victim and the offender. Harm was indicated by whether the victim was injured (l=injured, O=not injured) and whether he lost time from work as a result (]=lost time, O=no time lost). The degree of threat was measured by whether there was a weapon present (1=present, O=not present), whether there was a gun present(1=gun, O=no gun), and the total number of offenders (1=more than one, O=one). Two dummy variables were used to indicate degrees of relationship. The first scored the respondent " 1 " if the offender was known and "0" if the offender was not known. A second dummy variable was " 1 " if the offender was a spouse, ex-spouse, girlfriend or, boyfriend and "0" otherwise. If the event occurred in a place open to the public, the variable was scored " 1 " and, otherwise, it was scored "0".

The data used to test these hypotheses were taken from the NCVS incident level file for 1992 to 1995. We aggregated the survey data over three years because assaults generally and series incident assaults specifically are relatively rare. During this period, there were 1,615 series incidents reported in the NCVS and. of these, 385 were assaults that occurred at work. These series victimizations were included in the analysis file. All victimization incidents that occurred to persons reporting more than one incident in a given interview were deleted from the analysis file. All of these " single " incidents that did not occur at work were also exclude from the analysis file. The final analysis file included all series incidents that happened at work (n=385) and all" single " incidents at work (n=1483).

The model will be tested using logistic regression since the dependent variable—single versus high volume repeat assault--is dichotomous. In the first model, all of the predictor variables will be introduced into the equation. In the second model, all of the indicators of occupation will be excluded from the model to determine the effect of that block of variables on

the predictive power of the model. Subsequent models will exclude other blocks of variables starting with attributes of the occupational role and then attributes of individuals and attributes of events.

Results

The model that includes occupation, attributes of the occupational role, attributes of the person in that role, and attributes of the situation provides the best fit to the data. This model explains about 16 percent. When the dummy variables for occupation are removed, the r-square decreases by more than 50%, from 16 percent to less than 8 percent. This indicates that a person's occupation is an extremely important predictor of the likelihood they will be a victim of high-volume repeat victimization at work. The removal of blocks of variables pertaining to attributes of the work role and attributes of the situation also produced a significant drop in the r-square, which indicates their importance in predicting the probability of becoming a high-volume repeat victim at work. Removing attributes of the occupational role, such as whether someone worked at night or whether they worked in a central city of an SMSA, reduced the r-square from 16 percent to 14.8 percent. Excluding attributes of the victimization event that described the seriousness of the victimization reduced the chi-square from 16 percent to 14.1 percent. When the attributes of persons in the occupational role are excluded from the model, the chi square goodness of fit measure does not decrease significantly (from 16 percent to 15.4 percent). These results indicate that the occupation chosen is the most powerful predictor of being a high-volume repeat assault victim at work. The results also suggest that holding specific jobs or roles in an

occupation will affect the risk of repeat victimization and that the nature of the victimization will also influence the risk of becoming a high-volume repeat victim on the job. Attributes of people in the occupation are the least powerful predictors of high volume repeat victimization on the job.

While some of the effects of occupation on the risk of repeat victimization are consistent with expectation, others are not. Working in law enforcement or security has a statistically significant and positive effect on repeat victimization (b=2.14). This is consistent with Dodge's (1987) work. Working in medical occupations and in teaching also has a significant and positive effect on the risk of repeat victimization (b=.56 and .53 respectively), but this effect is about one-fourth of that observed for law enforcement occupations. It was somewhat surprising that being in retail and transportation occupations did not have a significant effect on the risk of victimization. since earlier studies had found occupations like these to be at high risk (Lynch, 1987; Collins et al., 1987).

Attributes of the environment in which the work is performed also affect the risk of repeat victimization. but the magnitude of these effects is substantially less than it is for occupation. The chi-square for the model drops from 16 percent in the best fit model to 14.8 percent in the model with attributes of the environment excluded. While this is a statistically significant decrease in the chi-square, it is about one-sixth of the decrease observed when the occupation dummies were removed.

The attributes of the environment affect the risk of high-volume repeat victimization in the expected ways. As expected, working at night has a significant positive influence on the risk of repeat victimization (b=.62). Similarly, working in central cities increased the risk of high

volume repeat victimization as expected (b=.311, p=.02). Working alone was also expected to increase the risk of repeat victimization and, indeed, was found to be associated with high-volume assault victimization on the job *(b=.36,* p.=.027).

The effects of individual attributes of persons in the job do not have a significant effect on risk of high-volume repeat victimization as a block. When these variables are omitted from the model, the change in the chi-square is not significant. Specific attributes, however, are significantly related to risk of repeat victimization on the job. Specifically, gender, mobility, and income affect the risk of repeat assault victimization on the job. Men have a higher risk of high-volume repeat assaults on the job than do women (b=.41, p=.012). Persons who have moved numerous times in the past five years also have a significantly higher risk of repeat victimization than more settled persons (b=.066, p=.022). Education and income are not significantly related to the risk of repeat victimization on the job. Age has a positive and marginally significant effect on high-volume repeat assaults (b=.011, p.=.099) Marital status has a negative effect on risk of repeat victimization, but this effect is only marginally significant (b=-.264, p.=.08).

The attributes of crime events as a block have a statistically significant effect on the risk of repeat victimization on the job. When these variables are omitted from the model, the r-square decreases from .16 to .141. This is approximately the same effect on the model as we observed for attributes of the work environment, and substantially less than the effect of occupational role. Some of the effect of attributes of crime events are in the expected direction and others are not. The seriousness of the crime event was expected to be negatively related to the continuation of the assaults and, thereby, the occurrence of high volume repeat victimization. Seriousness was assumed to be a function of the durable harms resulting from the event and the

amount of force or threat used; the greater the harms, the less the chance of high-volume repeat victimization. The public nature of the event was also presumed to affect the transition to repeat victimization; the more public the event the less likely it was to continue.

Some forms of durable harm were associated with repeated assaults and others were not. Physical injury had no effect on the risk of repeated assault, while time lost from work had a large negative effect on that risk (b=-l .76, p=.007). Events that resulted in loss of time from work did not become high-volume repeat assaults. The degree of threat also has mixed effects on the likelihood of repeated victimization. The presence of a weapon does not affect the risk of repeat victimization, but the presence of a gun has a strong negative effect on that risk (b=-.998, p=.006). Familiarity between victims and offenders was expected to promote repeated victimization because this familiarity would both make the parties available to each other while reducing the uncertainty of the outcome. In fact, knowing the offender has no significant effect on the likelihood of repeated victimization, but when the offender is an intimate there is a strong positive effect on the likelihood of repeat victimization. This effect is probably due to the fact that some domestic violence occurs at work and these events have little to do with the job per se (Lynch and Planty, 1998a). Finally, the public nature of the assaults is not consistently related to the likelihood of repeat victimization. Reporting the event to the police, for example, has no effect on the likelihood of repeat victimization, while having the event occur in public has a large negative effect on the likelihood of becoming a repeat victim (b=-.545, p=.001).

Conclusions

We know very little about high volume repeated victimization generally and even less

about such victimization in the workplace. Much of the theory and empirical evidence are taken from studies of property crime (especially burglary) and is not particularly appropriate for repeated assaults. There is some information on repeat assaults from the domestic violence literature, but the domestic context is quite different from the workplace and it is not clear how well theories that explain the former can help us understand the latter. Given this state of existing knowledge, this s?udy presents a minimal theoretical framework and some empirical evidence to begin the process of constructing a theory as to why people become the victims of high-volume repeated victimization on the job, as opposed to becoming one-time victims.

The common wisdom flowing from Dodge's work (1987) was that high-volume repeated victimization at work was the result of the occupation chosen. Some jobs, and especially those involving order maintenance functions, are riskier than others. People in those jobs will be victimized at a higher rate than people in other jobs. Dodge's work, however, was only descriptive and did not control for other factors, such as the specific characteristics of the occupational role or the attributes of persons filling that role, that could account for differences in the risk of repeat victimization. Police officers, for example, are disproportionately young and male, which should increase their risk of victimization in any given unit of time and thereby their risk of repeat victimization for reasons independent of their job.

This study confirms Dodge's conclusions. Occupation is the major determinant of whether a person will become a high-volume repeated assault victim on the job even when other aspects of the individual are held constant. Moreover, those jobs that involve order maintenance functions have a much higher risk of repeat assault than those that do not. Being a police officer, security guard, or in some other law enforcement occupation substantially increases the risk of

becoming a repeat assault victim on the job. Working in medical occupations and as a teacher, however, also increases the risk of repeat victimization, even though the effect on risk is much less than for law enforcement personnel. Being in other occupations that involve risky behavior (e.g. contact with the public, handling money, or having a mobile workplace) but not order maintenance functions does not significantly affect risk of repeat victimization. The risk of high-volume repeat victimization on the job seems to be directly related to the prominence of the order maintenance function of the job. Thus, persons in occupations that require a great deal of order , maintenance will have a high risk of becoming a repeat assault victim on the job than those that do not.

It is also interesting to note that order maintenance obligations are highly correlated with high-volume repeat assaults on the job and vulnerability in the sense of exposure or absence of guardianship. This suggests that the high-volume repeated assault is far from a random event. It is not something for which every working person is at risk; rather it is an intrinsic part of the job.

These findings with regard to the danger of occupations differ when studying the incidence of victimization rather than the risk of repeated victimization. While the risk of , victimization on the job generally is highest for police and security personnel, medical occupations and teachers, the risk for taxi drivers, bus drivers or persons in retail businesses is higher than it is for other occupational groups (Warchol, 1998). In this analysis of high-volume repeat victimization, such is not the case. Police officers and the other occupations experience high-volume repeat victimization, but those in transportation and retail do not. The findings from prevalence studies and studies of repeat victimization are not necessarily contradictory. Persons in order maintenance occupations may have high prevalence as well as high incidence,

while persons in retail and transportation may be high on prevalence but not on incidence. Perhaps persons in retail and transportation can change to areas or specialties within the occupation where the risk is less. In so doing, the victimization would not persist and the incidence would remain loh.

While the occupation chosen is the principle determinant of high-volume repeat victimization, other factors influence risk. Attributes of the work environment such as the dangerousness of the location also affect the risk of repeat assaults, but much less so than the , occupational role per se. Nonetheless, people in specific occupations can reduce their risk by not working at night or by looking for work in less dangerous areas.

These types of changes in task or environment may be prompted by the nature of the victimization encountered on the job. Events that have serious repercussions such as time lost from work or a serious threat (e.g., presence of a gun) will generally not become chronic or high-volume repeat victimization. It is not clear from these data exactly why or how this is. It is possible that these victims leave the job or go to a different environment while retaining the same job. It is also possible for them to stay in the same environment and job but simply do the job differently. Sorting out these reactions would be useful. It is somewhat surprising to see the amount of harm or threat that must occur before there is a difference between single and series events. General weapon use, for example, does not make a difference. It takes a gun. Injury is not sufficient.

It is clear from these models that the occupational differences in high-volume repeat assaults are not due to the selection of different types of people into these occupations. Demographic characteristics of the respondents do not have as much of an effect on the risk of

high-volume repeat victimization at work. The fact that gender and marital status have some influence on risk is interesting, but the effects are small.

It is interesting to note that the effects of reporting to the police on high-volume repeat victimization on the job is not significant, but nevertheless negative. This stands in stark contrast to what is found in studies of repeat assaults among intimates where involving third parties is a powerful impediment to the persistence of the assault. For some reason, involving third parties either does not occur or it is not very effective in stopping this behavior. From what we know about the differences between partner violence and repeat violence at work, this result makes some sense. In partner violence, the offender is the same person in almost all of the incidents, while in repeat violence at work the offender is very often different in each incident. In the former case. intervention of third parties may have a specific deterrent effect on the individual offender, \vhile in the latter case it will not as the lesson of third party intervention is lost on the offender.

This study provides some useful information for the development of interventions that may reduce the level of repeat assaults on the job. Since the greatest determinant of repeat assaults or threatening work environments is the occupational role, it makes sense that this would be the place to look for ways to reduce this phenomenon. Unfortunately, these data are not sufficient to specify particular changes in the occupational role, although collecting such data is conceivable. Moreover, it is probably the case that many of the attributes of the occupational role cannot be changed to reduce the risk.

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Table 1: Hypotheses

1.0 The occupational role will have a greater effect on the occurrence of high volume repeat assault than attributes of the job, attributes of the person or attributes of the events.

1.1 Occupations that ask members to perfom a social control function will have more high volume repeat assaults than other occupations.

1.2 Occupations that place members in situations where they are alone, carry money and have contact with the public will have more high volume repeat victims than other occupations.

2.0 The attributes of the specific job environment will affect the risk of high volume repeat victimization.

2.1 Persons who work at night will be more often high volume repeat victims than those who ordinarily work in the daytime.

2.2 Persons who work in central cities will be high volume repeat victims than persons who work elsewhere.

3.0 The characteristics of persons filling specific occupational roles will affect the risk of victimization.

3.1 Persons with high levels of education will have greater marketability than those with lower educational attainment. This enhanced marketability will enable them to leave risky jobs and thereby reduce the risk of repeat victimization.

3.2 There are fewer high income jobs than low income jobs. The demand for high income jobs is greater that the demand for low income jobs. Persons with high income have less marketability than those with low income and therefore less ability to leave risky jobs. Consequently income should be positively related to becoming a high volume repeat assault victim.

3.3 Women will not be as physically aggressive in the performance of their occupational roles and will therefore encourage repeat victimization on the job.

3.4 Married persons will have greater stability in their lives and greater responsibilities that require them to minimize risks and thereby avoid high volume repeat assaults on the job.

3.5 Residential mobility is a sign of instability and the willingness to take risks. This risk taking behavior will extend to the job where the person will not avoid dangerous situations and will become a victim of high volume repeat assaults.

3.6 Younger persons will be more willing to take risks on the job and that will make them more likely to be high volume repeated assault victims than older persons.

4.0 Attributes of the victimization events will affect the risk of being a high volume repeat assault victim.

4.1 Events with high levels of injury or loss will encourage the victim to avoid such situations in the future and thereby reduce the risk of the victimization continuing and becoming high volume repeat victimization.

4.2 Events that involve greater force or threat will encourage victims to leave dangerous situations and reduce the risk of becoming high volume repeat victims.

4.3 Events among persons known to the victim may be part of an ongoing relationship which will prevent the victim from leaving the situation and will increase the risk of becoming a high volume repeat victim.

4.4 Events in which third parties are asked to intervene are less likely to high volume repeat assaults because these third parties will take steps to alter the circumstances so that the assault cease.

4.5 Events that take place in public do not let the victim ignore or rationalize them. Victims are more likely to take steps to avoid repeat occurrences and high volume repeat, victimizations are more likely to occur.

Table 2: Concepts, Variables and Indicators

Concepts	Variables	Indicators	Coding
Occupational Role	Social control role	Law Enforcement	I-Law 0-Else
		Medical	1-Medical 0-Else
		Teachers	1-Teachers 0-Else
	Exposure	Retail	1-Retail 0-Else
		Transportation	I-Tramp. 0-Else
Attributes of Job	Guardianship	Night work	I-Night 0-Else
		Others Present	1-Others 0-Alone
	Proximity	Central city location	1-City 0-Else
Attributes of Persons	Marketability	Education	I-Highschoolor< 0-else
		lncome	Categories 1-14
	Integration	Marital status	1-Married 0-Else
		Times moved in previous 5 years	Reported Number
	Crime Related Attributes	Gender	1-Male 0-Female
		Age of Victim	Reported Age
		Black	I-Black 0-Else
		Hispanic	1-Hispanic 0-Else
Attributes of the event	Harm	Injury	I-Injured 0-No Injury
		Time lost from work	1-Workloss 0-No Workloss
	Degree of Threat	weapon present	1-Weapon 0-No Weapon
		gun present	I-Gun 0-No Gun
		number of offenders	1 -one 0- >one
	Familiarity	offender known	1 -acquaintance 0-other
		intimate partner	1-partner 0-other
	Privateness	occurred in public	1-outdoors 0-indoors
		reported to police	1-reported 0-not reported

Table 3. Univariate Statistics

Variables N= 1645	Mean	Median	Std. Dev.	Range	Min.	Max
Single_Series	.21	.00	.40	1	0	1
Law	.I7	.oo	.38	I	0	1
Medical	.I3	.oo	.34	1	0	1
Retail	.I6	.oo	.36	1	0	I
Teaching	8.14E-02	.oo	.27	1	0	1
Transportation	3.27E-02	.oo	.18	1	0	1
City Work	.56	1.00	.50	1	0	1
Night Job	.21	.oo	.41	1	0	1
Others	.71	1.00	.46	1	0	1
Education	.37	.oo	.48	1	0	1
Income	9.95	11.00	3.48	13	1	14
Age	35.40	34.00	11.43	76	12	88
Gender	.64	1.oo	.48	1	0	1
Married	.54	1.oo	.50	1	0	1
Moved	1.75	1.oo	2.35	30	0	30
Black Victim	7.33E-02	.oo	.26	1	0	1
Hispanic Victim	5.53E-02	.oo	.25	1	0	1
Injury	.I2	.oo	.33	1	0	1
Work Loss	1.53E-02	.oo	.I8	1	0	1
Weapon	.21	.oo	.40	1	0	1
Gun	1.73E-02	.00	.28	1	0	1
One Offender	.83	1.oo	.38	1	0	1
Acquaintance	.50	1.oo	50	1	0	1
Partner	1.82E-02	.oo	.I3	I	0	1
Reported	.46	.oo	.50	1	0	1
Outdoors	.33	.oo	.47	1	0	1

Table 4: Logistic Regression Models Predicting Series as Opposed to Single Victimization.

Variable	Full Model, N=1645	Block 1	Block 2	Block 3	Block 4
Law	2.149 .000***		2.268 .000***	2.151 .000***	2.013 .000***
Medical	.558 .014**		.647 .004***	.563 .0055***	.575 .008***
Retail	.015 .948		.119 .599	-.041 .853	.022 .919
Teacher	.527 .053*		.476 .079*	.527 .039**	.602 .023**
Transportation	.304 .491		.227 .602	.303 .484	.033 .940
City Work	.317 .024**	.145 .267		.247 .064*	.279 .040**
Night Work	.612 .0001***	.971 .000***		.735 .000***	.654 .000***
Others Present	.350 .033**	.373 .016**		.335 .034**	.375 .014**
Education	-.068 .653	-.173 .212	-.058 .699		-.109 .454
Income	.008 .721	.033 .115	.002 .948		-.002 .924
Age of Victim	.011 .124*	.011 .070*	.009 .199		.012 .078*
Gender	.410 .013**	.640 .000***	.406 .012**		.305 .048**
Married	-.259 .088*	-.112 .433	-.304 .041**		-.256 .082*
Moved	.065 .025**	.060 .028**	.071 .014**		.078 .005***
Black Victim	-.063 .815	.250 .308	.030 .909		-.159 .539
Hispanic Victim	-.388 .225	-.311 .301	-.473 .133		-.458 .145
Injury	.128 .574	.139 .521	.174 .441	.117 .588	
Workloss	-1.754 .008***	-1.755 .006***	-1.863 .004***	-1.828 .0047***	
Weapon	.259 .209	.266 .161	.289 .152	.298 .126	
Gun	-.979 .007***	-1.108 .001***	-1.013 .004***	-1.185 .0007***	
One Offender	-.378 .059*	-.094 .616	-.377 .057*	-.370 .054*	
Acquaintance	-.060 .698	-.085 .544	-.134 .377	-.166 .254	
Partner	1.711 .0001***	1.381 .001***	1.652 .000***	1.703 .000***	
Report	-.205 .169	.196 .138	-.168 .252	-.240 .094*	
Outdoors	-.540 .001***	-.340 .025**	-.625 .000***	-.524 .001***	
Chi-Square =	288.35	135.25	262.66	500.53	260.62
df =	25	20	22	17	16

p =	0.000***	0.000***	0.000***	0.000***	0.000***
Pseudo-R^2 =	16.1%	7.9%	14.8%	15.4%	14.1%

* significant at < 0.1; ** significant at < 0.05; *** significant at < 0.01

Chapter Five

Partner Violence:
An Examination of Assaultive Relationships

Introduction

Violence between intimates is widespread. Researchers have estimated between 15-30% of all couples have experienced some form of violence in their relationships (Frieze and Browne, 1989:177-180). Since there is good reason to believe that intimate violence is under-reported in both police records and self-report surveys, some sources estimate that the rate is much higher (Straus, Gelles and Steinmetz, 1980; Strube, 1988). In 1996 there were 1,800 murders and approximately 1,000,000 violent incidents involving intimates (BSJ, 1998). The literature paints a picture of intimate relationships as often hostile and potentially dangerous. Moreover, these characterizations suggest that violence among intimates is enduring or cyclic (e.g., Gelles, 1974; Rounsaville, 1978; Walker, 1979).

Past research has identified correlates of domestic violence in an effort to understand why it occurs (e.g., Gelles and Straus, 1988; Jasinski and Williams, 1998; Ohlin and T o n r y , 1989). The use and interpretation of this work must be tempered by an awareness of its limitations. Specifically, while the theoretical discussions of intimate violence have addressed both the prevalence (if it occurs) and the incidence (how often it occurs), much of this research has focused on prevalence. The reasons why someone is assaulted once in a given period of

time, 'however, may be very different from the reasons why the assaults persist.[9] It is important, therefore, to study the problem of persistence independent of prevalence to see if the reasons people stay in chronically abusive relationships are different from the reasons that episodic assaults occur.

Previous empirical work that has addressed the issue of incidence used only the highly selective populations taken from police or victim service agency records (e.g., Weis, 1989; Johnson, 1995). Examining only the limited population of those who seek aid, however, can yield inaccurate estimates of the nature of intimate violence (e.g. percent resulting in injury) and correlates of persistence that would not hold in the population of intimate assault victims more generally.

This paper uses the data on series and non-series incidents from the National Crime Victimization Survey (NCVS) to explore the issue of persistence. These data permit the separate treatment of incidence and prevalence. Since it involves the use of a general population survey, the data is not as selective as police or agency data.[10] High volume repeat victims of assault by intimates identified by the survey are compared to persons who report only one incident of intimate violence in the same six-month period. We argue that the difference between these victimization experiences is persistence. Various theories of persistence that have been identified in the intimate violence literature will be tested. Specifically, we identify five concepts that have

9 Persistence is used here as a specific subset of incidence. It is appropriate for partner violence because in almost all cases the repeated victimization involves the same victim and the same offender across the crime events. In this way, these repeated partner assaults are more like a continuous state than other types of repeated assaults that do not share the same victim and the same offender.

10 The NCVS includes combinations of both male and female victims and offenders. This being said, 87% are female victims, 83% involve male on female assault, 11% involve female on male assault, and the remaining 6% involve same-sex partner assault. In a preliminary examination comparing only male on female assault to the whole sample, no significant differences were identified.

been correlated with chronic violence between intimates—economic barriers to leaving, stress levels and sources, level of violence, exposure to social controls, and individual attributes. In addition, we control for several response errors that may affect the quality of the NCVS data, including the a "gag effect" (Coker and Stasny, 1995).

Violence Among Intimates, Domestic Violence and Abuse

Identifying the relevant research in this area is complicated by the wide variety of terms 4 used to characterize this crime, each of which can have a slightly different meaning (Gelles, 1985; Gelles and Straus, 1988; Weis, 1989). Domestic violence is often defined as,wife battery in the home, but it also includes child abuse, violence towards husbands, partner violence and elderly abuse. Spouse abuse falls under the larger umbrella of domestic violence. Abuse is differentiated from violence, the former emphasizing the enduring nature of the assaults. Partner violence implies that the offender and victim are intimate, but not necessarily in a marriage relationship or even living together. Given the number of different terms used to characterize violence among intimates, it is important that we define our terms at the outset.

This paper explores "intimate or partner violence." We define a partner as a spouse, ex-spouse, boyfriend, or girlfriend. These relationships seem to be very different from other types of relationships between individuals. For example, there are certain expectations (e.g., the expectation of intimacy) and routines between partners that are ofren very different from those between friends, other family members, co-workers, or roommates. Individuals seem to invest more in relationships with partners and therefore will expect more (Rusbult and Martz, 1995).

This investment may also differ in terms of the quality and type of investment. It is logical then to believe that the reasons why a person continues to remain in a violent relationship **may** be due to the unique nature of relationships between partners.

Theories of Persistence from the Partner Violence Literature

Research examining intimate violence, particularly spouse abuse, have identified **a** number of potential sources for chronic violence between intimates. These various factors discourage the victim from leaving the relationship or otherwise ending the abuse. Further, these factors can be usefully grouped into those that are social structural, those that are due to the relationship, and those that are situational. A victim's position in the social structure, such as whether they are employed, can affect their ability to be economically self-sufficient and thereby their ability to leave abusive situations. Similarly, the nature of the relationship between intimates can influence persistence. A long-term marriage may be more difficult to leave than a dating relationship. Finally, aspects of the victimization itself have been identified as contributing to the persistence of the violence. Victimizations that involve egregious injury or the intervention of a third party, for example, may enable a victim to leave or cause that third party to end the violence.

Social Structural Factors: Economic Conditions

The economic status of victims may be related to persistence in that it can affect the victim's ability of the victim to leave (e.g., Gelles, 1976; Kalmuss and Straus, 1982; Truninger,

1971; Strube and Barbour, 1983, 1984; Rusbult and Martz, 1995) or take other actions **that** would end the violence. Most simply, if a victim feels that she does not have the resources **to** survive on her own, she will endure the assaults. This may be especially true if ending **the** violence means arresting or incarcerating the offender who may be her only source of income. The absence of economic resources may also encourage persistence in interaction with other factors. Children, for example, may present additional economic strains (Gelles, 1976; Gelles and Straus, 198S>, so that a victim who could support herself independently of her partner may not be able to support a family by herself. It is even possible that the mere perception of economic deprivation can encourage persistence as in the case where a victim is afraid of losing her children in a custody dispute because she is unemployed. She may have resources to live independently but she may feel that the perception of economic inability may influence the court's decision. Others argue that a victim, "given sufficient motivation," will find a way to leave regardless of resource availability (Rounsaville, 1978:17).

Social Structural Factors: Stressful Environments

Research has shown that the level of stress in the environment is correlated with abuse. The more stress individuals in a relationship are exposed to, the greater the likelihood of violence in that relationship. Stress can be a product of urban life, low income, or unemployment (Straus et al., 1980; Gelles, 1985). The lack of resources to meet daily needs creates tension between individuals living in the same space. Stress can also be the product of personal trauma outside of the relationship such sickness, addiction, or death of family members. The level of stress in the environment can therefore affect the prevalence of intimate violence and the persistence of stress

can lead to the persistence of violence.

Relational Factors: Marital and Family Status

The nature of the relationship between intimates can also encourage or discourage persistence. The institution of marriage may inhibit a person from leaving an abusive relationship (Strube and Barbour, 1984; Rusbult and Martz, 1995). Individuals who formalize their relationship with all the social and legal implications of such action may be more likely to remain in the relationship (and thereby remain at risk) than those in less formal unions. Conversely, individuals in formal marriage relationships may have a greater incentive 'to seek intervention by a third party because of the perceived need to make the relationship work. Similarly, intimates with children may have more to lose in leaving because they have invested or committed more to the relationship (Strube and Barbour, 1984; Rusbult and Martz, 1995). Additionally, it may be more difficult to leave a long term union than it a more recent marriage. Victims who leave their relationship may also stigmatized by friends, family, and others. This may be especially true when children are involved. On the other hand, the presence of children may discourage persistence if the parent fears that the violence will or has affected the children (Rounsaville, 1978).

There are other qualitative aspects of relationships that can affect persistence. Couples with a greater ability to communicate may be able to find ways to reduce stress or resolve disputes in ways that discourage the continued use of violence.

Situational Factors: Level of Violence

The level of the violence or durable harm is another important factor in violent relationships. Low-level violence (e.g., verbal assaults. slapping, pushing) may reduce the level of exposure to social controls and it may be easier for the victim to rationalize the behavior. The victim may feel they are able to deal with the abuse by means other than formal actions. If the nature of the violence is not severe (e.g. slaps) the victim may not recognize herself as **a** victim unless it escalates into more serious violence. If a victim suffers serious harm, however, she may mobilize or be exposed to both formal and informal mechanisms that will reduce the violence (Rounsaville, 1978). Hospital room workers, for example, are trained to recognize abuse and to intervene. Friends, family, and co-workers who become aware of the violence may offer resources and alternatives to end the relationship. High levels of injury and time lost from work, therefore. may reduce the likelihood that a person stays in a chronic abusive relationship. This may happen because the victim finally defines the event as a crime or the outcomes bring the violence to the attention of others.

Situational Factors: Exposure to Social Controls

Exposing the violence to a wider public may be an important aspect of the informal and formal social control of violence between intimates. The very private nature of intimate relationships may be an important factor that increases the likelihood that abuse becomes chronic. Most intimate violence occurs in a private setting and it therefore becomes difficult for the usual mechanisms of social control to intervene (Lynch and Planty 1998a; Zawitz, 1994; BSJ, 1998). Further, many victims fail to report violent behavior. This may be due to a fear of

escalation, a sense of shame, certain economic barriers, or a belief that the partner's behavior will change. When assaults are exposed to others, however, informal or formal social controls may be triggered. Events that occur outdoors, at work or during leisure activities, or when the victim reports the assault to the police or other agency, may keep the violence from becoming chronic. Assaults that occur at howe are not likely to be exposed and may therefore encourage persistence.

There can be a variety of mitigating circumstances that will make a victim reluctant to mobilize the police or others to end the violence. The involvement of alcohol or drugs, for example, offers a convenient justification for the violence (Flamer, 1993; Kaufman Kdntor and Straus, 1987, 1989; Jasinski and Williams, 1998). He "only does it when he gets drunk" removes the offender's responsibility for the violence. The substance and not the person is to blame (Gelles and Straus, 1988; Gelles, 1993). Various forms of provocation or perceived provocation on the part of the victim may also make a victim reluctant to seek external help.

A great deal of attention has been given to the actions of formal agents of social control, such as the police, once they have been mobilized (e.g., Sherman and Berk, 1984; Berk and Sherman, 1988; Berk, 1993). Mandatory arrest has been advocated as a response to intimate violence. Arrest is believed to have both a specific and a general deterrence effect on subsequent offending and thereby the persistence of intimate violence. Others have suggested that such actions by the police only worsen the situation by depriving the families of the offender's income or exposing the victim to retaliation (Buzawa and Buzawa, 1993). Whatever the effectiveness of mandatory arrest as a response to intimate violence, it is clear that what the police do after they are called is believed to have an effect on the persistence of the victimization.

Testing Theories with the NCVS

The NCVS includes information on many of the theories of partner violence identified as important to the persistence of intimate violence. The specific indicators used to measure these concepts are presented in Table 1 and discussed selectively below.

Persistence

Persistence of intimate violence is indicated by the volume of assaults that a respondent reports in the previous six months. Persons reporting one assault by an intimate in the previous six months is considered a low-volume or episodic victim of intimate violence. Persons reporting six or more assaults in that period are considered high-volume repeat victims or victims of chronic assaults by intimates. These high-volume repeat assaults must also be committed by the same person.

It is somewhat arbitrary to select the standard of six or more assaults in a six-month period for chronic or persistent assaults. One could argue that a single assault every six months for twenty years is chronic or persistent. Conversely, six assaults in a six-month period followed by zero assaults in the next twenty years may not be chronic. In one sense, then, our operationalization of persistence can understate the condition because it only examines a six-month slice of life. Someone who does not qualify as chronic in the six months may qualify in the seventh in which case we have incorrectly labeled as non-chronic someone who is indeed chronic. This will be the case in any cross-sectional study of persistence. Life goes on even though your study may not and as life changes so will one's value on the dependent variable,

persistence. Presumably studies with longer, even lifetime, reference periods will be less subject to this error. Unfortunately, these studies are subject to substantial recall bias as people selectively forget events from the more distant past (Biderman and Lynch, 1981). There is then the trade-off between accuracy in recall and the completeness with which the full range of experience is captured by a survey. Since no optimum point has been established for this trade-off, we can only proceed with studies using different points on the continuum and compare the result to see if the choice affects the results.

The other problem with our definition may be with the choice of six assault victimizations as a standard for chronic. One can legitimately argue that two or three assaults in a six-month period would constitute persistence. The cut-off point of six was chosen because these persons are clearly the victim of repeated violence in a short period of time. Moreover, there was some evidence that about half these victims did not believe the pattern of violence was over at the time of the interview (Lynch and Planty, 1998). The event or condition was continuing. Second, by choosing this criteria we could take advantage of the new questioning on series victimization in the NCVS. These additional items allowed us to establish that all of the events in the series involved the same person and place over time. This information is useful in distinguishing a series of unrelated events from events of continuous duration where the victim and the offender are the same. This is a defining characteristic of persistent intimate violence that is difficult to establish unless the victim is asked to do so. Since the major purpose of most victimization surveys is the estimation of victimization rates, they treat every incident as a point-in-time event and do not ask victims to make associations across events. This severely limits the

11 In 97% of the series incidents the offender was the same person. In 3% of the cases, the offender was the same

ability to identify correctly events of continuous duration or persistence. The trade-off here is between identifying completely repeat victimization as opposed to identifjing events of continuous duration, i.e., those that share persons or places.

Either way, however, we run the risk of failing to identify chronic victims. We therefore chose to emphasize the inter-relatedness of events and to use the series victimizations for which that determination could be made. This will undoubtably understate the level of chronic or persistent assault. but it is not clear how this will affect the distribution of chronic intimate assaults across the levels of the independent variables used in this analysis. If the distribution of chronic assaults across levels of the independent variables does not differ from that obtained with less restrictive definitions of chronic, then the particular definition of chronic used here will not affect the results.

Finally, our decision to exclude 1Tictims with less than six assaults from our definition of chronic was done to eliminate ambiguous cases and thereby increase the chances of finding concrete differences between the unambiguously chronic and the episodic, i.e., a single victimization in a six-month period. The very restrictive operationalization of persistence that we used here will likely accentuate the differences between episodic and chronic victims of intimate violence. Less restrictive definitions would increase the false positives and thereby degrade or mask differences between episodic and chronic victims. Other "cut points" on the continuum from episodic to chronic victims of intimate violence can be employed in future research to determine the effects of these decisions regarding the measurement of persistence.

Given that the definition of persistence is six or more assaults by the same intimate

person some of the time. None of these cases had a different person as the offender all of the time.

partner, it is important to note that virtually all persons reporting six or more victimizations in the NCVS are treated as series incidents (Lynch and Planty, 1998). Consequently, using series incidents to indicate persistence should not introduce error into our analysis. Prior to the redesign of the survey, the number of incidents required to be treated as a series incident was much lower and much more at the discretion of interviewers. This fact made it difficult to determine whether being treated as a series incident was a true reflection of high-volume repeat victimization or simply interviewer preference. Changes in this procedure in 1992 lessened interviewer discretion when defining series incidents and gave us more confidence that series incidents do reflect high-volume repeat victimization.

Social Structural Factors

The survey includes information on the economic status of victims including their employment status, household income, and educational attainment. It also contains data on factors that are related to stressful environments including whether the respondent lives in the central city of Standard Metropolitan Statistical Area (SMSA). These variables can be used to test both the economic and stress-related theories of persistence. Household income can be used to indicate the general level of economic strain faced by the household. The employment and educational attainment of the victim will be used to measure the dependence of the victim and thereby her ability to leave. Central city residence will be used as an indicator of a stressful environment.

Other social structural variables were also included in the model even though they were not explicitly referenced in the literature on partner violence. Most of these variables have been

found to be correlated with victimization more generally (however many are also contradictory and controversial (Gelles and Loseke, 1993; Jasinski and Williams, 1998). These variables include the race, gender, age, and residential mobility of the victim. The rates of violence are found to be highest for hispanic females between victims between the ages of 18-30 years. Generally, the less mobility the more likely the chronic abuse.

Relationship Factors

The NCVS has some information on the nature of the relationship between the partners, but not a great deal of information on the perceptions of the relationship or the quality of it. The survey records the marital status of the victim at the time of the incident (and currently), whether the victim lived in a household with children, and the degree of relationship between the partners engaged in the violence (e.g., spouse versus ex-spouse). These variables can be used to measure the degree of investment the victim has in the relationship. The greater the investment, the greater the persistence.

Situational Factors

The NCVS has an abundance of information on the situation in which the victimization(s) occur that can be used to measure the concepts identified in theories of persistence. One dimension of the seriousness of the violence is the degree of injury, threat, or durable harm. The NCVS collects information on whether an injury occurred and how serious the injury was, i.e., treated in emergency room or requiring a hospital stay. Degree of threat would be indicated by the presence of a weapon, and whether the victim was actually hit or threatened but not struck.

Days lost from work would be an indicator of the extent of durable harm. The greater the level of durable harm, the less likely that the assault will persist.

The involvement of third parties is measured by whether the victim reported the incident to the police or some other official. The model also includes variables to assess the degree of intervention, such as whether the victim was arrested and whether the victim received services from a victim services agency.

The public nature of the assaults is measured by 1) whether there was someone else present at the time of the assault, and 2) whether the incident occurred at home or in a public place.

The model contains two attributes of the assault that could be perceived as mitigating circumstances which might encourage victims to stay in a relationship involving frequent assaults. The first of these attributes was whether the offender was perceived to be under the influence of alcohol or drugs, and the second was whether the victim resisted. That an offender was drunk or high gives a victim the opportunity to attribute the assault to the substance and not to the person. Victim resistance implies that the victim perceives a threat or attack and is attempting to protect herself. Victims who perceive themselves in a dangerous environment may be more likely to try an leave or end the violence.

Response Errors: Gag Effect and Survey Controls

Unlike most victimization surveys, the NCVS has been the subject of a great deal of methodological work and, as a result, we have some knowledge of the error structure of the survey (Biderman et al., 1986; Biderman and Lynch, 1991; Coker and Stasney, 1995). Specific

procedures used in the NCVS (and other surveys) result in non-uniformities in measurement where some respondents report more or less victimization because they received a specific procedure even when the actual rates of victimization across respondents is the same.

The "gag effect" refers to the unwillingness of respondents to report victimization when other members of the household are present or for a particular type of interview method (e.g., telephone). Coker and Stzsney (1995) found victims reported fewer rapes in in-person interviews conducted with others present. This suggests that survey conditions may gag or cause under-reporting by the respondent especially with crimes involving sexual assault and domestic violence. In an effort to address this problem, we enter a variable indicating whether there was another person present when the interview was conducted and a second variable indicating whether the interview was conducted in-person or on the phone. If the presence of others makes those with more persistent victimization more reluctant to talk about their experience then those less victimized. this variable will assess the effects or the magnitude of that reluctance. It is most likely, however, that the gag effect will influence whether a person will report intimate violence at all and not whether they will report less as opposed to more persistent violence. There is some concern that the gag effect could influence the distribution of the type and severity of violence. While a victim may report an incident, they may not reveal the true extent of the violence in terms of injury, situational variables, or offender characteristics.

Given that the NCVS data used here is cross-sectional, there is the chance that respondents who experience the initial assault very near to the date of the interview will not have the chance to experience additional assaults before the interview and thereby be classified as chronic victims. Conversely, respondents experiencing their first victimization in the most

distant month of the reference period will have a greater chance of becoming a chronic victim during the reference periods and thereby being recorded as such in the survey. This can lead to the classification of some persons as episodic victims when, at a later date, they would become chronic victims. In an effort to take account of this bias we have entered into the model the month of the reference period in which the initial event occurred. If there is an effect based on the time between the interview and the event, it should be reflected with this variable.

The Models

Four different logistic regression models were estimated predicting persistence. The first model included all partners, i.e., spouses, ex-spouses, boyfriends, and girlfriends. The second model employed the same sample but excluded the variables measuring the gag effect. A third model was estimated with only spouses and the fourth model was estimated with just spouses and with the gag effect variables omitted. The distinction between spouses and non-spouses was made because of the ambiguity of the status of non-spouse partner with respect to intimacy. Boyfriend and girlfriend can have a wide range of meanings and the effects of various predictor variables can be quite different for this group and spouses. The gag effect variables were removed simply to see what effect response error had on the models. The models estimated included direct effects for all of the variables described in the foregoing section and listed in Table 1.

Table 1. Univariate Statistics

Variables N=687	Mean	Median	Std. Dev.	Range	Min.	Max.
bing~e_berles	.23	.oo	42	1	0	1
Age	30.29	29.00	10.22	75	13	88
Children	.69	**1.oo**	.46	1	0	1
Employed	.58	**1.oo**	.49	1	0	1
Education	.57	I.oo	.49	1	0	1
Income	6.79	7.00	4.14	13	1	14
Leisure	.I6	*.oo*	.37	1	0	1
Home	.62	**1.oo**	.49	1	0	1
Working	4.95E-02	.oo	.22	1	0	1
Moved	2.71	2.00	2.81	20	0	20
Others	.43	.oo	.50	1	0	1
Outdoors	.25	.oo	.43	1	0	1
Black Victim	.16	.oo	.37	1	0	1
Hispanic Victim	8.3OE-02	.oo	.28	1	0	1
Gender	.13	.oo	.34	1	0	1
Resistance	.78	I.oo	.42	1	0	1
Alcohol/Drugs	.42	.oo	.49	1	0	1
Married	.17	.oo	.38	1	0	1
City	.35	.oo	.48	1	0	1
Injury	.46	.00	.50	1	0	1
Work Loss	6.I1E-02	*.00*	.24	1	0	I
Weapon	.I6	.oo	.37	1	0	1
Gun	4.66E-02	.oo	.21	1	0	1
One Offender	I.04	1.oo	.I9	1	1	2
Spouse	.34	.oo	.48	1	0	1
Ex-spouse	.I6	.oo	.37	1	0	1
Report to Agency	.I8	.oo	.39	1	0	1
Report to Police	.59	1.oo	*.49*	1	0	1
Arrest	.21	.oo	.41	1	0	1
Respondent Only	.18	.oo	.39	1	0	1
Telephone	.54	1.oo	.50	1	0	1
Risk Period	3.46	3.00	1.92	5	1	6

Results

The models for partner violence differ somewhat from those predicting spousal violence. The response error variables have large effects on the explanatory power of the models but including or excluding these Lrariables does not affect the coefficients for the other predictors. They tend to retain their sign and magnitude.

Partner Violence

The model of persistence estimated with all partners explained a substantial amount of the variance in persistence. The pseudo-R squared for the model was .246, which was' significant at the .OOO level. In the best-fit model at least some social structural variables, relationship variables, and situational variables affected persistence. Household income had a negative effect on persistence. The higher the household income the less likely the assaults were to become chronic. If the offender was an ex-spouse, the probability of the assaults becoming chronic increased. Events that were reported to the police were significantly less likely to become chronic than those that were not reported to the police. Receiving assistance from victim service agencies was positively related to persistence. Persons who sought service were more likely to be victims of chronic assaults than single assaults. Mediating attributes of the event, such as whether the offender was under the influence of alcohol or drugs or whether the victim resisted, were not significantly related to persistence. The presence of others during the interview had a significant, positive effect on persistence. Persons interviewed with others present of company were more likely to report chronic assaults. Also, the time between the event and the interview

was significantly related to persistence such that the longer the period from the initial event to the interview the more likely it was for repeat assaults to have occurred.

It is equally important to note the variables that were not significantly related to persistence. The economic status of victims did not affect persistence. Thus, victims with **more** than a high school education or those who were employed were not more likely to be victims of chronic assault than other victims. Victims with more commitments, either in the form of children or marriage, were no more likely to be chronic victims than other respondents. **The** level of durable harms resulting from the assaults did not affect persistence nor did the level of threat indicated by weapon use or injury. Whether the event(s) took place in a public getting also did not affect persistence.

When the gag effect variables and the risk period variables were removed from the model, the power of the model decreases substantially, but the effects of the independent variables stay largely the same as in the original model. The psuedo-R squared for the model without these response variables was .126 or about half that of the model including the response error variables. The effect of the risk period variable is particularly large. There is some reason to believe that this variable may over-correct for differences in risk period. More will be said about this in the concluding section.

Spousal Violence

The models estimated with only spousal assault victims were much more powerful in explaining persistence than models estimated with all partners. The pseudo-R squared for the spouse-only models was .383, which was significant at the .000 level. Some of the variables

found to be significant in the partner models were also significant in the spouse-only models. Chronic assaults were less likely to be reported to the police than single events. Chronic assault was associated with seeking service from a victim service agency. The gag effect variable was positively related to persistence, as was the time between the initial event and the interview. While the effect of income was not significant at the .I level, it was negative and close to significant (.13). Other variables that were not significantly related to persistence in the partner model were significant in the spouse-only models. The age of the victim was negatively related to persistence. The younger the victim the more likely the assaults were chronic. Still, other variables that were significantly related to persistence in the partner models were not significant in the spouse-only models. Specifically, arrest was negatively related to persistence in the partner violence equation but insignificant in the spouse-only equation. Arrest of the offender had no effect on persistence in spousal assaults.

These relationships do not change much when the gag effect and the risk period variables are removed from the model, but the psuedo-R squared drops from .38 to .25.

Discussion

Violence among intimates is prevalent and often serious. It is important that we understand the sources of this violence so that appropriate responses to the problem can be developed. Unfortunately, this type of violence is difficult to study because it is often not considered a crime by the participants and even when it is considered a crime, victims are often reluctant to report it. Consequently, much of our empirical research is conducted with flawed

data. In view of this, it is important to examine intimate violence using a variety of data sources each of which has a different set of limitations. If persistent patterns emerge then we can be more confident that the observed results are not due to the error structure of a particular data source. It is important that any given study be interpreted in light of the likely sources of error in the data used.

This paper examines the issue of persistence in intimate violence using the series incident data from the NCVS. These data have been largely ignored in the past because of uncertainty about their accuracy. Methodological work in the mid- and late- 1980s, as well as the redesign of the survey in 1992, clarified some of the ambiguity concerning the meaning of series incidents. These clarifications made these data more usable for the examination of the persistence of intimate assaults.

The foregoing analyses suggest that it may be useful to distinguish among assaults involving partners and those involving spouses. While many of the determinants of persistence are the same for both groups, some are markedly different. Moreover, our ability to predict persistence of spousal assault is substantially better than our ability to predict the persistence of partner assault more generally.

The analyses of both partner and spouse assault support some of the theories of persistence derived from prevalence studies and fail to support others. Partner assault persists in low-income households more so than in high-income households. This is consistent with the strain theories of persistence where the lower socio-economic status increases the anxiety of not having the things necessary to sustain life and this, in turn, increases stress which leads to violence. These findings are also consistent with sub-cultural theories of violence wherein

violence is the prescribed way of resolving disputes including domestic disputes.

The fact that many indicators of the socio-economic status of the victim are not significant fails to support inequality theories of persistence. These theories attribute intimate violence to economic inequalities between partners. For example, where one spouse has a job and the other does not, the jobless spouse cannot leave the relationship because she has no means of support. Consequently, she stays and the assaults persist. If this was the case, we would expect that women with less education or women who are unemployed would be over-represented among the chronic assault victims. These variables, however, have no effect on persistence. Explanations of persistence that emphasize children and the willingness to endure chronic assaults because of children are also not consistent with these results. Living in a household with children has no effect on persistence.

Theories of persistence that emphasize relationships receive some support from this analysis. Being married at the time of the assaults is not significantly related to persistence. Thus, being legally married rather than being in some other type of partnership does not protect one against the persistence of assault. However, whether the offender was an ex-spouse is positively related to chronic assaults. This suggests that the process of marriage dissolution may be the occasion for multiple, persistent assaults. Since we do not know when the marriage was dissolved, we cannot say definitively that this is the case. It may be that the hostility between ex-spouses continues for many years, rather than being restricted to the period of the actual breakup.

This analysis also supports theories of persistence that emphasize mobilizing third parties as a means of stopping assaults before they become chronic. Police involvement is significantly correlated. negatively, with chronic partner assault. More informal forms of third party

involvement, such as having the event occur in a public place or with others present, does not seem to affect persistence. On the other hand, more proactive forms of third party involvement are related to persistence in some cases but not in others. Specifically, arrest is negatively associated with persistence for partners, but not for the persistence of violence among spouses. This may be due to the fact that persons in less formal partnerships dissolve their union in the aftermath of the arrest while married persons do not. Because the relationship continues so does the assault.

Involving certain third parties, such as victim service agencies, is positively related to persistence for both partners and spouses. While it is possible for services to cause continuation, as in the case where the assailant is angered by the fact that the spouse has received service, it is more likely that persistent assaults are referred to services more frequently than episodic assaults. Victims may also choose to use these services as a last resort, when no other alternatives are available.

Theories that link escalation to the severity of the event are not consistent with these analyses. None of the indicators of harm (e.g., injury, loss of time from work, or weapon use) are significantly related to persistence. Evidently, even assaults that have higher levels of injury and affect other social realms (i.e., work) can persist. Greater durable harm appears not to prevent continuation of the assaultive behavior.

Finally, the presence of mitigating attributes of events does not seem to influence persistence. Offenders use of alcohol or victim resistance is not related to persistence.

In sum, then, these analyses offer some support for theories of persistence that emphasize strain due to economic marginality, the stability of relationships, and involvement of third parties

(going public). They are not consistent with theories that explain persistence in terms of economic inequality between partners, the presence of children, or the severity of the crime events.

In evaluating these'analyses it is important to understand the limitations of these data and the assumptions underlying the analysis. We believe that the strength of these data lie in differentiating victims of episodic intimate violence from those for whom intimate violence is a continuous state. We use high volume repeat victimization in a short period of time as an , indicator of this state. We are using attributes of events to characterize people. Consequently, the information on the attributes of events in series incidents is simply not as good as The infomation provided on episodic incidents. Respondents reporting series incidents are summarizing pattern of behavior, while those reporting on one incident are reporting on a point-in-time event. The former will be a less adequate description of the individual events in the series, but perhaps a good description of the pattern. As a result, the effects of attributes of events, such as whether the police were called or if there was injury, on persistence may not be as accurate or as easily interpretable as they would be in data with more of a focus on individual events. On the other hand, point-in-time data collection may confuse the random occurrence of events with a pattern of inter-related behavior. All of this suggests that it will be useful to contrast these results with those using longitudinal data on the occurrence of incidents of intimate violence to see if attributes of individual events have different effects on subsequent events.

An important assumption of this cross-sectional study is the operationalization of persistence. It contrasts persons who have experienced persistent patterns of intimate violence

with those who have not. The difference between these two groups is assumed to be due to the persistence of the assaultive violence. We do not measure persistence per se. That would require longitudinal data on intimate assaults so that an assault at TI could be used to predict the risk of assault at T2 and beyond. 'These repeated assaults could then be used to define patterns of persistence. Such data would probably give greater weight to attributes of incidents, such as reporting to the police or injury, in predicting persistence than the approach that we have taken in the paper. This is the case because the data on individual incidents would be better and we , would be better able to control for the effects of characteristics of individuals on persistence. In the cross-sectional models used here, differences between the single and series victims'can be due to persistence and a whole host of other factors that cause respondents to report single as opposed to series incidents. We must rely on statistical controls to hold these factors constant so that we can model persistence.

With this said, unless approaches that build patterns of assaults from data on individual incident reports ask respondents about the inter-relatedness of these events, then they may misidentify randomly occurring events as patterns. At this point, self-report data of this type is not available. Until it is, the best approach would be to examine the problem of persistence with a variety of data sources and compare the results to provide some enlightenment.

Given the complexity of this problem and the quality of existing data, it would be foolhardy to make policy recommendation based on these results. It is useful, however, as an exercise, to reduce the result to one simple and premature policy statement. If one recommendation must be made from this study, it would be that getting victims of intimate violence to the attention of the police limits the persistence of that violence. Whether the police

arrest the offender or not seems less important universally than whether the police are called.

One interpretation of the relationship between reporting to the police and persistence of partner assaults is that the police intimidate the offender and, as a result, the assaults did not continue. It is equally plausible, however, that the involvement of the police is simply a manifestation of a prior decision by the victim that the assaults are not normal behavior and they must stop. This decision may lead to the victim terminating the relationship or simply redefining it so that the violence does not continue. It is this decision and the subsequent steps taken by the victim and not he police involvement per se that makes the violence stop. We do not have the information necessary to choose from among these different explanations.

The differences between the spouse-only analysis are interesting. First, the fact that our models have much greater explanatory power for this restricted class of partner assaults than they do for the wider class of partner assaults suggests that these two classes—spousal assault and non-spouse partner assault—should be treated separately in future analyses. Second, the broad similarities in the partner and spouse-only models suggests that some of the same processes are at work in fostering persistence. Third, the importance of victim age and reporting to an agency other than the police in the spouse-only model (and not in the partner model) is intriguing. One way of accounting for the negative relationship between age and persistence in the spouse-only model is that age is serving here as a proxy for the longevity of the relationship. The longer people have been married the less likely they are to engage in chronic assault behavior. Alternatively, those who engage in chronic assault behavior are less likely to stay married. Hence, younger married folks will engage in persistent assaultive behavior but older married folks will not.

Whether the victim seeks help from an agency other than the police is significantly related to persistence in the spouse-only sample, but not in the partner sample. This may be due to the fact that married couples will be more likely to seek aid than non-married couples. It could also be that these non-law enforcement agencies may be helpful in getting the participants to end the assaultive behavior when they have more committed people to work with

Table 2. Variables

Economic Barriers
Household Income Level
Victim Employed
Education Level
Children
Reported to Police
Age of Victim

Stress Factors
City Living
Children
Victim Employed
Household Income Level
Relationship Type- Married

Exposure to Social Controls
Report to Police
Report to Victim Agency
Others Present During Incident
Incident Occurred:
- Home
- Working
- Leisure Activity
- Outdoors

Number of Times Moved
Offender Arrested

Level of Violence
Injury Suffered
Work Loss
Weapon Used
Gun Used
One Offender
Victim Resistance

Individual Attributes
Race of Victim
- Black
- Hispanic

Gender-Female
Use of Alcohol / Drugs by Offender
Relationship Type- Married
Age of Victim
Victim Resistance

Survey Controls / Gag Control
Interview Conditions
- Telephone
- Respondent Alone

Risk Period

Table 2b. Variable Predictions

Variable	Predicted Relationship **to Chronic** Violence
Economics Barriers	
Household Income	-
Unemployed (victim)	+
Education (victim)	-
Children	+
Report to Police	-
Age (victim)	-
Stressful Conditions	
City Living	+
Employed (victim)	+
Household Income	-
Relationship Type- Married	+
Children	+
Level of Violence	
Injury	-
Work-loss	-
Weapon	-
Gun	-
One Offender	+
Resistance	-
Exposure to Social Controls	
Others Present	-
Outdoors	-
Report to Police	-
Report to Victim Agency	-
Home (incident location)	+
Leisure (incident location)	-
Work (incident location)	-
Number of Times Moved	+
Individual Attributes	
Race of Victim	
Black	+
Hispanic	+
Gender-Female	+
Resistance	-
Alcohol or Drug Abuse	+
Relationship Type-Married	+
Age of Victim	-
Gag Effect	
Type of Interview-Telephone	-
Interview- Respondent Alone	+
Period of Risk	+

Table 3. Partner Assaults, N=628

Variable	Full Model	no risk correction	no gag control	no gag or risk correction
Victim's Age	-.011 coefficient .381 significance	-.009 .460	-.010 .450	-.008 .486
Children	.172 .520	.013 .960	.020 .939	-.102 .670
Victim Employed	-.099 .673	-.110 .620	-.077 .741	-.104 .641
Education	-.136 .559	-.184 .409	-.160 .490	-.193 .385
Household Income	-.084 .010***	-.065 .031**	-.087 .005***	-.064 .028**
Report to Agency	.462 .097*	.500 .057*	.464 .093*	.485 .063*
Leisure Activity	-.419 .325	-.229 .569	-.425 .314	-.248 .537
Home Activity	.166 .605	.362 .226	.120 .705	.325 .274
Work Activity	.585 .256	.872 .076*	.568 .266	.850 .083
Moved	.062 .117	.057 .117	.050 .187	.044 .217
Others Present	-.240 .304	-.282 .207	-.273 .241	-.309 .164
Outdoor Location	-.067 .810	.022 .935	-.129 .639	-.038 .884
Black Victim	-.517 .159	-.524 .131	-.481 .187	-.505 .144
Hispanic Victim	-.006 .987	.079 .835	.034 .932	.100 .790
Victim's Gender	.494 .142	.410 .196	.523 .118	.438 .166
Resistance	.215 .421	.124 .624	.163 .537	.071 .778
Alcohol/Drugs	.237 .299	.268 .217	.265 .242	.287 .182
Married	-.168 .605	-.246 .426	-.178 .583	-.237 .440
City Living	-.032 .895	.030 .896	-.020 .934	.030 .897
Injured	.090 .705	.078 .728	.092 .699	.076 .734
Work Loss	-.614 .276	-.467 .381	-.578 .299	-.476 .371
Weapon Used	-.012 .972	.021 .949	-.061 .860	-.018 .955
Gun Used	-.555 .412	-.280 .663	-.524 .434	-.259 .686
One Offender	-.417 .501	-.328 .579	-.459 .458	-.356 .546
Spouse	.174 .548	.181 .506	.135 .639	.151 .576
Ex-spouse	.645 .039**	.738 .013**	.608 .051*	.721 .015**
Reported to Police	-.465 .060*	-.560 .018**	-.509 .038**	-.588 .012**
Offender Arrested	-.545 .097*	-.478 .127	-.520 .110	-.483 .122
Respondent Only Interview	.745 .030**	.555 .085*		

Telephone Interview	.311 .284	.342 .216		
Period of Risk	.423 .000***		.414 .000***	
Chi-Square =	110.41	54.66	105.66	51.52
df =	31	30	29	28
p =	0.000***	0.004***	0.000***	0.0044***
Pseudo-R^2 =	16.1%	8.3%	15.5%	7.9%
Pseudo-R^2 =	24.6%	12.7%	23.6%	12.0%

* significant at < 0.1; ** significant at < 0.05; *** significant at < **0.01**

Table 4. Partner Assault Cases, N=628

Variable	Full Spouse Model	No Reporting	No Reporting or Gag Control	No Reporting or Risk Correction	No Reporting, Gag or Risk Correction
Victim's Age	-.011 coefficient .381 significance	-.012 .352	-.010 .420	-.009 .433	-.009 .460
Children	.172 .520	.151 .570	-.010 .968	-.025 .919	-.145 .541
Victim Employed	-.099 .673	-.116 .620	-.095 .683	-.130 .557	-.125 .570
Education	-.136 .559	-.014 .554	-.165 .476	-.190 .393	-.202 .362
Household Income	-.084 .010***	-.080 .013**	-.082 .009***	-.060 .047**	-.056 .051*
Report to Agency	.462 .097*	.416 .132	.411 .134	.428 .099*	.408 .114
Leisure Activity	-.419 .325	-.387 .362	-.389 .354	-.199 .618	-.219 .583
Home Activity	.166 .605	.196 .540	.143 .649	.384 .198	.341 .249
Work Activity	.585 .256	.622 .225	.605 .233	.896 .067*	.868 .074*
Moved	.062 .117	.065 .103	.051 .181	.061 .091*	.046 .193
Others Present	-.240 .304	-.288 .214	-.331 .149	-.336 .129	-.373 .089*
Outdoor Location	-.067 .810	-.060 .829	-.128 .641	.027 .920	-.039 .880
Black Victim	-.517 .159	-.597 .101	-.565 .117	-.606 .077*	-.589 .085*
Hispanic Victim	-.006 .987	-.051 .899	-.015 .970	.039 .918	.057 .880
Victim's Gender	.494 .142	.489 .144	.520 .119	.382 .225	.413 .189
Resistance	.215 .421	.187 .480	.127 .626	.100 .690	.041 .870
Alcohol/Drugs	.237 .299	.215 .343	.243 .280	.237 .269	.256 .230
Married	-.168 .605	-.162 .617	-.164 .611	-.247 .421	-.228 .454
City Living	-.032 .895	-.034 .888	-.025 .918	.026 .910	.019 .934
Injured	.090 .705	.093 .696	.091 .699	.058 .796	.053 .811
Work Loss	-.614 .276	-.709 .210	-.680 .221	-.570 .286	-.589 .268
Weapon Used	-.012 .972	-.000 .999	-.052 .879	.037 .910	-.006 .985
Gun Used	-.555 .412	-.572 .397	-.535 .422	-.326 .612	-.297 .642
One Offender	-.417 .501	-.327 .595	-.365 .551	-.204 .731	-.228 .698
Spouse	.174 .548	.165 .565	.121 .670	.176 .515	.138 .605

Ex-spouse	.645 .039**	.606 .051*	.564 .067*	.692 .019**	.672 .021**
Reported to Police	-.465 .060*				
Offender Arrested	-.545 .097*	-.778 .010***	-.772 .010***	-.751 .009***	-.771 .007***
Respondent Only Interview	.745 .030**	.797 .019**		.594 .063*	
Telephone Interview	.311 .284	.362 .207		.405 .139	
Period of Risk	.423 .000***	.430 .000***	.421 .000***		
Chi-Square =	110.41	106.84	101.30	48.91	45.06
df =	31	30	28	29	27
p =	0.000***	0.000***	0.000***	0.012**	0.016**
Pseudo-R^2 =	16.1%	15.6%	14.9%	7.5%	6.9%
Pseudo-R^2 =	24.6%	23.8%	22.7%	11.4%	10.5%

'significant at < 0.1; ** significant at < 0.05; *** significant at < 0.01

Table 5. Spouse Assault Cases Only, N=218

Variable	Full Spouse Model	No Risk Correction	No Gag Control	No Gag or Risk Correction
Victim's Age	-.071 coefficient .017** significance	-.055 .030**	-.056 .048**	-.043 .076*
Children	.239 .664	.299 .570	.031 .953	.117 .815
Victim Employed	-.502 .277	-.568 .179	-.416 .353	-.500 .226
Education	.075 .862	.079 .847	.092 .828	.107 .791
Household Income	-.100 .133	-.070 .239	-.123 .049**	-.091 .106
Report to Agency	1.100 .027	1.024 .027	.986 .042	.940 .038
Leisure Activity	-.923 .390	-.563 .584	-.746 .466	-.494 .623
Home Activity	.343 .570	.514 .380	.424 .470	.569 .319
Work Activity	-1.245 .304	-.723 .528	-.980 .393	-.530 .637
Moved	.090 .255	.126 .079*	.097 .194	.122 .073*
Others Present	-.163 .738	-.096 .835	-.100 .836	-.014 .975
Outdoor Location	.214 .741	.186 .767	.140 .824	.099 .873
Black Victim	.120 .885	.201 .791	.275 .726	.196 .789
Hispanic Victim	-.283 .675	-.047 .942	-.230 .726	-.006 .992
Victim's Gender	1.127 .133	.869 .196	1.173 .102	.939 .149
Resistance	-.160 .728	-.111 .798	-.176 .701	-.163 .701
Alcohol/Drugs	.143 .759	.326 .442	.097 .831	.315 .447
Married	.029 .954	-.080 .861	-.083 .864	-.137 .756
City Living	-.298 .547	-.238 .607	-.182 .704	-.085 .849
Injured	-.443 .347	-.236 .588	-.274 .544	-.118 .778
Work Loss	.681 .481	.283 .742	.945 .291	.508 .536
Weapon Used	-.126 .856	.130 .830	-.301 .650	-.054 .926
Gun Used	-.388 .812	.108 .940	-.373 .815	.154 .913
One Offender	-3.007 .091*	-1.687 .321	-2.817 .112	-1.488 .378
Reported to Police	-1.690 .001***	-1.760 .001***	-1.707 .001***	-1.714 .001***

Respondent Only Interview	1.375 .043**	1.256 .047**		
Telephone Interview	-.015 .978	.058 .913		
Period of Risk	.466 .000***		.452 .000***	
Offender Arrested	-.304 .671	-.344 .603	-.162 .814	-.290 .653
Chi-Square =	61.98[1]	43.97	56.63	38.99
df =	29	28	27	26
p =	0.0003***	0.028**	0.000***	0.0489**
Pseudo-R^2 =	24.7%	18.3%	22.9%	16.4%
Pseudo-R^2 =	38.0%	28.0%	35.1%	25.1%

* significant at < 0.1; ** significant at < 0.05; *** significant at < 0.01

Table 6. Spouse Assault Cases Only, N=218

Variable	Full Spouse Model	No Reporting	No Reporting or Gag Control	No Reporting or Risk Correction	No Reporting, Gag or Risk Correction
Victim's Age	-.071 coefficient .017**significance	-.056 .050**	-.041 .129	-.038 .116	-.029 .225
Children	.239 .664	.067 .898	-.164 .742	.018 .970	-.144 .755
Victim Employed	-.502 .277	-.509 .245	-.464 .276	-.606 .134	-.553 .162
Education	.075 .862	.060 .885	.085 .833	.045 .909	.054 .888
Household Income	-.100 .133	-.068 .283	-.078 .187	-.036 .522	-.047 .372
Report to Agency	1.100 .027**	.915 .053*	.785 .085*	.804 .064*	.723 .089*
Leisure Activity	-.923 .390	-1.006 .344	-.761 .447	-.722 .468	-.606 .531
Home Activity	.343 .570	.378 .518	.436 .442	.423 .445	.474 .383
Work Activity	-1.245 .304	-.700 .558	-.415 .710	-.173 .874	-.033 .976
Moved	.090 .255	.090 .236	.085 .230	.122 .080*	.110 .098*
Others Present	-.163 .738	-.372 .423	-.393 .384	-.342 .436	-.337 .431
Outdoor Location	.214 .741	.316 .604	.204 .730	.249 .671	.160 .781
Black Victim	.120 .885	-.128 .867	.030 .968	-.026 .970	-.001 .999
Hispanic Victim	-.283 .675	-.200 .754	-.132 .833	.107 .860	.137 .818
Victim's Gender	1.127 .133	.752 .302	.812 .248	.492 .438	.570 .358
Resistance	-.160 .728	-.168 .704	-.193 .660	-.130 .753	-.194 .633
Alcohol/Drugs	.143 .759	.012 .979	-.052 .902	.220 .576	.192 .619
Married	.029 .954	-.133 .776	-.223 .622	-.265 .527	-.293 .476
City Living	-.298 .547	-.234 .621	-.118 .793	-.275 .535	-.128 .761
Injured	-.443 .347	-.417 .342	-.272 .522	-.292 .477	-.190 .633
Work Loss	.681 .481	.506 .597	.660 .455	.159 .853	.321 .693
Weapon Used	-.126 .856	.012 .985	-.148 .811	.258 .648	.072 .895
Gun Used	-.388 .812	-.055 .971	.061 .968	.195 .885	.257 .845
One Offender	-3.007 .091*	-2.116 .229	-1.846 .287	-.664 .689	-.538 .744
Reported to Police	-1.690 .001***				

Respondent Only Interview	1.375 .043**	1.559 .018**		1.268 .035**	
Telephone Interview	-.015 .978	.321 .540		.298 .547	
Period of Risk	.466 .000***	.478 .000***	.450 .000***		
Offender Arrested	-.304 .671	-1.170 .061*	-.965 .106	-1.219 .034**	-1.113 .048**
Chi-Square =	61.98	50.35	44.30	29.86	25.16
df =	29	28	26	27	25
p =	0.0003***	0.0059***	0.014***	0.321	0.454
Pseudo-R^2 =	24.7%	20.6%	18.4%	12.8%	10.9%
Pseudo-R^2 =	38.0%	31.7%	28.2%	19.6%	16.7%

*significant at < 0.1; ** significant at < 0.05; *** significant at < 0.01

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Chapter Six

Summary and Conclusion

Repeated Burglary in the NCVS

The analysis of repeated burglary victimization examined the risk of burglary in housing units over a three year period in the NCVS. The victimization experience of persons in these units was assessed at six-month intervals. The dependent variable in the analysis was the number of burglary victimizations experienced in a six month period. This variable was included in mixed random effects model with other attributes of the housing unit and the households that occupied the unit. Some of these characteristics were relatively fixed such as the race of the household head, family income, whether the housing unit was a single family or a multiple unit dwelling, marital status of the household head, and whether the housing unit was in the central city of an SMSA or elsewhere. In addition to these relatively fixed characteristics of the housing unit and its occupants, the model also included a number of other attributes that were more likely to vary over time. One of these time-varying characteristics was household composition, i.e. whether the housing unit lost members or gained members since the last time it was interviewed. The other was the number of burglaries experienced by the household in the previous interview. Finally, a number of variables known to be related to response errors in the survey were included in the model to take account of these errors.

Prior burglary victimization is the most crucial variable for testing the predictive power

of prior burglary victimization for subsequent victimization. If this lagged burglary variable is the best predictor of subsequent burglary victimization then the premise underlying much of the work done in England would be supported. If prior burglary has little predictive power , then we must question whether prior burglary victimization is the best predictor of subsequent victimization.

The other variables in the model are important in that they can also tell us whether they can be useful in predicting subsequent burglary risk. These variables also help in "holding other things constant" so that we can be sure that the relationship between prior and subsequent burglary is not due to these other factors. This is more important for building theories of victimization than it is for allocating police resources. The inclusion of other time-varying variables is particularly important because cross-sectional surveys have not been able to assess the effects of these variables on burglary risk.

The results of this analysis confirm that prior burglary victimization is positively related to subsequent burglary victimization, but other attributes of housing units and their occupants are much stronger predictors of burglary risk than prior burglary victimization. Age of the household head, location of the housing unit, whether the household head is married are much better predictors of burglary and other attributes such as changes in household composition and size of the household are about as discriminating as prior victimization.

These findings suggest that there are predictors other than prior burglary victimization that may be better to use in guiding resource allocation. They suggest further that explanations of burglary that rely upon "entering into the victimization state at time one" may not be that useful in understanding the risk of burglary.

We must be cautious with these results especially in light of the enormous body of good work that has been done in England on this topic. More work of this type must be done to ensure that these results are accurate and broadly applicable. It could be that English burglars have different offending patterns than those in the U.S.. More likely it is the high selective nature of police data relative to victim survey data that produces these differences. Police data over represent repeated victims and thereby the contribution of "state dependence" to explanations of burglary victimization.

If this is the case, that police data are highly selective, is it necessarily bad to use prior burglary to build theory and to allocate resources? For theory building this selectivity would be fatal. A relatively small and atypical subset of the burglary victim population should not be used to understand the population of burglary victimization. It is not clear that selectivity is that consequential for allocation. For these purposes anything that distinguishes low from high risk persons is useful regardless of whether the predictive trait "causes" the subsequent victimization. The preoccupation with prior victimization as a predictor can be damaging however in several ways. The selectivity of police data focuses prevention resources on a restricted subset of potential victims and unintentionally ignores all others. Allocation models based on other sources of data that are less selective than police data would include more of the at risk population. It would be fairer. Second, the focus on prior victimization as a predictor of risk can inhibit the search for better predictors. The data on prior police reported victimization are conveniently available to the police. It is very tempting to stay with these familiar but flawed data rather than begin to explore or create other data sources that may provide better methods for resource allocation as well as better data with which to test theories.

Repeat Assaults in the NCVS

Repeated assaults are different from repeated burglary and they need not be investigated the same way. In the case of burglary, the victim often knows very little about the offense and the offender and so it is not very useful to study repeat burglary by asking the victim to associate the events or to indicate how these events may be inter-related. The best way to proceed is to find regularities across repeated burglary events and formulate hypotheses as to why they occurred (Polvi et al., 1991; Forrester et al., 1988). In the case of assault the victim knows much more about the offender and the event and it is useful to ask the respondent why these repeated assaults occurred or to ask the victim about commonalities across these events. This can shorten the search for patterns and guide our interpretation of same. Unfortunately, most available victimization surveys did not, until very recently, include such questioning.

In 1992 changes in the way that the NCVS handle "series incidents" provided some limited but very valuable questioning about how high volume repeated victimizations may be related. Specifically the new questions elicited whether the repeated events involved the same offender or different offenders, whether they occurred in the same place or in different places and whether the situation had end or continued. This information can be used in concert with other information to suggest whether and how repeated events may be related. If we know that events share the same offender, for example, "state dependence" explanations become more plausible.

Repeat assaults cannot be studied as an undifferentiated mass because it is clear from the limited descriptive information that we have that there are very different subclasses of assault. The analyses conducted on these subclasses confirms that point. The factors that distinguish

between one-time and repeat victims of domestic assault are very different from those that distinguish one time and repeat victims at work.

The descriptive work done by Dodge (19843987) as well as the analyses done here suggest that our efforts to understand the source of repeat assault should focus on three domains or institutional arrangements--work, school and domestic violence. It is in these settings where the bulk of high volume repeat assaults occur, so it must be something in these settings that promotes repeat assaults.

We examined the unity of persons and places within these settings to determine if there might be sub types within these three classes that could be usefully distinguished. Our assumption was that repeat victimizations that involved the same offender over time would be quite different from those that did not. Those that involved the same victim and offender may be more "personally motivated than those that did not. In these incidents, the victim was singled out for who they were not because they were an attractive target based on universalistic standards.

Using this standard, there appeared to be two modal types of repeat assault at school. Approximately one half of the series incidents at school involved the same offender and one half did not. This suggests that a substantial portion of the high volume repeat assaults at schools may be personal vendettas while another substantial portion are less personal violence. This latter type of victimization could be the result of racial or ethnic conflict or gang activity.

There is also some evidence that repeat assaults at home could be usefully divided into those that involve intimate partners and those that involve other familiars such as siblings or parents and children. Moreover, there is some evidence presented here that high volume repeat victimization involving intimate partners is different from that between spouses.

The evidence for different subclasses of repeat assaults at work is less clear. The vast majority of offenders are not known to the victim and are not the same across events. This is less the case for occupations that are less concerned with order maintenance, e.g. transportation workers or retail sales persons. There is a small but noticeable proportion of the repeat assaults at work that involve intimate partners. These are less assaults on the job than they are domestic assaults that simply take place on the job.

The fact that repeat assault clusters in schools, intimate relationships and work suggested that we focus more analytical investigations on these domains or institutional arrangements. In so doing, we picked the two most populous classes of repeat assaults--those at work and those between intimates--and compared repeat assault victims with those victimized only once in the same period. Following the work done on domestic violence, our analyses of repeat assault victimization explored the issue of persistence. The difference between the singly victimized and the repeatedly victimized in a given unit of time is persistence. For some reason, persons who are repeatedly victimized are stuck in or remain in dangerous settings to be victimized again. Why?

The answer to this question is different for intimate violence than it is for high volume repeat assaults at work. The single best predictor of whether assaults among intimates become chronic is whether they are reported to the police. Episodic violence between intimates does not become chronic when the event is reported to the police. The relationship between chronic victimization and arrest is not as robust, but it too is negative. Arrests are more prevalent among the episodic assaults that it was among the chronic repeat assaults. These results compliment the findings of the experiments done to test the effects of arrest on duration (Sherman et a1....) .

While these experiments examined the effects of arrest on persistence in the population of events reported to the police, these data address the effects of reporting to the police in the wider population intimate assaults, including those not reported to the police. These data suggest that in this wider population reporting to the police, independent of the action that the police take when they get there, has a negative effect on persistence. This is true for the partner violence and for violence between spouses, i.e. those who are formally married. All of this suggests that efforts should be taken to promote the reporting of partner violence as a means of fostering desistance.

It is interesting to note that arrest has a significant negative effect on persistence of intimate violence in the wider population of partner assaults but not when the focus is restricted to violence among spouses. This warrants further inspection. It may be that arrest works among partners because the relationship is terminated after the arrest, while the relationship among married folks continues and so do the assaults.

In the case of high volume repeat assaults at work, involving third parties such as the police has little effect on the termination of the assaults. Occupations that involve order maintenance functions are more at risk of repeat assaults than those that do not. Persons in these roles can protect themselves by performing the job at times, i.e. daytime rather than night time, and at places, i.e. central city versus elsewhere, where it may be less dangerous. If the assaults become serious it appears that steps are taken to end them, but it is not clear what these steps are. It is clear from these analyses that whatever intervention is taken, it cannot be offender oriented. It must situational. This is the case because the events at work seldom involve the same offender across all events. Hence any intervention based upon specific deterrence is unlikely to

be effective. This would explain why reporting to the police does not discriminate between episodic and persistent assaults at work. The type of situational intervention could include having persons work in teams or having those in order maintenance roles avoid confrontation until they have overwhelmingly superior force so as to discourage assaults.

What more do we need to know?

With respect to burglary, we must confirm the relatively limited utility of prior burglary victimization as a predictor of subsequent burglary. This can be done with additional analyses of victim survey data in which special attention is given to the use of shorter periods between incidents and separate models are estimated for events reported to the police and events not reported to the police. The analyses described here use the number of burglaries in a sixth month period to predict the number that will occur in the next sixth month period. Analyses of police data suggest that repeat burglaries happen very quickly after the first incident. If this is the case then the current analysis may under estimate the predictive power of prior victimization. These analyses should be repeated with shorter reference periods, say burglary in one month predicting burglary in the next month. If the predictive power of prior burglary remains weak in these models then models should be run separately for burglaries reported to the police and for burglaries not reported to the police. If the selectivity of police data accounts for the greater predictive power of prior burglary in police data then the predictive power of prior burglary should be greater in the model using cases reported to the police relative to cases not reported to the police.

If these studies confirm that prior burglary is a poor predictor of subsequent burglary, then serious efforts to predict burglary should be under taken using victim survey data. These data could be collected on a jurisdiction basis and not in omnibus surveys like the NCVS. They could include much more specific information on housing units and their occupants as well as some information on areas. Since more and more local police departments are using victim surveys as part of the move to community policing, these surveys could be used to model burglary risk. Ideally this research could be conducted within the context of a partnership between a local university or research institute and a local police department. The department could sponsor and administer the survey while the researchers engaged in an iterative process of survey development, data collection, modeling, and further development. This process could lead to a method of resource allocation that is better than and more equitable than using prior burglary victimization.

Our understanding of repeat assaults would benefit from a more systematic questioning of victims about the inter-relationship among events repeated events. At present surveys like the NCVS and the BCS ask about the relationship of events only for very high volume repeat victimization. These types of questions should be asked whenever a respondent reports multiple events. This would permit the investigation of the full range of repeat victimization and not simply the very high volume repeat victimization. The questioning about the inter-relationship among events should be more extensive. It should include direct questions about perceived motive.

We should take advantage of the fact that repeat assaults seem to take place in specific domains. First we should confirm that the clustering by domain that we see among high volume

repeat victimizations adheres throughout the entire range of repeat victimization. Second, we should engage in more focused studies of each domain following the leads suggested by the foregoing analyses. In the work domain, for example, we would want to have detailed information about the occupational role. So for law enforcement officers we would want to know what the nature of tfieir specific assignment was, e.g. vice versus patrol. We would want to more about the specific environment in which they do their work. Do they work in dangerous of safe places? Do they work alone or with co-workers? In school, we would want to know about the roots of those events that seem to be vendettas and to ask specifically about whether more impersonal assaults are taking place in the context of gang activity and racial or ethnic strife. Finally, we would want true longitudinal data from a self report survey so that we could begin to separate the "state dependence" from "heterogeneity". This type of information would permit the actual measurement of persistence rather than approximating persistence as we have done here.