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**THE TIME-WINDOW EFFECT IN THE
MEASUREMENT OF REPEAT
VICTIMIZATION:
A Methodology for its Examination,
and an Empirical Study**

by

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Abstract: *Crime control strategies and criminological theory have been increasingly informed by developments in the study of repeat victimization in recent years. As a consequence, the measurement of repeat victimization is an important issue. The outcome of the measurement of repeat victimization can influence the manner in which police and other agencies develop, implement and evaluate crime control efforts. In addition, the results of measurement can influence the theories and explanations that derive from empirical study. Among the several measurement issues that have been identified to date in relation to repeat victimization, a key issue is that of the "time-window effect." The term refers to the fact that the length of the period of observation directly affects the proportion of repeat victimization that is "captured." The present study has two key aims. First, it presents a method to measure the size of the time-window effect. Second, it tests this method em-*

pirically with residential burglary data from three cities. The implications for criminological research and crime control practice are then discussed.

INTRODUCTION

While the phenomenon of repeat victimization has long been recognized, it is only in the decade and a half since the publication of the Kirkholt project in Britain in the late 1980s (Forrester et al., 1989, 1990) that its significant implications for crime control and criminological study have been developed. This renewed interest in repeat victimization has led to a growing exploration of the implications for theories of crime, the understanding of crime and offending patterns in different areas, and the development of policy implications for agencies, including the police and victims services. There are several recent reviews of repeat victimization (Davis et al., 1997; Friedman and Tucker, 1997; Pease and Laycock, 1996; Pease, 1998). There is variation in the extent to which repeat victimization has been incorporated into crime control practice and crime research: a recent national-level review of policing practices in the U.K. observed that all police forces in that country had a policy on repeat victimization and were beginning to integrate that work into routine policing practice (Farrell et al., 2000). The study of repeat victimization is rapidly gaining ground in Australia (Criminal Justice Commission, 1997; Mukherjee and Carcach, 1998; Townsley et al., 2000; Morgan, 2001), in the Netherlands (Hakkert and Oppenhuys, 1998; Kleemans, 2001), and in relation to cross-national comparative study (van Dijk, 2001; Farrell and Bouloukos, 2001).

Despite significant developments in the understanding of repeat victimization and its prevention, many questions remain unanswered. Among these are methodological issues related to measurement, including what has been termed the "time-window effect." This issue is addressed here using police data relating to residential burglaries occurring in three large cities. The "time-window effect" is the term used to denote the fact that the proportion of crimes that appear to occur against the same targets will change with the length of the period during which crime is observed. The issue has received little study since it was outlined in 1993 in the following terms:

A study of crime in an area for a one week period will show virtually no repeat victimization. This is because crimes which

are noted during the observed week may be repeats of crime the week before, or may be precursors of crimes in the subsequent week. Even though some of the crimes are in fact repeat crimes or linked to future repeat crimes, by research of this kind they are observed as "single-incident" crimes. Even if there are only six days between one incident and the next, only those where the prior crime took place on the first day of the study would have the repeat recorded as such.

Repeat victimization is therefore under-counted, and single-incident crimes are over-counted. The extent of the problem of the "time-window" of the research is proportional to the length of the period of observation. A study with a long reporting or recording period — perhaps several years of crime with dates and times of occurrence — will have virtually excluded this problem. A study with a very short time period has this problem acutely. In the study of school burglary and property crime mentioned [elsewhere in the monograph], this problem was tackled using a simple weighting formula to account for the under-estimate (Farrell and Pease 1993:19).

Despite this recognition of the time-window effect, it has received little theoretical or empirical attention. Yet two examples should demonstrate its importance and give justification to the present study. The first example relates to victim surveys. Enormous insight into crime and victimization has been gained in recent decades from crime victim surveys of the local, national and international variety. Typically, such surveys utilize a one-year reference period over which victimization experiences are measured. But what if this is not the optimal period in which to measure repeat victimization? The effect could be that, since it is already evident that repeat victimization plays such an important role in the make-up of crime, our current understanding of crime victimization patterns is significantly distorted.

The second example relates to the development, implementation and evaluation of local crime prevention projects by the police and other agencies. The authors have encountered many instances where practitioners have concluded that "There is no repeat victimization in my data," or that "Repeat victimization does not occur in my area." In some instances this may be true (although we have yet to encounter this to any significant extent), but in others it may be because many local crime audits utilize a data set that covers only a short period of time, to the extent that repeat victimization is virtually excluded. Our colleague Ken Pease tells of an instance when a police department

“suddenly found” a significant level of repeat victimization in their crime data where none had previously existed — the day before he was due to visit them. The anecdote embodies the general notion that more than a cursory examination of data is necessary to reveal the rate of repeat victimization. Issues in the measurement of repeat victimization will influence the manner in which crime prevention efforts are developed, implemented and evaluated, at the local, national and international levels. It is not inconceivable that, in the longer term, these measurement issues could influence the rate of initiation of efforts to prevent repeat victimization. Hence, while the present study is primarily methodological in nature, it is evident that it has implications that are directly related to, and potentially significant for, policing and crime prevention practice and, more broadly speaking, for the manner in which crime is studied.

DATA AND METHOD

The present study uses data relating to residential burglaries reported to the police in Baltimore, Dallas and San Diego. These three large cities were selected in part because they had computer information systems that utilized a “justified” address field or an address “lookup table.” This meant that the typing and spelling errors typical of many police data sets should not be encountered since each new burgled address entered into each system is automatically cross-referenced with a list of known street names for the city. This is a separate methodological issue that has been identified, but will be briefly rehearsed here. If, for example, the property at 23 Washington Place is burgled and entered in the computer as “23 Washington Place,” then burgled again and entered as “23 WASHINGTON PL.” (that is, using upper rather than lower case, and/or the truncation of the street name), analysis of the aggregate data may not recognize these burglaries as occurring at the same address. We therefore sought to minimize this possible source of methodological error from the outset. Data were collected from each city for a three-year period, producing data sets of between 20,000 and 40,000 burglary incidents for each.

This paper has two aspects. The first is a methodology for measuring the time window. The second is a case study in the use of that methodology. This statement of the nature of the methodology is important since it may prove that empirical studies using different data sources and crime types produce different findings. The development and utilization of a standardized methodology however, should allow knowledge in this area to develop with transparency. The methodology for the measurement of the time-window effect is conceptually

simple: we compared rates of repeat victimization captured using data for different periods of time. More specifically, we split each three-year data set into 36 one-month data sets, and measured changes in the rate of repeat victimization as increasingly longer time windows were used. First we measured the proportion of repeats found in one month's data, then that found in two months' of data etc., up to 36 months' worth. Although conceptually simple, translating the concept into practice was somewhat more laborious. It involved writing algorithms to produce 36 separate analytic runs for each of the data sets, prior to the transformation and analysis of the results.

FINDINGS

As the work progressed, not only was the importance of using a longer period of data apparent, but the monthly incremental or marginal effect could be measured. The extent of repeat burglaries measured in only a few months' data was typically small and increased gradually, with similar patterns for each city. The findings are presented in full below, but two of the key findings were that, on average across the three cities:

- (1) A one-year time window captures 42% more repeats than a six-month window;
- (2) A three-year window captures 57% more repeats than a one-year window.

The percentage of burglaries observed to be repeats for time-windows of different numbers of months, for each city, is shown in Table 1, indexed to 100 at 12 months. Twelve months is probably the most commonly used and understood measurement period, since crime rates are typically annual, whether from victim surveys or other sources. The table shows that, for the present data sets, when only one month's worth of data are used, repeat burglaries constituted 33%, 38% and 38%, in Baltimore, Dallas and San Diego, respectively, of the number of repeats that were captured in that city's one-year study. When three years or 36 months of data were examined, it was found that repeat burglaries constituted 162%, 146% and 163% percent of the total city burglaries in comparison to a one-year time window.

Table 1: Volume of Repeat Burglaries Observed Using Different Time Windows, by City (Indexed to 100 at 12 months)

Months	Indexed to 100 at 12 months		
	Baltimore	Dallas	San Diego
1	32.5	38.1	37.5
2	38.6	47.2	37.0
3	43.1	56.9	49.4
4	54.3	63.5	54.9
5	62.2	67.4	62.4
6	69.6	72.7	69.0
7	75.4	76.7	74.0
8	81.3	81.1	80.3
9	87.5	86.1	85.2
10	91.0	90.9	92.0
11	95.3	94.6	95.1
12	100.0	100.0	100.0
13	103.6	102.8	105.4
14	107.4	106.5	108.4
15	110.0	109.3	112.5
16	113.9	112.6	115.3
17	116.8	116.2	119.9
18	119.0	117.7	122.6
19	121.3	119.7	125.5
20	121.4	121.4	128.8
21	124.7	122.6	133.6
22	127.3	123.8	137.0
23	130.2	125.2	138.8
24	133.2	127.3	140.6
25	135.5	128.7	142.0
26	137.7	130.1	143.8
27	140.5	132.0	145.7
28	143.1	133.7	149.0
29	146.5	135.8	151.0
30	149.6	137.3	152.3
31	151.5	138.8	153.1
32	153.8	140.5	155.7
33	156.0	142.0	157.0
34	158.1	143.0	158.0
35	159.4	144.5	160.7
36	161.5	146.1	163.2

The results are more visually compelling when presented graphically. Whereas marginal revenue curves are common in the study of Economics, Figure 1 shows what are effectively marginal capture curves for the measurement of repeat burglary. The shape of the curve for each city reflects the marginal change in the rate of repeat burglaries captured for different time periods of study. The curves are clearly non-linear, demonstrating a diminishing marginal increase in the rate of repeats captured after a period of approximately 18 months. What proved quite surprising to the authors was that the shape of the curve was perhaps less pronounced than expected. While the increase in the rate of repeats begins to slow after around 18 months, it does not slow quickly, and the curve is still rising quite steeply after three years. Simple extrapolation from the curves suggests that even more repeats would be captured if even longer periods of data were available. It is possible that this is a factor inherent to the present data sets that may not prove generally replicable, and is a point returned to later as part of the discussion.

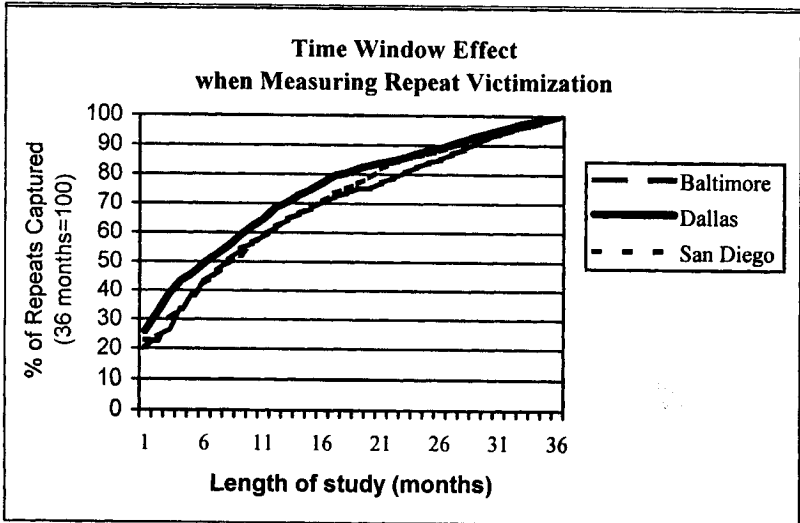
DISCUSSION

Issues are raised both by the general and specific shape of the time window effect in Figure 1. The amount of repeat burglaries captured increases rapidly at first, then becomes less as the time window expands. These diminishing marginal returns are as might be expected for two reasons. The first is that the cut-off effect of the study period at either end (failing to measure precursors and subsequent burglaries related to ones within the study period) will have less overall significance. They tend to produce an absolute rather than a relative effect. The second is that because repeats are known to occur quickly after victimization, then, after an optimal study period or time window has been reached, the overall effect of this phenomenon will diminish.

It is possible that the shape of the curves in Figure 1 may be steeper at the three-year point than those which future studies will show. This may occur since some of our present data combines residential burglaries from individual dwellings with that from multiple-dwelling apartment complexes where burglaries in individual apartments are grouped together at the same address. We propose that distinguishing between these different categories of residential burglary in future work may produce a more rapid decrease in the slope of the curves. This is for future replications to determine. For the present purposes, we conclude that while the specific effect of the time window may be slightly overstated, its general shape and nature (and hence the implications of the study) should be correct — despite

the acknowledged limitation of the data sources. Future studies may find that the time-window effect tails off more quickly if apartment-specific data contain repeats that occur more quickly on average than those in the present data sets.

Figure 1



A preliminary interpretation of the findings can be attempted in terms of event dependency and risk heterogeneity. Event dependency is when one residential burglary leads to another at the same dwelling. This may be because the offender realized it was a suitable target, informed associates of such, or because something about the dwelling changed to make it visually more attractive to other offenders (such as a broken lock not having been replaced). Risk heterogeneity in this context refers to when the same dwelling is more attractive in comparison to others and remains so, attracting different burglars to commit otherwise unrelated burglaries. Event-dependency is the most compelling explanation of the fact that many repeats occur within a short period of time of the first crime. Crimes occurring for reasons related to risk heterogeneity would be more randomly distributed in time since they are unrelated. The time-window effect in

the present study seems to lend support to both explanations. The time-window effect is greatest at first, concurrent with an event-dependent explanation of repeat burglary. However, the longevity of the time-window effect, although at a lower level, is concurrent with risk-heterogeneous explanations. As mentioned previously, the fact that some multiple-dwelling addresses are included in the data sets under the same street number may slightly influence these findings. Recording such burglaries as repeats would tend to overstate risk heterogeneity since burglaries at these separate dwellings would be more likely to have a more random distribution in time.

Although we have utilized data for three cities by means of demonstrating a general applicability of the findings, we propose that the present work needs replicating on different data sets and for different crime types before we can make wholesale generalizations. A call for replication of a study and recognition of its limitations is standard practice, however. It should not let us preclude the possibility of discussing possible implications for research, policy and crime prevention practice based upon the expectation that the findings will for the most part prove to be robust.

The present findings make a strong case that the rates of repeat victimization uncovered by crime surveys are gross underestimates. The typical one-year time-window of crime surveys may measure at least 50% less repeat victimization than actually occurs. If this is true, our understanding of crime patterns in general may be significantly skewed.

We would not expect that analyzing crime surveys that ask questions with a longer recall period than one year, such as the International Crime Victims Survey, will resolve the issue of the time window. This is because the memory-decay effect of interviewees when asked about crime occurring several years ago will almost certainly produce disproportionately low rates of repeat victimization. It is possible that the U.S. National Crime Victims Survey may shed some light upon these issues through the analysis of a three-year recall period data set compiled from repeated panel surveys. Such a data set was recently constructed by Brian Wiersema and Richard Titus (see Titus, 1997). However, though police crime records embody problems due to underreporting of crime, they are independent of memory problems, and as such may be preferable for the present purposes.

The present findings suggest the possibility that local crime audits, or any crime analysis that uses data covering only a short period of time such as a few months, may produce such grossly misleading findings as to be totally misleading. The implication of the present study is that, the longer the time window, the better.

The present findings lend further credence to the development of the prevention of repeat victimization as a crime prevention strategy. Levels of repeat victimization may well be significantly greater than has been recognized in most studies to date. However, the measurement of repeat victimization that is used to develop and evaluate crime prevention projects needs to be conducted with caution and with appropriate consideration given to methodological issues that may otherwise be assumed to be negligible.

Repeated residential burglary appears to be explained by both event dependency and risk heterogeneity. It also seems reasonable to expect that an interaction effect between the two will occur. Thus, an attractive residential property that is burgled will be more likely to be burgled again soon thereafter if the offender finds it to be a suitable target. Burglary due to risk heterogeneity should lead to event-dependent burglary.

For crime control strategies, utilizing victimization as a flag that triggers the placement of crime prevention resources will remain an efficient means of resource allocation. This is particularly true if risk heterogeneity is caused at least partly by simple visual cues to offenders that cause them to think a property is attractive. Such otherwise enduring cues of attractiveness might be altered at low cost if recognized during a police security survey of the property. Allocating police officers responsible for security surveys to properties that have proven vulnerable may thus be a simple means by which such resources could be used more effectively.

Repeat victimization prevention strategies that are able to reduce event dependent burglary in combination with those that address issues of risk heterogeneity (by the same or different tactics) may thus prove the most effective over all. When the possibility of detecting offenders who return to replicate a recent burglary is considered, then event-dependent burglaries become of greater practical significance.

There is a need for replication of the methodology used here with different burglary data sets, and also with different types of crime. The data used here were aggregated to the building level rather than the individual-address level. We would anticipate that the finding would change depending on the level of aggregation of the data. Although prediction is tricky and treacherous, we might anticipate some of the findings if this analysis were conducted at the individual-address level: we would expect the frequency of short-term repeats to increase, since repeats at the same address are more likely to be related repeats than those at different addresses within the same building. Consequently, we might anticipate that the time window is of lesser influence over longer periods (greater than a year) than was

found in the present study. In terms of the graphical curve, we would expect it to level off more quickly, so that the marginal returns to extending the observation period decreased more rapidly after the first year. This is a hypothesis that future research might seek to test.

CONCLUSION

Methodological issues relating to the measurement of crime rates can be of importance for policy, practice, and crime theory. They can influence substantive knowledge about crime patterns and the way in which crime prevention strategies are developed. Such methodological issues should not be neglected in favor of the rush towards grandiose social theories, with which much criminological research is unfortunately concerned. We would propose that in the study of repeat victimization, continued attention to methodological issues of measurement should continue to inform theory, crime control, and policing practice.



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