Measuring and interpreting repeat victimization using police data: An analysis of burglary data and policy for Charlotte, North Carolina.

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MEASURING AND INTERPRETING
REPEAT VICTIMIZATION USING
POLICE DATA: AN ANALYSIS OF
BURGLARY DATA AND POLICY FOR
CHARLOTTE, NORTH CAROLINA

by

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Abstract: This study of repeat victimization and crime prevention tackles issues relating to the measurement and interpretation of police data. In a study of repeat burglaries in Charlotte, NC, published in this series, LeBeau and Vincent (1997) framed their discussion as a critique of our work on preventing repeat victimization. We reanalyze and reinterpret the data and findings from that study. First, we explain why the police data significantly understated the true rate of repeat burglaries in Charlotte. Second, we reanalyze the published data to show that, nevertheless, there were at least nine times more repeat burglaries than would occur by chance and that, after a first burglary, the predictability of repeats rises dramatically with each subsequent burglary. Third, we explain why the policy conclusions of LeBeau and Vincent were misleading in relation to preventing repeat burglary. Fourth, we explain why their policy conclusions were misleading with respect to the potential use of burglar alarms as part of a prevention strategy. Fifth, we list some of the relevant literature that the study should have considered more closely. We conclude that their 1997
study did a disservice to the study of repeat victimization and crime prevention, and we reassert that the prevention of repeat burglaries could be a useful component of crime prevention strategy in Charlotte. More generally, the need for academic studies to consider the methodological issues relating to the measurement of repeat victimization is not sufficient reason to avoid or delay practical crime prevention efforts. We conclude with a "hit rate challenge" that crime prevention research should seek to identify predictors that are more efficient than, and at least as practical as, prior victimization.

INTRODUCTION

Perhaps the central concern of crime prevention strategy is the allocation of practical prevention efforts in proportion to risk of victimization. One resource allocation strategy for which there is much research support involves combining victim help with crime prevention to prevent repetition of crime against the same targets. Since police are overwhelmingly the first responders to crime calls, they constitute the agency with greatest potential to enable such strategies. Police data afford the most frequently used source of information on repeat victimization, with the result that the manner in which such data are collated, analyzed and interpreted is critical for the crime prevention mission. This study focuses upon the analysis and interpretation of police data on repeat victimization by presenting a case study of the analysis and interpretation of such data on repeat burglary in Charlotte, North Carolina.

In volume 8 of the Crime Prevention Studies series, James L. LeBeau and Karen L. Vincent analyze and map police data on repeat burglary and repeat burglar alarm activations in Charlotte, NC (LeBeau and Vincent, 1997). With its opening sentence, the article frames itself as a critique of our work on repeat victimization (1997:289):

A new doctrine for enhancing the efficiency and efficacy of crime prevention measures has been to focus on those that have already been victimized, because prior research has indicated they are more likely to be victimized again (Farrell and Pease, 1993).

Those parts of the Charlotte study relating to repeat burglaries and the use of alarms to prevent repeat burglaries will be reanalyzed and reinterpreted in this essay. Two key conclusions were drawn by Le Beau and Vincent that are central to the present discussion. The first is that
...efforts focusing on reducing repeat victimization might be inefficient since burglary is primarily a single-address crime (p. 306).

The second conclusion relates to the use of alarms to prevent repeat burglaries, and is that,

Therefore, within a police agency, any improvements in effectiveness and efficiency gained by focusing on repeat-burglary victimizations can be negated by continuously expending the resources for responding to burglar alarms (p. 290).

In what follows, we take issue with the method and interpretation that yielded these conclusions, and propose that they are incorrect. Where “the study” is referred to without further specification, it is the Le Beau and Vincent work to which reference is being made.

The present chapter is structured as follows. First, we argue that the study did not account for the known methodological issues relating to the measurement of repeat victimization using police crime data. Second, we argue that, even taking the data at face value, they were misinterpreted in the study. We reanalyze the data to show that there were nine times more repeat burglaries in Charlotte than would be expected by chance, and that the likelihood of further burglary increases dramatically after each burglary at a particular target. Third, we discuss general policy issues and conclude that, with over a quarter of all burglaries being repeats, there is rich potential for Charlotte Police Department to develop a crime prevention strategy based around the prevention of repeated burglary. Fourth, we discuss the specific policy issue relating to the use of burglar alarms. We suggest that our earlier recommendations for policy and practice in this area were misinterpreted. We propose that the policy analysis of the study therefore did not engage with the recommendations that it purports to criticize. We describe how, as part of an overall burglary prevention effort, the overt and covert use of alarms, selectively allocated to high-risk premises on a rotating basis, may be a resource-efficient means of promoting crime prevention and offender detection. The fifth key issue locates the study in the broader context of knowledge relating to repeat victimization. The documented evidence relating to repeat victimization, even that which existed prior to the 1997 Charlotte study, means that, even if the study’s data and policy analysis had been well-founded, a generalized conclusion could not be based upon the study. This chapter draws overall conclusions in its final section.

The justification for this case study is as follows. LeBeau and Vincent’s (1997) article “Mapping It Out: Repeat Burglar Alarms and Burglaries” does a potential disservice to the study of repeat victimi-
zation and crime prevention. By disregarding previous research, and consigning crime reduction through preventing repeat victimization to the realm of "doctrine," potential harm was done to reasoned debate of the issues, as well as to the development of crime prevention policy and practice. It is not just the city of Charlotte that may be affected: The dissemination of the published work could potentially influence a wide range of crime mapping, crime prevention, and policing practitioners and researchers, victim service practitioners, policy makers and others who could conclude that the findings of the work are applicable in other jurisdictions. If this were the case, it could be detrimental to victims. By describing the limitations of the work it is hoped that this may reduce the chances that these errors will be repeated elsewhere.

THE KEY ISSUES

1. Methodological Issues of Measurement

There are at least three reasons that police-recorded crime incident data understate the rate of repeat burglaries. They were discussed in our 1993 monograph that is referred to in the opening line of the LeBeau and Vincent study. However, those authors do not address these issues in their article, and present the data at face value as a true representation of the rate of repeat burglaries in Charlotte in 1990. The three methodological issues are addressed below in turn, with some recent examples set out. A fourth issue is touched on and addressed later in this essay.

It has long been known that large amounts of burglary, as with many crimes, are not reported to the police. This has traditionally been known as the "dark figure" of crime (see e.g., McClintock, 1970). When it comes to repeat victimization, the dark figure is disproportionately large. This issue was discussed in our 1993 monograph in the section entitled "Methodological issues in the study of repeat victimization," as follows:

**Recorded crime data**

The under-reporting of crime to the police is compounded in the case of repeat victimization. Much crime goes unrecorded since it is unreported...some crime is reported to the police but remains unrecorded. Taking both reasons together, much crime fails to appear in police data banks. This tends to the understatement of the extent of repeat victimization. For example, a household suffers a burglary. A burglary has roughly a 70% (or 0.7) chance of featuring as a recorded crime in police statistics. The household suffers a second burglary. This too
has a roughly 70% chance of featuring in police statistics. This means that the chance that they have both been recorded is 0.49 or 49% (that is, $0.7 \times 0.7$). With three burglaries at the same address, given the same chance of being recorded...[only] 34% will have all three burglaries recorded (Farrell and Pease, 1993:16).

Rates of reporting burglaries to the police in the U.S. are lower than those used in the quotation above. The National Crime Victimization Survey suggests that consistently around 50% of burglaries are reported to the police nationwide, with a range from 48.5% to 51.8% for the period 1993 to 2000 (Rennison, 2000:10). If the reporting rate in Charlotte is close to the national average, this would mean that the understatement of repeat burglaries in the Charlotte dataset was even greater than that suggested in the quotation above. It would mean, for example, that only 25% of households with two burglaries would report both to the police ($0.5 \times 0.5 = 0.25$ or 25%) and that only 12.5% of households with three burglaries reported all three (since $0.5 \times 0.5 \times 0.5 = 0.125$ or 12.5%). Moreover, the simplifying assumption that reporting rates are independent and do not change for repeat burglaries does not necessarily hold: Mukherjee and Carcach (1998) suggest that second and subsequent burglaries are less likely to be reported to the police than "first" burglaries. If this were the case in Charlotte, it would further compound the understatement of repeat burglaries. For example, if the average likelihood of reporting a first burglary was 50% but that of reporting a second burglary was only a 40%, then only 20% of households with two burglaries would report both (since $0.5 \times 0.4 = 0.2$ or 20%). Though the actual reporting rates in Charlotte will undoubtedly differ from those used here for illustration, the basic point stands.

There are at least two other methodological issues of relevance that will be covered more briefly. For a fuller explanation, the reader is referred to Farrell and Pease (1993, chapter 3). Data formatting and cleaning is of crucial importance in indexing the levels of repeat victimization. The manner in which locations are recorded means that repeats at the same place are often overlooked. For example, a burglary recorded at the location "1 High St." may not necessarily be linked to a burglary recorded at the location "1 High Street" since, although they are the same location, the specific formulation of the address is different in that data field. This is common since computerized police data are kept for operational rather than research purposes. It cannot be stated with certainty that this was accounted for in the data relating to Charlotte, since the issue is not discussed in the published study. However, our experience elsewhere, including the analysis of data on repeat burglaries in Dallas, Baltimore and
San Diego (see Farrell et al., 2002), would suggest that it is likely to have been a relevant methodological consideration. It is highly unlikely that the Charlotte burglary database would not have required any such data formatting. Overcoming the current limitations of police information technology remains critical in relation to repeat victimization research (see Pease, 1998:32-33).

The third methodological issue concerns the time-window in the measurement of repeat victimization. One year of police data will have an overrepresentation of single-incident burglaries relative to the total. Some of the “singles” will be repeats of burglaries the year before. Some of the “singles” will be precursors of burglaries that occurred after the last date of the dataset. A recent study examined the time-window effect upon the measurement of repeat burglaries. It found that, as a proportion of total burglaries, 12 months of data captured, proportionately, 42% more repeat burglaries than six months of data. Three years of data captured, proportionately, 57% more repeat burglaries than one year (Farrell et al., 2002). Anderson et al. (1995) developed a weighting that aimed to account for the attrition of repeats due to the time-window (see also in Townsley et al., 2000). Nick Tilley (1993) tackled the time-window problem using a “rolling year” that tracks repeats at a location for a whole year after a burglary. The rolling-year method has more recently been used by Deborah Lamm Weisel and Don Faggiani in their ongoing study of repeat burglaries in Austin (TX), Indianapolis (IN), Miami (FL), and Montgomery County (MD) (Faggiani and Lamm Weisel, 2000). A recent guide for practitioners recommends using a rolling one-year period to examine repeat victimization (Home Office, 2001:9). The time-window issue is not mentioned in the original publication on burglaries in Charlotte, from which we infer that it was not considered.

A possible fourth methodological issue could be raised: Analysis at a lower spatial level than that of the city would most likely have identified smaller areas of Charlotte with above-average burglary incidence and higher rates of repeat burglary. We return to this issue later.

In short, known methodological issues relevant to the use of police data for the study of repeat burglaries were not adequately considered in relation to Charlotte. The relevant methodological issues had been tackled in a range of prior publications, including work cited by LeBeau and Vincent. We conclude that the true rate of repeat burglaries in Charlotte was underrepresented, and that failure to recognize this fact contributed to a misunderstanding of the extent of repeat burglaries.
2. Observed and Expected Repeat Burglaries in Charlotte

This section reanalyzes the burglary data presented in the 1997 article. We expand upon the criticism of the Charlotte study presented by John Stedman and Deborah Lamm Weisel (1999:8) who wrote:

[T]he presence of a repeat problem may be missed, as it was by researchers in one U.S. city who reported that "burglary is primarily a single-address phenomenon since 81.25% of all the victimized addresses and 61.1% of all the burglaries involve just one call to a single address" (LeBeau and Vincent, 1997). According to this study’s data, one-time victimizations occurred at...61 percent [of addresses]. But the remaining 39 percent of burglaries ... occurred at addresses with two or more burglaries during the calendar year. This is a significant amount of repeat victimization.

In this section, we reanalyze the original published data from the Charlotte study. Hence the reanalysis is not contingent upon an acceptance of the methodological arguments made in the previous section. The reported levels of repeat burglary in Charlotte were clearly far below what the authors of the study had expected to find. This was, in part, what led them to assert that preventing repeat burglaries would not be an efficient crime prevention strategy. In this section we demonstrate that, even in the published burglary data, there were nine times more repeat burglaries than would be expected if they were randomly distributed. Further, we demonstrate that the likelihood of further burglary increases dramatically after each burglary against a particular target. If the methodological issues discussed in the previous section were also considered, it is likely that the nine-fold difference would be significantly greater.

Some targets will be repeatedly burgled by chance alone. If burglary were randomly distributed across targets, some would still get hit two, three or even more times. This possibility can be controlled for by comparing the actual distribution of burglaries across targets to that of the Poisson distribution. The Poisson distribution describes the expected distribution of burglaries across targets if burglaries are independent of each other. The work of Sparks et al. (1977) compared the distribution of repeat victimization to that expected under the Poisson distribution. The Poisson distribution is given by the formula

\[ f(x) = \left( \lambda^x \ e^{-\lambda} \right) / x! \]
where \( x \) is the number of burglaries we are estimating, \( \lambda \) is the mean number of burglaries in the population, and \( e \) is the base of the natural logarithmic function (\( \ln \)). In this instance, \( \lambda = 0.0573 \) mean burglaries per property (10,828 burglaries across 188,896 properties eligible for burglary victimization in Charlotte in 1990), and \( x \) is the number of times burgled (column 1 of Table 1).

In Table 1, the "observed" levels of burglary are taken from Table 3 of the published study of Charlotte burglaries (LeBeau and Vincent, 1997:298). The expected levels of burglary were calculated for this paper using the Poisson formula. The first row of numbers in Table 1 shows the estimated number of properties in Charlotte that were not burgled in 1990. Since the published burglary data include residential and non-residential properties, the total number of properties in Charlotte was derived as the sum of the number of households in Charlotte (data from the 1990 census)\(^1\) plus the number of businesses in Charlotte (data provided by the Business License Office of the Charlotte Tax Office)\(^2\). The totals at the base of the two final columns show the expected and observed levels of repeat burglary in Charlotte. If burglary were distributed according to chance alone, we would have expected only 2.81 percent of all burglaries to be repeats at the same addresses. The bottom right cell of the table shows that the previously published data — which, for reasons mentioned, understates the rate of repeat burglaries — demonstrates 25% or a quarter of all burglaries as repeats against the same addresses. This is a nine-fold magnitude of difference from that which would occur by chance. The difference is massive and highly statistically significant (\( x^2 = 292,677.7, \text{d.f.}=2, p<0.000; \text{see Table 2} \)). Moreover, it is the 0.8% of all addresses (\( n = 1,500 \) addresses) reporting more than one burglary that accounted for 39% of all burglaries (4,212 of 10,828 burglaries: see Table 1).

What if crime prevention resources were allocated to properties that had been victimized more than twice? Many police repeat victimization prevention efforts now use a "graded response" so that properties burgled more frequently receive more preventive attention (after Anderson et al., 1995). This is based on the finding that victimization risk increases with each victimization (Ellingworth et al., 1995). Consider the impact of a graded response in Charlotte. Whereas only 8,116 or 4.3% of the addresses in Charlotte reported a burglary, this rises to 18.5% of those burgled once, 33.9% of those burgled twice, and 44.4% of those burgled three times. Those burgled four times had a greater than even chance of reporting another burglary within the year (54.9%). This rose to 63.7% after a fifth burglary, but leveled off somewhat after that. The contingent risk of burglary after \( n \) burglaries is shown in Figure 1.
<table>
<thead>
<tr>
<th>Times burgled</th>
<th>N Addresses</th>
<th>N Burglaries</th>
<th>Expected</th>
<th>Observed</th>
<th>N Repeat Burglaries</th>
<th>Expected</th>
<th>Observed</th>
<th>% Repeat Burglaries</th>
</tr>
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<td>293</td>
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<td>17</td>
<td>8</td>
<td>0</td>
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<td>0</td>
<td>0.00</td>
</tr>
<tr>
<td>Times burgled</td>
<td>N Addresses</td>
<td>N Burglaries</td>
<td>N Repeat Burglaries</td>
<td>% Repeat Burglaries</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>--------------</td>
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<td>Observed</td>
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<td>Expected</td>
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<td>Expected</td>
<td>Observed</td>
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<td>1</td>
<td>0</td>
<td>43</td>
<td>0</td>
<td>42</td>
<td>0.00</td>
<td>0.39</td>
</tr>
<tr>
<td>Total</td>
<td>188,896</td>
<td>188,896</td>
<td>10,824</td>
<td>10,828</td>
<td>304</td>
<td>2,712</td>
<td>2.81</td>
<td>25.05</td>
</tr>
</tbody>
</table>

Sources: Table 3 of LeBeau and Vincent (1997:298); Number of households in Charlotte in 1990 from 1990 census; Number of business properties in Charlotte in 1990 from the Business License Office of Charlotte Tax Office.

Note: Differences in column totals for expected and observed burglaries are due to rounding.
Table 2: Grouped Observed and Expected Distribution of Burglaries in Charlotte 1990

<table>
<thead>
<tr>
<th></th>
<th>Addresses with 1 burglary</th>
<th>Addresses with 2 burglaries</th>
<th>Addresses with 3 or more burglaries</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Expected</td>
<td>10,221</td>
<td>586</td>
<td>17</td>
<td>10,824</td>
</tr>
<tr>
<td>Observed</td>
<td>6,616</td>
<td>1,982</td>
<td>2,230</td>
<td>10,828</td>
</tr>
<tr>
<td>Total</td>
<td>16,837</td>
<td>2,568</td>
<td>2,247</td>
<td>21,652</td>
</tr>
</tbody>
</table>

χ² = 292677.7, df=2, p<0.000.

Note: Differences in row totals for expected and observed burglaries are due to rounding.

Figure 1. Contingent Risk Trajectory:
Percent Chance of Charlotte Burglary after n Priors

A commentator might suggest that our calculations in Table 1 could be wrong if the estimates of the number of premises in Char-
lotte — that is, the number of potential “targets” of burglary — are incorrect. To account for this possibility, we conducted a sensitivity analysis using a range of estimates of the number of properties eligible to be burgled in Charlotte in 1990. We set the estimated number of properties in Charlotte (i.e., varying λ, lambda, in the Poisson distribution estimates) at levels ranging from 25% above to 25% below the actual number of properties as estimated from the census data and the data provided by the Business License Office of the Charlotte Tax Office. The results are presented as Table 3. It was found that, even when the number of properties varies greatly, the influence upon the level of expected repeat burglaries is sufficiently small that it makes little difference to our overall analysis and its interpretation. The conclusion from this sensitivity analysis is that the findings of the comparison of observed and expected levels of repeat burglary in Charlotte are robust.

**Table 3: Sensitivity Analysis to Measure Change in Level of Expected Repeat Burglaries in Charlotte According to Number of Eligible Properties in Charlotte**

<table>
<thead>
<tr>
<th>Sensitivity Test Level (% Change from Best Estimate of Number of Properties)</th>
<th>Number of Properties Eligible to be Burgled in Charlotte</th>
<th>Percent “Expected Repeat Burglaries”</th>
</tr>
</thead>
<tbody>
<tr>
<td>25% fewer properties</td>
<td>141,672</td>
<td>3.73</td>
</tr>
<tr>
<td>10% fewer properties</td>
<td>170,006</td>
<td>3.12</td>
</tr>
<tr>
<td>5% fewer properties</td>
<td>179,451</td>
<td>2.96</td>
</tr>
<tr>
<td>Estimate from 1990 census and Charlotte Business Office*</td>
<td>188,896</td>
<td>2.81</td>
</tr>
<tr>
<td>5% more properties</td>
<td>198,340</td>
<td>2.68</td>
</tr>
<tr>
<td>10% more properties</td>
<td>207,786</td>
<td>2.56</td>
</tr>
<tr>
<td>25% more properties</td>
<td>236,120</td>
<td>2.26</td>
</tr>
</tbody>
</table>

a. Used in Table 1.

In short, whereas the original analysis suggested that the data showed little repeat burglary in Charlotte, our reanalysis showed that
there were nine times more repeat burglaries than would be expected by chance, and that on average, risk increases sharply after each burglary reported at an address. As mentioned at the beginning of this section, our reanalysis is independent of the fact that the police burglary data is an understatement of the true rate of repeat burglaries.

3. Interpretation of Findings

This section explains a key issue relating to the interpretation of the empirical findings of the LeBeau and Vincent study, and its major policy conclusion. This issue is independent of the two issues discussed to this point.

Probably the most important conclusion of the 1997 study was as follows (LeBeau and Vincent, 1997:306):

At the citywide level, efforts focusing on reducing repeat victimization might be inefficient since burglary is primarily a single-address crime.

This conclusion does not make a critical distinction between two concepts. The first concept is efficiency, or cost-effectiveness, in relation to preventing repeat burglaries. The second concept is the volume of crime that might be prevented by targeting repeat burglaries. The statement in the quotation conflates the two concepts, and, as will be explained below, thereby leads the authors to a conclusion that, in our view, is not supported by their analysis.

With regard to the issue of efficiency, the empirical finding that "burglary is primarily a single-address crime" does not necessarily lead to the conclusion that "efforts focusing on reducing repeat victimization might be inefficient." The fact that preventing repeat burglaries may not prevent all burglary does not mean that it cannot be either a useful or efficient burglary prevention strategy. For example, if only 10% or 5% or even 1% of burglaries were repeat burglaries, it could still be the case that targeting repeat burglaries would be an efficient crime prevention strategy. This is because efficiency, or cost-effectiveness, is a relative concept. The cost-effectiveness of the prevention of repeat burglary should be measured relative to that of other crime prevention strategies. In short, if a crime prevention strategy is more cost-effective, per crime prevented, than the alternatives, then it should be introduced irrespective of the overall volume of crime that could be prevented. In understanding this issue it is also useful to remember that preventing repeat burglaries can and should be used alongside, and as a complement to, other crime prevention strategies.
With regard to the issue of the volume of burglaries that might be prevented, the 1997 study reports (LeBeau and Vincent, 1997:295):

Burglary is primarily a single-address phenomenon since 81.52% of all the victimized addresses and 61.1% of all the burglaries involve just one call to a single address.

The statement is factually correct according to the data presented and, while it has been explained above why these numbers underrepresent the true rate of repeat burglaries, the numbers will be taken at face value for present purposes. The issue here is partly one of presentation and the formulation of the statement. The fact that "61.1% of all the burglaries involve just one call to a single address" means that 38.9% (i.e., 100% minus 61.1%), or nearly 4 in 10, of all the burglaries involve more than one call to a single address. The emphasis introduced by the word "just" in the sentence leads the reader to believe that this is a bad thing. Not so. The presentational issue is as follows: It is easier to perceive the value and extent of the crime prevention potential from the fact that 4 in 10 calls are to repeat-addresses than it is to perceive it from the fact that most calls are to single-addresses. In the context of the above discussion on efficiency of crime prevention effort, it is clear that tackling repeat burglaries can be an attractive and efficient strategy even if it means that less than half of all burglaries could potentially be prevented. No policy maker or police chief would dismiss a strategy because it "only" had the potential to reduce crime by less than half!

A technical aside is necessary here, to explain the distinction between the 38.9% of calls (4 in 10) to repeat-addresses and our earlier finding that 25% of calls were repeats. The two numbers are entirely compatible but are calculated using different numerators. The 38.9% includes all repeat calls to addresses including the first call (i.e., first, second, third, fourth calls etc.). The 25% figure refers only to repeat calls (second, third, fourth etc.), but excludes the first in each series since this is not, chronologically speaking, a repeat.

In relation to efficiency and crime prevention, a further note is also warranted in relation to the level of spatial aggregation of the Charlotte burglary data. The study presents data that are aggregated across the whole city. Prior to this, it had been shown that repeat victimization is disproportionately high in high-crime areas (Trickett et al., 1992). A more recent extensive study of repeat burglaries in several U.S. cities used police-recorded crime data. It was conducted by Deborah Lamm Weisel and Don Faggiani, and found similar beat-level spatial concentrations of repeat burglaries (Lamm Weisel and Faggiani, 2000, 2001). Were the Charlotte burglary data disaggregated into smaller spatial units such as census tracts or police beats,
it is likely that it would show disproportionately high rates of repeat burglary in some areas. In these areas, crime prevention efforts targeting repeat burglaries would be commensurately more efficient. In the present study, it is possible that, for example, targeting repeat commercial burglary in the central business district of Charlotte would be particularly rewarding. Since the data were geo-coded to allow the production of maps, it is possible that this type of areal analysis would still be possible with the original dataset. The technical feasibility of such analysis was acknowledged in the study (Le-Beau and Vincent, 1997:307), but it was not undertaken.

4. Understanding Alarm Policy

This section relates to the use of burglar alarms in relation to the prevention of repeat burglaries. It first presents the relevant conclusion of the 1997 study of Charlotte. It then explains why, in the context of the strategic use of alarms in the prevention of repeat burglaries as proposed in our earlier work, the conclusion was misplaced.

The main conclusion relating to burglar alarms and repeat burglaries that is drawn in the 1997 study of Charlotte runs as follows (Le-Beau and Vincent, 1997:290):

Moreover, what is more problematic is that a majority of the burglary alarm calls are false. Therefore, within a police agency, any improvements in effectiveness and efficiency gained by focusing on repeat-burglary victimizations can be negated by continuously expending the resources for responding to burglar alarms.

As formulated, this statement could be said to be factually correct. In the context of the study, however, it is clearly presented as a critique of our earlier proposals relating to the use of burglar alarms as part of strategic efforts to prevent repeat burglaries (Farrell and Pease, 1993; Farrell, 1995). It will be shown here that the assertion does not engage with the substance of our argument relating to the use of burglar alarms in this context.

The 1997 study of repeat burglary and the use of burglar alarms in Charlotte does not address the key substantive aspects of the strategic manner in which we had proposed that burglary alarms should be utilized. This strategy had three components. We had previously proposed: (1) the use of a relatively small pool of portable alarms, (2) that these alarms would be allocated to high-risk repeatedly burgled locations, and (3) that the alarms would be rotated between targets according to victimization risk, where risk is indicated by high-volume repeated burglary. Further, our previous work addressed the issues relating to the proliferation of burglar alarms and
the volume of false alarms that appear to be highlighted in the Le-Beau and Vincent study. Specifically on this issue, a 1995 publication stated (Farrell 1995:520):

The use of portable intruder alarms for crime prevention has advantages over permanent alarms. The problems of 'conventional' (i.e., permanent) intruder alarms are twofold. First is the high rate of false activations. It has been estimated that over 95 percent of intruder alarm activations are false (see Pease and Litton, 1984:190). A second problem that compounds the first is the proliferation of permanent alarms across households and commercial properties. False alarms are a drain on police resources. With the proliferation of alarms, this becomes a great drain on police resources... With the portable intruder alarms [installed temporarily at repeatedly burgled locations during the period of high risk after a burglary], the problems of proliferation and false activation are both overcome to some extent. The number of alarms issued is far lower, and the number of false alarms will be much less... A genuine activation is much more likely during the loan period than one from a permanent alarm where allocation is not determined by probability of victimization. The alarms will not proliferate because they are issued on a temporary basis by an agency that reclaims them. [The sentence in brackets was inserted for overall brevity.]

This paragraph, from an article published in 1995 in the Crime and Justice series, addresses the issues raised by LeBeau and Vincent. The strategy was also discussed in earlier publications relating to the prevention of repeated domestic violence (Farrell et al., 1993; Lloyd et al., 1994), in which a small pool of alarms were allocated according to risk on a rotating basis. The reason that portable alarms can be rotated is that many studies of repeat victimization have found that repeats occur within a short period of time after victimization. The first study to examine this in depth was in Saskatoon, Canada (Polvi et al., 1990). More recent studies with similar findings on the time-course of repeat residential burglary are those by Matthew Robinson in his study in Tallahassee (FL), and those by Deborah Lamm Weisel and Don Faggiani in their studies in four U.S. cities (Faggiani and Lamm Weisel, 2000). Aspects of the methodology of time-course studies have been improved over time. Bill Spelman’s critique (Spelman, 1995) led Anderson et al. (1995:47) to adjust the method, but the resultant patterns of the time-course distribution were essentially the same (a finding replicated by Townsley et al., 2000).

There are some burgled properties where long-term crime prevention interventions are required. Indeed, permanent or long-lasting
crime prevention efforts are typically preferable if they do not impose a heavy resource burden. Such long-term changes could include design and other environmental and situational changes to target attractiveness, or the removal or alteration of attractive hot products that induce burglary. However, even in many of these instances, an immediate short-term intervention can provide a breathing space to allow a longer-term intervention to be developed (preferably one that is not based around an alarm system). Design changes do not take place overnight, whereas repeat burglaries do so, with unusual frequency. Further, if repeat offenders are detected at quick repeat burglaries, then a short-term intervention may alter some of the relevant longer-term risk factors.3

We naturally agree with the conclusions from the original Charlotte study with respect to part of the recommendations relating to how police might reduce the volume of false alarms. LeBeau and Vincent recommend the introduction of a system of fines for the owners of properties that produce repeated calls to, and drains upon, the police. This is a policy recommended by one of the present authors some years earlier (Pease, 1979; see also the discussion of the public and private costs of false alarms and insurance practice in Pease and Litton [1984], who cite five studies of false alarms from the 1970s). LeBeau and Vincent note that, “As a matter of fact, Charlotte is in the process of implementing a fine system.” In a review of false burglary alarms, Rana Sampson (2002) identifies different categories of response for the police. In her review, the three “best responses” are: “1. Requiring alarm companies to visually verify alarm legitimacy before calling the police” (Sampson, 2002:13), “2. Charging a fee for service for all false holdup, duress and panic alarms” (Sampson, 2002:14), and “3. Responding to holdup, duress and panic alarms only if they come from a stationary building” (Sampson, 2002:14). The responses with limited or some effectiveness include establishing a fee for responding to alarms, establishing an ordinance with escalating fines for alarms, accepting dispatch cancellations, alerting alarm companies about false-alarm abusers, publishing alarm companies’ false-alarm rates, holding false-alarm classes, and lowering the call priority of alarms (Sampson, 2002:15-17). It is recommended that police do not assign alarm calls the highest priority of response (Sampson, 2002:17). Sampson does not discuss the possibility of rotating a pool of priority alarms to high-risk burglary locations. This may reflect a desire to separate the issue from that of false alarms in general. However, it may also reflect the impact of the study of burglary alarms in Charlotte, which Sampson cites. While it is clear that there is a need for further research to examine the effectiveness of the potential for reallocating priority alarms, the present authors
hope that this potentially cost effective strategic use of alarms is not lost in the rush to reduce general false-alarm proliferation.

5. Research and Practice

It is clear from the discussion above that the 1997 study of Charlotte did not take into account some of the relevant previous research on the study of repeat victimization, even though some of these publications were cited. It is useful to clarify the extent of this research. Prior to the LeBeau and Vincent publication, there were many studies by a range of researchers using various techniques, data sources and methodologies. These provided a solid empirical base relating to repeat victimization. By no later than 1995 (two years prior to the publication of LeBeau and Vincent's study in 1997, to allow for some lead-in time), the relevant publications in the preceding decade included Forrester et al., 1988, 1990; Pease, 1991, 1992; Trickett et al., 1992; Farrell, 1992, 1995; Tilley, 1993a, 1993b, 1994; Johnston et al., 1994; La Vigne, 1994; Lauritsen and Davis-Quinet, 1995; Buck et al., 1995; Hope, 1995; Ellingworth et al., 1995; Spelman, 1995; and Buss and Abu, 1995. Since 1995, many more studies on repeat victimization have been published. At the time of writing, they number well over a hundred. Readers should note that we are not saying — and never have — that repeat victimization is always a major component of crime under all circumstances. However, we are saying that prior research and methodological issues should be consulted when examining repeat victimization.

As with most research in any subject, though less than in many, the relevant methodological issues relating to repeat victimization are not straightforward. However, it is important to clarify that practitioners need not understand these issues or take them into account when undertaking everyday crime prevention work. Once the need to develop responses to prevent repeat victimization is recognized, such responses are triggered by the reporting of a crime. Police officers seeking to determine the extent of victimization over the last year at a particular address can ask the victim, owner or manager. This does not require intensive day-to-day supervision and interrogation of a highly sophisticated information technology system, or weighting adjustments for under-counting and time-window factors — just as frying an egg does not require knowledge of the manufacture and physical properties of Teflon, or of the evolutionary biology of the hen. If risk-graded responses to victimization become accepted as routine good practice, many of the methodological issues that apply to research are far less relevant to everyday practice.
CONCLUSIONS AND A CHALLENGE TO CRIMINOLOGISTS

Based upon the reanalysis of the findings and interpretation of "Mapping it Out: Repeat-Address Burglar Alarms and Burglaries," our main conclusion is straightforward. The 1997 study of repeat burglaries in Charlotte, North Carolina, was misleading in several critical respects. Our reanalysis of the data and the policy implications strongly suggests that the Charlotte Police Department and the city of Charlotte could benefit from developing a policy to prevent repeat burglaries.

Police data are the source that is typically most easily available to researchers seeking to examine repeat victimization at a local or city level. Police data are routinely collated for operational purposes and frequently more available to researchers than data from other public or private agencies, whereas crime victim surveys are expensive to conduct on a large scale. Due to the important role that police data play as a source of information on repeat victimization, the manner in which they are analyzed and interpreted is critical. The task is not necessarily straightforward. Should the data be taken at face value, the effect upon the findings and policy conclusions may be detrimental to the crime prevention mission.

Several research questions remain in relation to the use of police data to examine repeat victimization. What is the undercount of repeats that is due to data entry discrepancies, errors, spelling and other mistakes in police databases? (Hanmer et al., 1999 make some progress in examining this issue.) Do such attrition rates differ between crime types, types of information technology and other variables? Are Mukherjee and Carcach's (1998) findings — that reporting rates decline with repeat victimization — generally applicable? Perhaps the most pressing task is to give practical assistance and advice to practitioners working with police data. If police data indicate a certain level of repeat victimization, what is the true rate of repeat victimization likely to be? Perhaps a formula for weighting can be developed so that, where partial information is known — such as crime type and area incidence — then the true rate of repeats can be estimated within reasonable confidence intervals. Such estimates could provide police and policy managers with information allowing them to infer the impact upon crime rather than upon recorded crime.

Before closing the chapter by issuing a "hit-rate challenge," we wish to stress our rejection of any suggestion that the prevention of repeat victimization should be the only crime reduction show in town. The suggestion would be absurd, not least in the light of published work to which we have contributed arguing for the integration
of burglary risk factors into a prevention strategy (Chenery et al., 2002). In yet to be published work, Kate Bowers and Shane Johnson of Liverpool University, and Brian Ewart and colleagues (2002) of Sunderland University, seek to explore interactions between prior burglary events, demography and criminal method in ways which maximize the predictability of first-time and repeat events. The authors are exploring notions of "virtual repeats," in which identical or similar characteristics of crime targets, albeit separated in space, drive the recognition and adoption of crime opportunities. These directions of work reflect the complex ways in which prior risk shapes burglary events which in turn shape similar events, according to propinquity in time and space, and target similarity. Our understanding of these sequences seems an apt avenue for the development of repeat victimization research, to the point where the concept melds into a sophisticated form of crime risk assessment.

Let us now return to the immediate future, and the metric by which direct forms of repeat victimization may be assessed as potentially contributing to crime reduction, as knowledge stands now. The study of predictive efficiency or "hit rates" is clearly important for the allocation of scarce crime prevention resources. Repeat victimization can be used to allocate resources with an increased confidence that they will be deployed where and when they are required. The daily and nightly task of the police manager is to expend effort, both reactive and proactive, in ways that maximize the likelihood that an offence will be thwarted or detected. On the basis of the Le Beau and Vincent data reanalysed here, allocating effort to homes on the basis of their recent burglary history is, on average, nine times better than allocating effort to homes randomly. However, the predictive power or hit rate of repeat victimization increases with further victimizations of the same target. Whereas 4.3% of all addresses reported a burglary, this rose to 18.5% of those reporting one, 33.9% of those reporting two, 44.4% of those reporting three, and for practical purposes, never fell below 50% thereafter. The rise in predictive efficiency is over fourfold after a single burglary, almost eightfold after a second, over tenfold after three, almost thirteenthsfold after four, and around fifteenfold thereafter. These are odds that the prudent police officer would be keen to accept, particularly since the routinization of attention to prior crime events relieves him or her of part of the burden of allocating resources on the basis of hunch or intelligence. The appropriate shorthand might be like that of computer disk speeds: a 4x or a 15x hit rate. Since police data underestimate repeat victimization, even the average 9x hit rate for Charlotte may be conservative. A standardized hit rate metric would facilitate comparison across alternative resource allocation strategies for crime prevention. If there are
more efficient and more practical hit-rate generators than repeat victimization, then they warrant comparative assessment via such a hit-rate metric. We propose the following "Hit Rate Challenge" to the field of crime prevention research: *Find a more efficient, but at least equally practical, predictor of crime than repeat victimization.*

This reanalysis and reinterpretation of data relating to burglary in Charlotte is intended to promote a broader understanding of the methodological and policy issues relating to the analysis and interpretation of police data on repeat victimization. Hopefully it will thereby promote effective crime analysis and crime prevention practices that are to the betterment of victims and society as a whole.

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REFERENCES


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NOTES

1. Data from the 2000 census were not available.

2. The authors thank Laura Wyckoff for her endeavors in tracking down the data on the number of businesses in Charlotte.

3. In relation to domestic violence rather than burglary, it was argued that the short-term loan of a priority-response alarm to victims could help create a breathing space in which a longer-term safety plan could be developed (Farrell et al., 1993; Lloyd et al., 1994).

4. For a more complete review of earlier studies, including those in the 1970's and 1980's, see Farrell, 1995.

5. If a three-year lead-time prior to publication was required, this would mean that only those publications up to and including 1994 would be considered. We included a quotation in the previous section from a 1995 paper (Farrell, 1995), but the issue it addressed was also tackled in Lloyd et al. (1994) and this was simply the clearest statement of the argument.