Journal of Quantitative Criminology, Vol. 21, No. 1, March 2005 (© 2005)
DOI: 10.1007/s10940-004-1789-0

Detecting Spatial Movement of Intra-Region Crime Patterns Over Time

Jerry H. Ratcliffe[1]

Many of the traditional measures of the degree to which crime patterns change over space and time have limitations. In particular most are unable to determine any change in spatial crime pattern within an areal unit. Usually studies measure the change in crime levels in contiguous areas (expressed as discrete sub-divisions of a study area), but this can become problematic due to difficulties such as the Modifiable Areal Unit Problem (MAUP). This paper describes a technique developed to allow researchers to examine intra-study region changes in crime patterns between two time periods without the need to aggregate crime counts to within-city areal boundaries. The method presented uses a random point nearest neighbor test combined with a Monte Carlo simulation. The process resolves problems of patterning and the MAUP that are common with a number of spatial displacement and pattern movement studies. This technique is demonstrated with example data from a city-wide police burglary crackdown in the Australian capital.

KEY WORDS: displacement; police crackdown; nearest neighbor; MAUP; point pattern change; Australia.

1. INTRODUCTION

There are many reasons why a researcher might be interested in detecting changes in the spatial distribution of crime patterns. These can include; early identification of changing criminal behavior, changing risk and opportunity structures caused by socio-economic development, displacement or diffusion of benefits resulting from a crime prevention strategy, and non-spatial displacement caused by changing modus operandi or criminal career adaptations.

In particular, the evaluation of crime prevention strategies has been an important feature of the burgeoning literature on "what works" in crime prevention (Sherman *et al.*, 1998). It is not sufficient to be able to say that an

[1]Department of Criminal Justice, Temple University, 1115 W. Berks Street, Philadelphia, PA 19147, USA. E-mail: jhr@temple.edu

initiative was successful in reducing crime: it must also have not precipitated any significant amount of displacement to other crime types, places or modus operandi. Being able to monitor any spatial changes in criminal behavior may also tell the researcher if the initiative has been applied evenly across the target area, if any diffusion of benefits has spread evenly or only sporadically, or (in the absence of a successful intervention) if external factors have changed offending behavior and hampered the crime reduction effort.

Displacement aside, it is also possible that crime patterns have changed for other reasons not connected with any displacement from a crime prevention initiative. Either way, for many practitioners the movement of crime patterns over time is a basic query in many research studies. Whether the research focus is crime pattern change as a result of different socio-economic conditions, displacement from a crime prevention initiative or because of other possibilities, the underlying question that has to be addressed in all of these studies is the spatial movement (or not) of a crime pattern over time. This fundamental question is the focus of this paper. The paper will describe a method to determine if there has been spatial movement in a crime distribution from one time period to another, and will demonstrate this technique using an example of a displacement study from a burglary reduction campaign in Canberra, Australia. Given that spatial displacement is one application of this technique, and a concern of many practitioners, we begin by considering the importance of spatial displacement in recent studies.

2. SPATIAL DISPLACEMENT AND CRIME INTERVENTIONS

While there are many different types of displacement, including type of crime displacement and temporal displacement (Hakim and Rengert, 1981, p. 11), concern is often voiced with specific regard to spatial displacement. Spatial displacement occurs when criminals respond to a crime prevention initiative and move their activities to another location. It can be a common complaint of crime reduction activity that any initiative will only displace crime and not reduce it, merely forcing offenders to commit offences in other places (Town, 2002).

Berry and Jones (1995) note that one of the most potentially beneficial uses of Geographic Information Systems (GIS) is the measurement of displacement resulting from crime prevention initiatives, and spatial displacement has been a common theme throughout much crime prevention literature. Even though there exists a concern regarding displacement, Hesseling's (1994) review of 55 studies found that there was no evidence of any displacement in 40% of the research, and no evidence of full displacement (the complete transposition of a crime rate from one place to another) in any of the remaining studies. A 2 year interrupted time series analysis found no evidence of displacement from an aggressive policing strategy to tackle disorder (Novak *et al.*, 1999), and indeed Green (1995) found evidence of diffusion of crime prevention benefits from the Oakland police SMART drug policing operation in 1988.

Traditional methods for measuring spatial displacement resulting from crime prevention initiatives have involved the measurement of crime levels in areas adjacent to the intervention area (Barr and Pease, 1990; Berry and Jones, 1995; Hesseling, 1994). For example, Sherman *et al.* (1995) examined gun crime levels in beats adjacent to the intervention beat, which was itself some distance from the control area, finding no evidence of displacement from a police gun crime initiative. There are however methodological limitations to this type of displacement study, and a number of researchers have identified methodological issues with displacement studies (for example Hesseling, 1995; Weisburd and Green, 1995).

First, the testing of neighboring police beats or similar administrative regions contiguous to the intervention site is vulnerable to the Modifiable Areal Unit Problem (MAUP), a potential source of error that can affect spatial studies that rely on inference drawn from data aggregated to spatial boundary units (Bailey and Gatrell, 1995; Openshaw, 1984; Unwin, 1996). The MAUP is a problem related to geographic boundaries and is operationalized as the difference in output and analysis that can occur if different boundaries are used to display and analyze the same data. For example, imagine a city map depicting a deprivation measure aggregated to census tracts across a city. By moving the boundaries and selecting completely different areas as the basis for aggregation of individual level data it would be possible to change the map. Indeed census tracts are usually comprised of smaller geographical units such as enumeration districts or block groups. By selecting different smaller units for aggregation into larger census tracts vastly different maps can be generated.

Secondly, the technique has limited application when the intervention being studied is a city-wide initiative. This latter problem was encountered when attempting to measure spatial displacement resulting from a city-wide burglary reduction initiative in the Australian capital, the example referred to later in this paper. Officers had free reign to operate in any part of the city meaning that it was impossible to determine either an intervention site or contiguous areas for displacement studies. Measuring displacement to neighboring areas beyond the city boundary was not a realistic proposition, given that most of the surrounding area is countryside.

It is worth noting that there is little point in studying any changing crime patterns in the vicinity of a crime reduction intervention if there is no perceived intervention effect in the first place. Bowers and Johnson make this point with a relatively simple intervention effect test that has been applied to crime reduction strategies in the UK (Bowers and Johnson, 2003). The impact from a crime reduction strategy is often best determined through the use of ARIMA interrupted time series analysis (McDowall *et al.*, 1980) with the crime reduction strategy introduced to an established series as a dummy variable. This type of approach has been applied to various crime problems, including vehicle crime (Krimmel and Mele, 1998), property crime (Weatherburn *et al.*, 2001) and a variety of police operations (Chilvers and Weatherburn, 2001; Langworthy, 1989; Novak *et al.*, 1999). If a significant intervention effect is detected, then the analyst is able to continue and explore the possibility of displacement or diffusion within defined boundaries.

This paper develops a new technique that is appropriate for measuring the change in point patterns over time, through the development of random point nearest neighbor distance calculations combined with a Monte Carlo simulation process. Importantly, the test does not rely on the creation or adoption of formal boundaries within the study region, avoiding the MAUP. The technique can be used to determine the existence of significant changes in intra-regional crime patterns, and is therefore applicable to many types of crime reduction intervention studies. The next section describes the use of random point nearest neighbor calculations, and then discusses some of the operational factors to be considered with this type of analysis, geocoding limitations and the necessity to correct for edge effects. The paper then describes the application of the Monte Carlo process before finishing with an application of the technique to an example data set drawn from a police burglary reduction initiative in Canberra, Australia.

3. INTRA-REGION PATTERN CHANGE OVER TIME

3.1. Nearest Neighbor Distances

While first developed in the 1940s and 1950s by botanists, nearest neighbor distances have been used to examine the spatial arrangement of points in a variety of application areas (Davis, 1986). More recently, and more applicable to the crime research environment, CrimeStat, a computer program developed through a National Institute of Justice grant, uses nearest neighbor indices and distances to examine crime point pattern data sets (Levine, 1999). Nearest neighbor statistics can be used to develop a nearest neighbor distance, defined as the mean distance from each point to its nearest neighboring location, and from this a nearest neighbor index with a mean random nearest neighbor distance used as a denominator. The denominator is determined as a ratio of the study region area to the number of points within.

To use the terminology of Bailey and Gatrell (1995), we can start by considering a nearest neighbor *event-to-event* distance W, being in a crime context the distance between one crime location and the nearest neighboring crime location within a study area R. The other type of event that we can consider is the *random point-to-event* distance, X, being the distance between a randomly generated point r within the study area, and the nearest neighboring crime event. To investigate the spatial dependence in a single crime pattern we could examine the observed distribution of either of these nearest neighbor measures. While limited to only measuring variation in event–event or point–event distances, this approach allows for the measurement of first order spatial effects that are a feature of many spatial crime data sets, due to variations in criminal opportunity (Cromwell *et al.*, 1999), socio-economic circumstances (Hagan and Peterson, 1995; Hakim, 1982) and policing concentration (Sherman, 1990).

Calculation of a matrix of nearest neighbor distances, either W or X, resulting in a nearest neighbor index, will only show a measure of spatial dependence for a single crime pattern. In other words, nearest neighbor analysis is usually applied as a global measure of distribution that can tell us if a crime point distribution is randomly patterned, dispersed or clustered, by calculating the mean of the distance between all points and their nearest neighbors and comparing this mean to a theoretical distribution. It cannot however tell us anything about individual patterns within the study area. Nearest neighbor indices can be compared across data sets to determine relative concentration of hotspots in point patterns, but the statistic in this form is unable to determine if comparable data sets have crime concentrations in the same area. Two crime data sets could therefore have the same nearest neighbor index value, indicating the same level of clustering, but have crime concentrations in completely different areas of the study region. If nearest neighbor distances are to be employed to compare the spatial concentration of two or more point patterns, different methodologies are required.

Whereas the normal application of a nearest neighbor analysis generates a mean distance from each point to its nearest neighbor, the approach in this paper compares individual distances between a random point and the nearest point in a crime data set. Bailey and Gatrell (1995, p. 119) suggest a random sample of points within a study region R, and calculate each nearest neighbor *random point-to-event i* distance (the first data set), and each nearest neighbor *random point-to-event j* distance (the second data set). This results in a set of paired point–event distances for each of the random points generated.

The global measure based on average distances is abandoned in favor of a technique that generates a ranked list of nearest neighbor distances from a random point to a crime point. For two data sets sharing the same study area, this means that two ranked lists are generated with the distance from each random point to the nearest point in both data sets. These paired values can then be ranked and compared using a non-parametric rank correlation measure. The value in this method is the sensitivity to second order spatial processes in each data set.

This can be seen in Fig. 1 where example distributions of two crime data sets (triangles and squares) are shown in a study region. Within the study region are four random points shown in circles labeled 1–4. In the first example (example A) the two crime patterns are similar and share the same corner of the study region. The table to the right shown the distance (in arbitrary units) from each random point to its nearest triangle and nearest square. Figures in brackets indicate the relative ranking of each random point within each crime distribution (triangles or squares). In example A, the points from both distributions share a common area and the relative rankings of both distributions with the random points are similar. When the

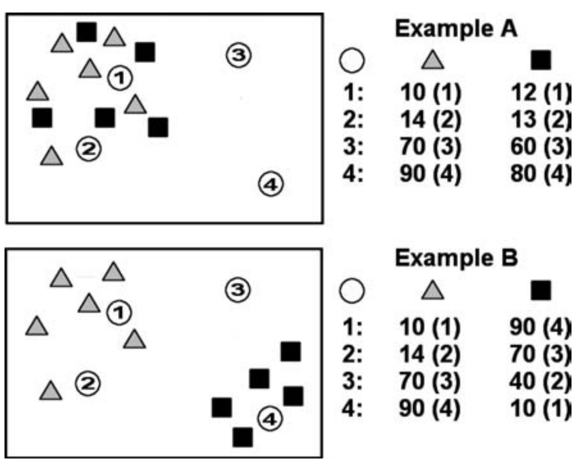

Fig. 1. Example A shows four random points, indicated as circles 1–4, dispersed in a study area with two crime distributions, triangles and squares. The table to the right shows the nearest neighbor distances from each random point to the nearest triangle point and the nearest square point. The distances are in arbitrary units, and the relative ranking of the random point within each crime set (triangles and squares) is shown in brackets. Where the crime points are interspersed and share the same general area of the study region it can been seen (example A) that the relative rankings of the random points is the same for both crime sets. When the two distributions are markedly different, as in example B, the relative rankings of the random points is different to the level where the change could be detected with a statistical test.

distributions are markedly different, as in example B, the nearest neighbor distances are different for triangles and squares and as the figures in brackets indicate, the relative rankings are also different. This is a change that could be detected by a statistical test, such as a Spearman rank order correlation.

Non-parametric tests may seem a little unsophisticated, but the use of correlated rankings neatly sidesteps a potential problem when examining crime distributions, namely the problem of comparing distributions when data sets have different numbers of points. When comparing pre- and post-test crime distributions around a police crackdown, it is anticipated from the literature (Sherman, 1990) that the latter distribution will usually have fewer actual events. This means that nearest neighbor distances would reasonably be expected to be generally larger given the availability of fewer points. In other words, if actual distances were used the greater distances that may result from the expected lower number of points would skew the statistical test, whereas the rankings (relative to random points) prevent the statistical test from being influenced by the ***n*** of the crime distributions. The use of non-parametric tests that compare within group rankings avoids this potential problem by avoiding direct comparison of the nearest neighbor distances from one data set to another.

There is also added advantage in a test that does not rank cases between groups, but ranks within the single crime distribution. Not only does this mean that crime distributions with significantly different numbers of points can be directly compared, but also that the number of random points (n_{ran}) in relation to the number of crime points is not a significant factor in the application of the technique, within limits. Increasing n_{ran} will increase the processing time though increasing the number of points also increases the likelihood that the random point generation process will create a more even distribution of points. Decreasing n_{ran} will reduce the degrees of freedom for a correlation significance test, but will improve processing time.

Decisions regarding the appropriate number of random points used will, to a degree, be a judgment of the researcher, though some guidelines are suggested. Too few points will reduce the degrees of freedom to the level that even substantial displacement may not be detected by the statistical test. Too many random points, especially where the number exceeds the number of points in any crime data set will tend toward multiple sampling of certain points, often outliers. Use of too many random points may over-emphasize the influence of these outliers reducing the opportunity for the test to detect change in the majority of crime points in hotspots or other clustered areas.

The result is a table with n_r rows, where n_r is the number of randomly generated points used in the study, with one column per crime data set showing the nearest neighbor distance from each random point to the nearest point in the data set. When comparing two crime patterns the relative rankings of the random point nearest neighbor distances can be assessed for significance using a rank correlation test such as a Spearman rank correlation coefficient test.

Interpretation of nearest neighbor distances can reveal changes in crime density in a single data set across a study region, however to be an effective measure there are two assumptions. First that all crime events that have occurred within the study region have been geocoded, and secondly that we can correct for edge effects. These assumptions are discussed in the next section.

3.2. Geocoding and Edge Effects

This type of spatial examination makes two implicit assumptions. Firstly the assumption exists that all crime events (of the chosen crime type) have been geocoded and exist within the mappable data set for selection, and secondly that a correction for edge effects has been made.

The first supposition means that they are available to be chosen as a potential nearest neighbor to a randomly generated point. In most cases there will be a limit to the data quality such that some crime events are not geocodable due to a variety of factors (Harries, 1999; Ratcliffe, 2001a). The question therefore arises as to how much geocoding is practically necessary to complete a reasonable spatial study given the data quality realities? One estimate is a minimum of 85% accurate geocoding (Ratcliffe, 2004), a figure derived from an analysis based on a statistical comparison of 100% geocoded thematic maps with a repeating series of maps with progressively fewer geocoded points. However a figure of 85% assumes that there is no significant pattern in the ungeocoded locations and that crime events that are not able to be mapped do not display significant spatial autocorrelation. In reality, a percentage figure in the mid to high nineties is more realistic for a constantly reliable hit rate. In the study that follows, geocoding was successful to at least 97%.

Although computationally intensive with large data sets, correcting for edge effects requires examination of each nearest neighbor distance calculation to ensure that edge effects are not a factor. They can occur when there is the possibility that a nearest neighbor distance for a point–event or event–event calculation near the boundary of the study region R will be biased (Bailey and Gatrell, 1995, p. 90) tending to the possibility that nearest neighbor distances are larger nearer the edge than in the center of the study region. This can occur because events near the boundary are denied the possibility of neighbors that reside just outside the study region.

This potential bias to exaggerate nearest neighbor distances near the border of R has been recognized but not perceived as a significant issue in the analysis of crime data. Edge effects are a common problem in crime analysis because law enforcement analysts rarely have access to crime distributions in neighboring police jurisdictions. Levine (1999, p. 141) suggests that failing to correct for edge effects can actually add a conservative facet to a significance test in a single pattern analysis. This suggestion is made on the basis that many social science data sets show evidence of clustering, therefore a test that exaggerates distances in some areas of R will increase the robustness of any significant clustering discovered.

In this study, however, the reverse argument occurs when comparing the spatial distribution of two point patterns when seeking to determine if the event concentrations occur in the same sub-regions of R. If the two patterns show markedly different distributions in the areas of higher density, then there is the possibility that one pattern will have a crime cluster close to a boundary while a comparable data set will not. Allowing random points to remain close to the boundary may favor a crime pattern that has a concentration of points near the border of R. This would exaggerate the significance of any comparative measure, with the potential to artificially increase the significance of the eventual test result. The distribution that is clustered near the border will benefit from random points near the border (and subsequently have low nearest neighbor distances) while the more dispersed distribution is denied the possibility of points that might fall just outside the study area. This would run the risk of artificially increasing the nearest neighbor distances from random points at the border to the latter distribution.

With a potential for a possibly false increase in one set of nearest neighbor distances, the correlation between nearest neighbor distances for random points at, or near, the border has the potential to be artificially inflated. In a significance test that seeks out differences in the nearest neighbor distances from one set of random point to two distributions, this could result in a false positive outcome. Davis (1986, p. 309) describes edge effects as "a serious defect for most practical purposes". It is therefore advisable to correct for edge effects if possible.

There are a number of possible solutions to edge effects. For general spatial studies, a "guard area" can be constructed around the edge of the study such that points in the guard area are not included in the analysis but can be selected as a nearest neighbor for a crime point within the study region (Gatrell *et al.*, 1996, p. 258). A guard area in this case is an internal boundary created within a study region that cannot be used to generate random points. Random points are therefore excluded from existing at the edge of the study area, though they are allowed to find nearest neighbors from within the guard area. This has the effect of forcing random points away from the edge, reducing the likelihood that a nearest point could be found outside the whole study area.

Difficulties can occur with choosing an appropriate guard area distance that ensures no point selected for analysis is too close to the study boundary while still retaining enough points for a meaningful analysis. The use of a guard area can also be restrictive in that the choice of a guard area is often largely subjective and cannot be adapted for different areas of the study region. In other words, if a 1000 foot guard area is selected, it is generally applied to the whole study region boundary irrespective of whether it is necessary.

This study employs a more adaptive approach by employing a spatial buffer query feature of a GIS.[2] Randomly generated points and their respective nearest neighbor distances can be discarded and a new random point generated for any random points that are closer to a boundary of the study region R than any nearest neighbor point–event distances. This is shown in Fig. 2 where a detail of a city is shown with crime events geocoded around various suburbs are from two crime series, 1 and 2. In this example

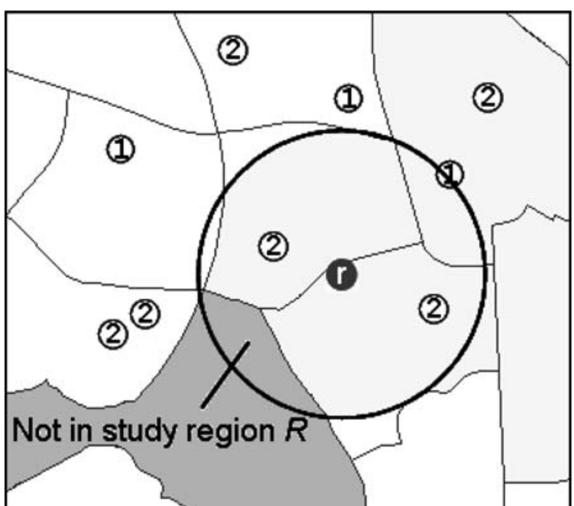

Fig. 2. Crime points from two series (1 and 2) are shown geocoded in a detail of a city's suburbs. A random generated point (r) is shown, with a buffer signifying the greater of the two nearest neighbor distances of r–1 and r–2. When edge effects are considered, points such as this random point would be rejected because part of the buffered area lies outside the study region R. It is possible that a 1 series point could lie closer to the random point r but outside the city boundary R. To correct for this, a new point would be generated and the same test administered.

[2]The two biggest desktop mapping packages in terms of market share (ESRI's ArcGIS and MapInfo) both have standard buffer creation utilities to create a selectable circle around a point with a user-determined radius.

series 1 could be considered a first series and series 2 a second series. A buffer generated around a random point shown as a dark point (r) is created for the larger of the two nearest neighbor distances (r−1 and r−2). The lower left part of the buffered region includes an area that lies outside of the city boundary (study region R). In this case, the random point would be discarded because crime series 1 is denied the possibility of a smaller nearest neighbor distance to a potential point outside the study area but within the circle shown.

This adaptive method allows for variation in the shape of the study region R by not requiring the construction of a guard area that might be excessive in some parts of R while unable to properly prevent edge effects in other parts of R.

It is adaptive because where nearest neighbor distances are small the buffer is smaller allowing random points closer to the edge of the study region. The method is sensitive to the distribution of crime series. In high crime areas the clustering of points from both series will mean that random points in a high crime cluster near the edge of R will have small r−1 and r−2 nearest neighbor distances, allowing random points to be accepted even when close to the border of R.

3.3. Monte Carlo Simulation

The methodology so far describes a process that assesses the correlation of two crime series through the use of nearest neighbor distances to a set of randomly generated points. It is possible that a set of random points could have a highly unusual distribution that finds a high correlation between crime series even when the distribution of the two series is substantially different. Nearest neighbor analyses are sensitive to outliers and this draws into question the reliability of a measure when an unusual distribution of the catalyst random point set can give an anomalous result.

An example of one type of problem is shown in Fig. 3. As with Fig. 2, crime points from two series (1 and 2) are shown geocoded in a detail of a city's suburbs. A random generated point (r) is shown as a dark point. In this example, there is a clear cluster of points from series 1 in the bottom left of the image and a lone point from the same crime series (1) in the top right, close to the random point. The nearest neighbor distance for the random point to the nearest point in series 1 would still be a relatively short distance because there is a lone series 1 point near the random point r even though the random point is some distance from the larger cluster in the bottom left. The nearest neighbor distance for this random point r to series 1 and to series 2 would be small and may achieve a similar rank order in a correlation

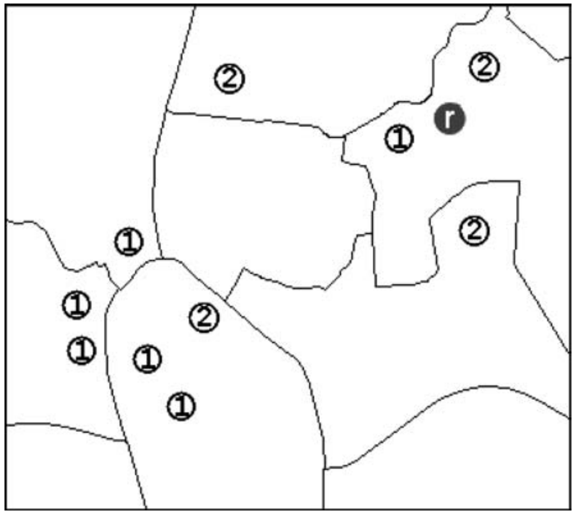

Fig. 3. A random point (r) is plotted some distance from the main cluster from crime series 1, featured at the bottom left of the diagram. The nearest neighbor distance $r\!-\!1$ will still be small however, due to the proximity of a single outlier some distance from the main cluster but close to r. Monte Carlo simulation is used to correct for this potential effect.

test. If more random points had a similar distribution that favored outliers or lone points in one distribution it may be difficult for the rank correlation coefficient test to determine any difference between the spatial densities of the tested series. To correct for the possible effects of an unusual random point distribution, this paper proceeds to describe the use of multiple realizations to better ascertain the actual correlation of two crime series.

The principle of Monte Carlo simulation is the realization that there may be times when you do not have a complete understanding of the processes that might affect the working of a system. In these circumstances, such as the random point generation process described here, it becomes problematic to develop a model to process error and predict an outcome. In essence, a different set of random points may generate a different result. However given that a set of multiple outcomes can be generated through repeated testing it is possible to use the statistical summary of the products to determine some new estimates for the process being tested that conform to the distribution of observed values. The use of Monte Carlo methods are considered appropriate as a means to combat error propagation problems within GIS (Openshaw, 1989).

The problem of different random point distributions can be formulated such that a Monte Carlo process can simulate repeated application of a stochastic process and generate a set of observed values. In the examination of burglary patterns in Canberra, 100 random points (after edge correction) were created in each realization of the Monte Carlo process.

The question arises as to what is an appropriate number of repeated testing (M). The whole process does require computation time, especially with the example data drawn from the Australian capital. Due to the unusual shape of the city of Canberra many points are discarded due to edge effects. A sufficient number of repeated observations must be made to realistically model the stochastic processes under investigation, while excessive testing adds little to the analysis. The number of simulations necessary to properly represent the distribution of the uncertainty factor is a subject of debate. Given that the result of each realization of the Monte Carlo process was a rank correlation coefficient it was possible to graph the coefficient frequency distribution. In the GIS literature values of $M = 50$ have been used (Davis and Keller, 1997), though simulations with as low as 20 runs have achieved statistically significant results (Hope, 1968).

In this study it was determined that 100 realizations of the Monte Carlo process were sufficient to allow for a normal distribution for the observed values of the Spearman correlation test, with absolute skew and kurtosis values less than 1.0. When interpreting the results, a statistically significant positive correlation indicates that the two distributions are in the same areas and therefore is interpreted as no pattern change, while a significant negative correlation indicates real movement in crime patterns. As such, the distribution of random points is critical to the test and increased numbers of realizations reduces the potential for an unusual random point distribution skewing the results. If time were pressing a value less than $M = 100$ would be acceptable, though this study emphasizes caution. Repeated testing in this manner removes the impact of any peculiarities due to unusual random selection processes in the generation of testable points, and allows conclusions to be drawn from the aggregated results of all realizations of the Monte Carlo process. The result is a normally distributed series of M Spearman rank correlation coefficients, where M is the number of simulations operationalized.

Increasing the value of M might be necessary to achieve a normally distributed set of outcomes, and the user could employ a test such as a Kolmogorov–Smirnov test to ascertain if sufficient realizations have been conducted such that a desired distribution of outcomes existed.

Once the user has a normally distributed series of M Spearman rank correlation coefficients (where M is the number of Monte Carlo simulations completed), the results must be interpreted to determine significance. As each simulation uses the same number of random points, the degrees of freedom will be the same and the test statistic for each run will be the same.

If a chosen level of significance is decided on, for example $P < 0.05$, then each individual run can be examined for statistical significance. Combining a number of observations from the Monte Carlo simulation for an overall significance test is a relatively simple next step. A histogram or table can be constructed to examine how many runs have achieved an individual level of significance at the chosen level (in this example $P < 0.05$). If in this example we remain with the 0.05 level then the result of the Monte Carlo simulations is statistically significant if 95% of the runs achieve the test statistic level.

In summary, this technique solves the problem of examining point pattern changes when points are aggregated to areal units such as police beats, census tracts or even regular grids as the nearest neighbor approach here is not vulnerable to the MAUP. Furthermore, the use of a random point generation of a more systematic point generation process has additional advantages. Although intuitively using a grid or similar artifice to distribute the points created as the basis for the nearest neighbor test, this type of systematic sampling process is vulnerable to the problem of "patterning" (Ebdon, 1996, pp. 42–44). This occurs when a regular pattern is placed over a similarly regular pattern of urban geography and is especially a potential problem in the regular patterns of US cities. For example, if robberies were highly clustered around major road intersections spaced at 1 km distance, a regular grid with a resolution of 1 km would potentially either miss every major junction, or exactly sample every major junction. A random point generation process resolves this problem. We now move to a demonstration of the technique.

4. EXAMPLE: BURGLARY IN CANBERRA

Located some 200 miles South of Australia's largest city Sydney, Canberra is the Australian capital. The city has a population of approximately 330,000 nestled in the hills of the Australian Capital Territory, an administrative region set up in the early part of the last century to house the capital city. The city has high levels of education and a significant proportion of the city's workforce are employed by the Federal government. The city also has a burglary problem. In 2000 there were 2494 burglaries for every 100,000 residents, the third highest rate of the eight Australia states and territories (ABS, 2001).

Policing of the Australian Capital Territory is contracted to the Australian Federal Police (AFP). In response to the rising burglary problem, the AFP undertook Operation Anchorage in the early part of 2001. Anchorage was a burglary reduction strategy that aimed to reduce burglaries by 20% on the previous year. The operation lasted approximately 4 months.

A number of law enforcement strategies were undertaken, including use of surveillance teams deployed to observe prolific offenders, use of random breath test road blocks in high burglary areas, execution of warrants for suspected offenders and any other tactics the operational management felt might be effective. The management were supported by a six person intelligence team dedicated to the operation. These individuals would analyze crime and map incidents across the city, enabling the operational management team to deploy resources at the most effective time and places.

Unlike in the United States, in Australia there is no real distinction between crime analysis and intelligence at the operational police level, so the intelligence staff were involved in a variety of activities including link association charting, crime mapping, and temporal analysis. While the number was variable, the operation had approximately 60 staff dedicated to Anchorage during the 4 months of operation, a considerable commitment for a force of approximately 600 officers.

While the management had the option to deploy resources across the whole city, the use of mapping and intelligence techniques gave the operation a spatial focus. This variable enforcement level was determined by the desire to focus resources in the worst hit areas, though these areas changed dependent on criminal behavior. The question of spatial displacement arose during discussions of the operation. While Canberra is generally surrounded by countryside (with the exception of the city of Queenbeyan to the South-East), displacement within the city was a consideration. Were the police upsetting the normal "business plan" of offenders, forcing them to target different areas from normal? The questions to be answered was; "Are within-city burglary spatial patterns changing as a result of Operation Anchorage?"

The difficulty with answering this question in a quantitative sense is demonstrated by Fig. 4. The two maps show hotspot surface maps of burglary in the more than 100 urban suburbs of the Australian Capital Territory in the 4 weeks prior to Operation Anchorage (left) and after the operation had been conducted for 4 weeks (right map). While the decrease in overall hotspot significance is to be expected, there are four distinct areas where it is possible some spatial change has occurred. In the area marked A, there appears to be a substantial decrease in activity in a number of suburbs. In the broader area of B, there has been some decrease, but also development of a new noticeable hotspot in one location, in the heart of a suburb that only had a moderate level of burglary in the earlier map.[3] Does this

[3]As a reviewer pointed out, hotspots of one crime distribution close to the borders of the study area could potentially be affected by the edge correction process. In the case of the noticeable new hotspot in the second map at area B, this hotspot is far enough from the borders of the study area to be unaffected by the edge correction process, which works closer to the edge of the city.

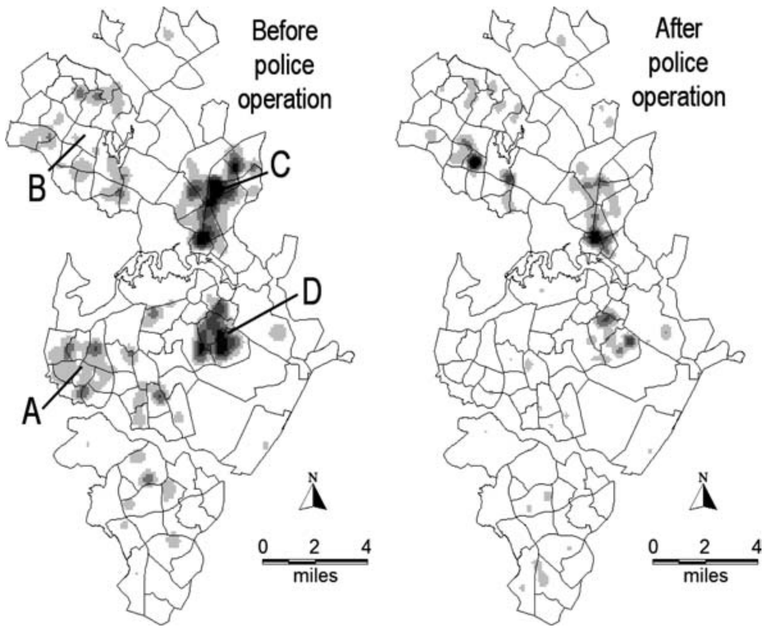

Fig. 4. Two hotspot maps of burglary in the ACT, one a snapshot of the 4 weeks of burglary prior to the police operation (left) and one a snapshot of burglary during the first 4 weeks of the operation (right).

indicate a spatial change resulting from the operation? Is this a "real" change? Areas C and D show decreases in burglary, with the possibility of some slight movement. The difficulty with a simply visual interpretation is the subjectivity of the analysis. Although there would appear to be some substantial decreases in activity, has there also been significant movement?

4.1. Testing Displacement Around Operation Anchorage

The first stage is to confirm that an actual crime reduction took place (Bowers and Johnson, 2003). It was therefore established that a positive intervention effect occurred, and that the reduction in crime was significant (Ratcliffe, 2001b). The next stage explores the possibility of changes in crime patterns resulting from the police initiative.

The technique explained in this paper was applied to city burglary patterns for four distinct crime series, each 28 days in length. The first three periods were the three 28 day consecutive periods immediately prior to the commencement of Operation Anchorage. The last period (4) was the first

Table I. Reported Burglaries in the Australian Capital Territory for the Four 28 Day Periods Examined in this Study

Period	Dates	Reported burglaries
Period 1	4 Dec 00–31 Dec 00	584
Period 2	1 Jan 01–28 Jan 01	556
Period 3	29 Jan 01–25 Feb 01	661
Operation Anchorage begins		
Period 4	26 Feb 01–25 Mar 01	386

28 days of Operation Anchorage. Table I described the four time periods and shows the number of burglaries reported across Canberra during each time.

In this example we explore if the first 4 weeks of Operation Anchorage were accompanied by spatial displacement of burglary targets. This question is of relevance to police agencies undertaking crackdowns in the name of crime prevention. If police activities are concentrated in a number of areas, will a reduction in crime also be associated with displacement of the crime that does remain? Traditional measures of displacement that rely on measuring levels of crime in discrete areas are not suitable in a situation where the AFP commanders were able to deploy resources to any part of the city in response to timely intelligence analysis of the crime problem. The technique described earlier was therefore developed to perform the analysis.

Each of the four periods (Table I, 1–4) were compared with the remaining three across 100 random points, with 100 realizations of the Monte Carlo process. Each simulation generated a rank order correlation. Fig. 5 shows a histogram of the Spearman rank correlation coefficient values for 100 simulations of the technique. The 100 simulations were the application of the random point nearest neighbor measurements between the period 3 data (immediately prior to the police operation) and the period 4 data (the first 4 weeks of the operation). With a test statistic of 0.165 (one-tailed, $P = 0.05$, Ebdon, 1996, p. 219) each realization produced a significant correlation between the point patterns to at least $P = 0.05$. While two distributions produced negative skew calculations slightly larger than -1.0, the kurtosis remained lower than 1.0 and, more importantly, all correlations produced Spearman rank correlation coefficient values significant to at least $P = 0.05$.

From these results we can determine that the spatial pattern of burglaries during the first 4 weeks of Operation Anchorage does not appear to vary significantly from the spatial patterning of offences in the preceding three 28 day periods (Table II).

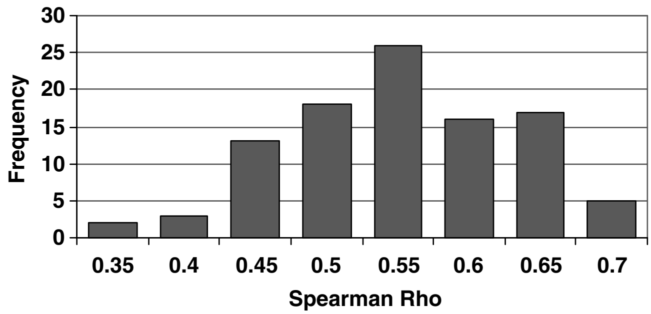

Fig. 5. Histogram of the Spearman rank correlation coefficient values for the 100 simulations that compared rank correlations between random point nearest neighbor distances for the period 3 data (immediately prior to the police operation) and the period 4 data (the first 4 weeks of the operation). Series has a mean of 0.525, standard deviation of 0.081, skew of −0.17 and kurtosis of −0.65, indicating an acceptably normal distribution.

Table II. Correlation Matrix Showing the Mean Correlation Coefficient of 100 Realizations of the Monte Carlo Process for Nearest Neighbor Random Point Analysis of the Three Time Periods Prior to Operation Anchorage, and the First 4 Weeks of the Operation

	Period 2	Period 3	Period 4
Period 1	0.562	0.574	0.543
Period 2		0.542	0.532
Period 3			0.525

With a test statistic of 0.165 at the 95% significance level, each realization produced a significant correlation to at least $P = 0.05$, while the mean values display a greater significance level.

5. CONCLUDING COMMENTS

This paper describes a new approach developed to test a specific question of spatial displacement from a city-wide police crackdown, or generally the movement of crime patterns within single areas. City-wide police operations take place around the world but are often subjected to a number of stock criticisms. These include the notion that crime will be geographically displaced, that the problem will return once the police crackdown ends, or that the crackdown is merely a political gesture. This technique specifically addresses one of these criticisms, that of spatial displacement. This is a common concern, even though there is scant evidence that displacement is a significant problem (Barr and Pease, 1990; Hesseling, 1994). Eck, in his chapter on prevention of crime at places, notes that where evidence can be found for displacement; ''displacement seldom overwhelms prevention effects'' (Sherman *et al*., 1998, Chapter 7).

Police departments are often keen to implement city-wide initiatives. Even though operations targeted at high risk places, people and times have been shown to be more effective than indiscriminant tactics (Sherman *et al*., 1998, Chapter 8), city-wide initiatives have the advantage that they do not appear to be favoring particular communities within the city. Furthermore, city-wide law enforcement initiatives are easier to sell to the rank and file officers, again with the view that no particular district or precinct is seen to be favored. These are realistic political considerations for a police chief. Police crackdowns therefore remain popular. Given the findings from Canberra, other areas may take some solace that displacement within the city does not necessarily occur.

In the example shown, the results showed a marked degree of similarity. This is to be expected as there is little evidence for displacement in the literature. As such, the interpretation of the graph in Fig. 5 is relatively easy. However, two distributions with significantly different spatial patterns would result in lower correlations because some random points would be closer to point clusters in one distribution and further from points in other distributions. This increases the chance that the normally distributed results would intersect with the P value that would indicate significant correlation (in the example here, 0.165). It would probably be reasonable to assume that the point patterns were not significantly different if more than 90–95% of the results were above this threshold, suggesting a relatively low combined error rate. Increased numbers of results from the Monte Carlo simulation that were below the threshold would begin to suggest significant changes in patterns.

The technique demonstrated does require some computer programming effort, however given that a number of researchers have noted methodological issues with displacement research, a more complex solution to displacement studies may not be popular with practitioners, but necessary.

From a research and practitioner perspective, the process shown here would constitute the middle stage in a larger study. The first stage, as discussed earlier, would be to establish if there has been any positive intervention effect that could cause the movement of crime patterns. The process shown here could then be applied to measure any significant crime pattern movement. If any were detected, the next stage would be to map and explore areas of crime movement and attempt to identify the processes and underlying reasons for the movement of the crime patterns. The procedure in this paper is therefore not a complete methodology, but a piece is a larger quantitative process.

ACKNOWLEDGEMENT

The author is grateful to a number of reviewers for their helpful comments.

REFERENCES

Australian Bureau of Statistics [ABS]. (2001). *Recorded Crime Australia 2000*. Australian Bureau of Statistics, Canberra.

Bailey, T. C., and Gatrell, A. C. (1995). *Interactive Spatial Data Analysis*, 2 edn. Longman.

Barr, R., and Pease, K. (1990). Crime placement, displacement, and deflection. In Tonry, M., and Morris, N. (eds.), *Crime and Justice: An Annual Review of Research*, Vol. 12. University of Chicago Press, Chicago.

Berry, W., and Jones, H. (1995). Investigating spatial patterns of crime through police statistics, crime surveys and census profiles – findings from Dundee housing estates. *Scot. Geogr. Mag.* 111(2): 76–82.

Bowers, K. J., and Johnson, S. D. (2003). Measuring the geographical displacement and diffusion of benefit effects of crime prevention activity. *J. Quant. Criminol.* 19(3): 275–301.

Chilvers, M., and Weatherburn, D. (2001). Do targeted arrests reduce crime? *Contemporary Issues in Crime and Justice* (NSW Bureau of Crime Statistics and Research), Paper 63, p. 16.

Cromwell, P., Olson, J. N., and Avary, D. A. W. (1999). Decision strategies of residential burglars. In Cromwell, P. (ed.), *In their Own Words: Criminals on Crime*, 2nd edn. Roxbury, Los Angeles.

Davis, J. C. (1986). *Statistics and Data Analysis in Geology*, 2nd edn. John Wiley, London.

Davis, T. J., and Keller, C. P. (1997). Modelling uncertainty in natural resource analysis using fuzzy sets and Monte Carlo simulation: Slope stability prediction. *Int. J. Geogr. Inf Sci* 11(5): 409–434.

Ebdon, D. (1996). *Statistics in Geography*. Blackwell, Oxford.

Gatrell, A. C., Bailey, T. C., Diggle, P. J., and Rowlingson, B. S. (1996). Spatial point pattern analysis and its application in geographical epidemiology. *Trans. Inst. Br. Geogr.* 21(1): 256–274.

Green, L. (1995). Cleaning up drug hot spots in Oakland, California: The displacement and diffusion effects. *Justice Quart.* 12(4): 737–754.

Hagan, J., and Peterson, R. D. (1995). Criminal inequality in America: Patterns and consequences. In Hagan, J., and Peterson, R. D. (eds.), *Crime and Inequality*. Stanford University Press, Stanford.

Hakim, C. (1982). The social consequences of high unemployment. *J. Social Policy* 11: 433–467.

Hakim, S., and Rengert, G. (1981). Introduction. In Hakim, S., and Rengert, G. (eds.), *Crime Spillover*. Sage, Beverly Hills, CA.

Harries, K. (1999). *Mapping Crime: Principles and Practice*. US Department of Justice, Washington DC.

Hesseling, R. (1994). Displacement: A review of the empirical literature. In Clarke, R. (ed.), *Crime Prevention Studies*, Vol. 3, Criminal Justice Press, New York.

Hesseling, R. (1995). Theft from cars: Reduced or displaced? *Eur. J. Criminal Policy Res.* 3(3): 79–92.

Hope, A. C. A. (1968). A simplified Monte Carlo significance test procedure. *J. R. Stat. Soc. Ser. B* 30: 583–598.

Krimmel, J. T., and Mele, M. (1998). Investigating stolen vehicle dump sites: An interrupted time series quasi experiment. *Policing: An Int. J. Police Strat. Manage.* 21(3): 479–489.

Langworthy, R. H. (1989). Do stings control crime? An evaluation of a police fencing operation. *Justice Quart*. 6(1): 27–45.

Levine, N. (1999). *CrimeStat: A Spatial Statistics Program for the Analysis of Crime Incident Locations*, Version 1.1. Ned Levine and Associates, VA and the National Institute of Justice, Washington, DC.

McDowall, D., McCleary, R., et al. (1980). *Interrupted Time Series Analysis*. Sage, Thousand Oaks, CA.

Novak, K. J., Hartman, J. L., Holsinger, A. M., and Turner, M. G. (1999). Effects of aggressive policing of disorder on serious crime. *Policing* 22(2): 171–190.

Openshaw, S. (1984). The modifiable areal unit problem. *Concepts Tech. Mod. Geogr*. 38: 41.

Openshaw, S. (1989). Learning to live with errors in spatial databases. In Goodchild, M., and Gopal, S. (eds.), *Accuracy of Spatial Databases*. Taylor and Francis, Bristol, PA.

Ratcliffe, J. H. (2004). Geocoding crime and a first estimate of an acceptable minimum hit rate. *Int. J. Geogr. Inf. Sci*. 18(1): 61–73.

Ratcliffe, J. H. (2001a). On the accuracy of TIGER-type geocoded address data in relation to cadastral and census areal units. *Int. J. Geogr. Inf. Sci*. 15(5): 473–485.

Ratcliffe, J. H. (2001b). Policing urban burglary. *Trends and Issues in Crime and Criminal Justice*, paper 248. Australian Institute of Criminology.

Sherman, L. W. (1990). Police crackdowns: Initial and residual deterrence. In Tonry, M., and Morris, N. (eds.), *Crime and Justice: An Annual Review of Research*, Vol. 12. University of Chicago Press, Chicago.

Sherman, L. W., Gottfredson, D., MacKenzie, D., Eck, J., Reuter, P., and Bushway, S. (1998). *Preventing Crime: What Works, What Doesn't, What's Promising*. National Institute of Justice, Washington, DC.

Sherman, L. W., Shaw, J. W., and Rogan, D. P. (1995). *The Kansas City Gun Experiment*. National Institute of Justice, Washington, DC.

Town, S. (2002). *Crime Displacement, the Perception, Problems, Evidence and Supporting Theory*. www.crimereduction.gov.au/skills10.htm, accessed February 2002, 12 pages.

Unwin, D. J. (1996). GIS, spatial analysis and spatial statistics. *Prog. Human Geogr*. 20(4): 540–551.

Weatherburn, D., Lind, B., et al. (2001). The short-run effects of economic adversity on property crime: An Australian case study. *Aust. New Zealand J. Criminol*. 34(2): 134–148.

Weisburd, D., and Green, L. (1995). Measuring immediate spatial displacement: Methodological issues and problems. In Eck, J. E., and Weisburd, D. (eds.), *Crime and Place*, Vol. 4, Criminal Justice Press, Washington, DC.