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CLOSING OFF OPPORTUNITIES FOR CRIME: AN EVALUATION OF ALLEY-GATING

ABSTRACT. A new situational crime prevention measure recently introduced into Great Britain involves the fitting of gates to alleyways running along the back of terraced properties to restrict access to local residents and reduce opportunities for offenders. A number of quantitative techniques were used to assess the success of the intervention in reducing burglary in the City of Liverpool. The results demonstrate that, relative to a suitable comparison area, burglary was reduced by approximately 37%, there was a diffusion of benefit to properties in the surrounding areas, and the scheme was cost beneficial with a saving of £1.86 for every pound spent. The analyses provide persuasive evidence that these reductions were attributable to the intervention. We argue that the methodological techniques demonstrated here can be applied more widely to crime prevention evaluations.

KEY WORDS: evaluation, situational crime prevention, diffusion of benefit, cost benefit analysis

INTRODUCTION

Interventions aimed at reducing crime take many shapes and forms. These include approaches that seek to change the behaviour of offenders and those at risk of offending (diversion programmes), the deterrence of offending behaviour through harsh penalties or other consequences on conviction and, the reduction of opportunities for crime through crime prevention.

Choosing the most appropriate intervention or combinations of approach requires not only a thorough understanding of the nature of the crime problems to be tackled, but also, an awareness of what ameliorative strategies are likely to be effective, under what conditions and at what cost. Since the early 1980s evaluations of situational crime prevention measures have shown that a range of such measures are effective at reducing crime. A comprehensive review, by Sherman et al. (1997) compiled information from these evaluations on what were, to date, the most promising approaches.

Whilst in-depth analyses of the prevalence, incidence and concentration of crime will undoubtedly help in profiling local crime problems, further analyses need to be conducted to identify the impacts (both anticipated and unexpected) of crime prevention initiatives on crime and other outcomes. To do this effectively, evaluators require not only comprehensive, accurate and timely crime data (preferably with precise geo-coding) but also, robust information on the delivery of crime prevention measures on the ground. The latter needs to include details of the measures themselves (for example,

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locks fitted to properties, closed-circuit television cameras installed), how many were provided, where they were targeted and when they were administered. Even if these data are provided, analytical techniques still need to be applied to explore relationships between the timing and intensity of measures and changes in crime.

The extent to which bespoke techniques have been developed that can be used to attribute changes in crime to prevention measures, to identify crime displacement and crime switch and to calculate the cost effectiveness of programs remains fairly limited. Through their involvement in several major evaluations of situational crime prevention initiatives in the UK, the authors have developed a set of methods for measuring the impact of interventions on crime and their associated positive and negative spill-over effects. This paper describes the application of such techniques to evaluate the effectiveness of one innovative situational crime prevention measure in north-west England.

Among other things, situational crime prevention involves the removal or blocking of opportunities for offending in the physical environment (Clarke 1992). Typical examples include the upgrading of physical security measures at vulnerable households, commonly referred to as *target hardening* (e.g. Anderson et al. 1995), incorporating anti-theft devices into mobile phones (Clarke 1995), and the installation of immobilisers in automobiles. Many different types of scheme have been evaluated, with mixed results.

The most successful approaches appear to be those informed by research and the potential mechanisms by which they should work understood *a-priori*. To illustrate this, research demonstrates that a few victims account for a large proportion of crime (e.g. Farrell 1995) and hence that victimisation is a good predictor of future risk (for a review, see Pease 1998). Results also show that repeat crimes occur swiftly (e.g. Polvi et al. 1991) and are more prevalent in poor neighbourhoods (e.g. Johnson et al. 1997). Consequently, many schemes have concentrated effort into protecting recent victims of crime, typically with impressive results (e.g. Anderson et al. 1995; Forrester et al. 1988; Johnson et al. 2001). For one pilot study conducted in Kirkholt in the north of England, burglary was reduced by approximately 70% in the first year (Forrester et al. 1988). Other schemes, such as those that improve street lighting (Farrington and Welsh 2002), have also been shown to realise considerable reductions in crime (an average of 20%) but not quite as dramatic as those for repeat victimisation strategies.

In the current paper we consider a relatively new form of situational crime prevention gaining widespread popularity in the UK, which, as far as the authors are aware, has yet to be systematically evaluated. *Alley-gating* involves the installation of hardwearing lockable gates across the alleyways to the rear of terraced (or other) properties. There is an abundance of these types of dwellings, particularly in British industrial cities. Many of them were built in Victorian times and have relatively narrow alley ways or 'entries' that can easily be spanned by the fitting of gates. Importantly, because the alleyways service an entire block of housing, gating-off the alley will protect all the houses within the block. The number of houses that are protected by each instalment of alley-gates will vary depending on the particular structure of the alleyways being protected. Generally however, they will protect two blocks of housing on either side of an alleyway, and, for the sample we used in the current research, each block with a median of 134 houses.

Keys to the gates are provided only to residents who live in the houses secured by the gates (for more details, see Johnson and Loxley 2001). The rationale underlying the intervention is that by restricting access to the rear of properties to residents alone, opportunities for offending will be reduced. Given that for a high proportion of incidents of burglary, offenders gain access via the rear of a property (e.g. Kershaw et al. 2000), alley-gating has the potential to reduce burglary significantly. Given the hard-wearing nature of the alley-gates (those implemented in our sample have an predicted lifetime of ten years) it is expected that the gates will lead to a long-term reduction in levels of crime. In theory, this reduction should be consistent over the lifetime of the gates. In practice, there may be some variation over time and area in the care taken by residents when using the gates (do they always lock the gates behind them? Do they lend others the key?). However, there was no evidence of such usage variation within the lifetime of the evaluation undertaken here.

As we shall see later, the implementation of multiple alleygating schemes are often phased over time. This is because the implementation process is fairly intense. In order to install the gates, the implementers need to gain the permission of all residents who will be affected, which can involve a lengthy public consultation process. Furthermore, there can be problems with gating if the alley is a public right of way. Gates have to be manufactured and obviously adapted to fit the particular alley. All in all, planning for one scheme can start over a year before actual implementation.

Whilst the primary objective of such schemes is to reduce burglary, they also have the potential to reduce other types of crime and disorder. Elsewhere we consider the impact of the scheme on residents (S.D. Johnson et al., unpublished manuscript), and focus here on the effectiveness of the scheme on levels of crime within and around the action area. However, before presenting the results, a number of conceptual and methodological issues warrant consideration.

The first relates to the data used to monitor changes in crime. The types of data typically used either are secondary data routinely recorded by police departments, or primary data collected using victimization surveys as part of an evaluation. The majority of studies have examined changes in patterns of crime reported to the police (recorded crime). Where these data have been geo-coded, with a precise geographical coordinate for each offense, detailed analyses can be conducted of changes in the spatial and temporal distribution of crime using a geographical information system (GIS).

Other studies have used victimization surveys (e.g. Painter and Farrington 1997). This method has the advantage of presenting the opportunity to capture information on crime and disorder not reported to the police,[1] residents' fear of crime and other factors. However, as crime is a low frequency event, large sample sizes are required to generate reliable results. Moreover, with the exception of very large-scale studies (such as Ekblom et al. 1996), analyses of changes in the spatial distribution of crime are generally unreliable where victimization surveys are the only source of data. With these qualifications in mind, the value of employing both methodologies should be clear.

A related issue is the logic of the quasi-experimental designs frequently used in evaluation research. In a comprehensive review of the literature, Sherman et al. (1997) developed the Maryland Scale for assessing the methodological adequacy of evaluations. In an ideal evaluation, they recommend that action and comparison areas would be selected randomly to reduce the possibility that factors other than the interventions themselves could have produced any effects observed (level 5 on their scale). However, as action areas are, in practice, rarely selected in this way – typically resources are distributed on the basis of need – many evaluations adopt a quasi-experimental design. Here the *counter-factual* (what would have happened in the absence of the intervention) is derived from contrasting the before and after crime rates for both treatment and suitable comparison areas to determine the effect(s) of the scheme(s). The rationale being that if the interventions had not been implemented, the changes observed in the action areas would have been commensurate with those in an appropriate comparison area. This type of analysis represents the minimum standard of methodological quality acceptable according to Sherman et al. (1997) (level 3 on their scale). In reality, many evaluations cannot reach the higher levels of the Maryland scale because the circumstances in which measures are implemented prohibit the use of the methods necessary to attain those levels.

A further technique, complementary to the before and after approach, compares the dosage or intensity of a particular intervention with the

[1]Results of the British Crime Survey suggest that up to 75% of crime is not reported to the police (Kershaw et al. 2000). The extent of this problem varies by crime category. For instance, 84% of incidents of burglary with loss *are* both reported to and recorded by the police.

response observed (e.g. see Ekblom and Pease 1995). The rationale, based on a variant of medical models, is that if an approach is successful then (up to a point) increasing the dosage should increase the effect observed (see also, Bowers et al. 2004) – a reduction in the crime rate. Despite the power of this approach, only a few evaluations (Bowers et al. 2004; Ekblom et al. 1996) conducted to date have adopted this design. Thus, if we are to fully understand 'what works' in crime prevention, more evaluations are needed that adopt these scientific standards.

One possible negative consequence of spatially concentrated crime prevention schemes that has frequently plagued policy-making, is the threat of crime displacement (Barnes 1995). Six different types of displacement can occur (see Repetto 1974), these being: temporal – where offenders commit crimes at different times of the day; tactical – where offenders adopt a different modus operandi; target – where offenders select a different type of target (e.g., different types of housing); type of crime – where offenders choose to commit a new type of crime; geographical – where offenders target new locations; and, perpetrator – where apprehended offenders are replaced by new ones. Different forms of displacement are, of course, not mutually exclusive. However, some forms of displacement may not always represent a completely negative outcome. For instance, where offenders commit less serious types of crime and hence the consequent offending behaviour is more socially acceptable, a form of displacement that Barr and Pease (1990) refer to as benign displacement.

Moreover, research demonstrates that where displacement does occur, it is rarely absolute (for a review, see Eck 1993). In fact, a considerable body of evidence suggests that where crime prevention schemes successfully reduce crime in an action area, a diffusion of benefit (Miethe 1991), where the crime reductive effects of a scheme extend to those nearby but outside the operational boundary of a scheme, is the more likely outcome (Eck 1993; Hesseling 1995). However, it is important to note that a variety of different techniques have been used to measure displacement (and diffusion of benefit), some better than others, which may affect the conclusions reached. Consequently, two of the current authors (Bowers and Johnson 2003) proposed a new method and descriptive statistic, the weighted displacement quotient (WDQ), to be used in evaluation research. Importantly, the adoption of a consistent approach to measuring these phenomena should make it possible to make reliable comparisons across a range of different types of scheme in future research.

For the current intervention, a number of types of displacement, or diffusion of benefit are particularly plausible. For instance, prevented from gaining access to properties via the alleyway, offenders might attempt to gain entry via the front of houses thereby changing their modus operandi (MO). However, this might attract attention and increase the likelihood of apprehension or identification, consequently limiting the likelihood that this would occur. Perhaps more plausible, is the possibility that offenders would simply commit burglary at nearby properties in other blocks of housing not protected by alley-gates (geographical displacement). They may also be deterred from committing burglary but instead engage in other similar types of property crime, such as theft from vehicle.

However, it is equally possible that a diffusion of benefit might result. For instance, restricting access to the alleyways will limit both entry *and* exit points. This reduces the number of potential escape routes for each property protected, perhaps increasing the risks involved to a level that may be unacceptable to many offenders. Thus, even offenders whose preferred MO is to gain access via the front of a property may be deterred from targeting houses protected by alley-gating. Other possibilities exist.

Recently, there also has been increased interest in not just identifying what works in crime prevention, but what effect-sizes are typically associated with different interventions. One approach to doing this involves the derivation of scheme outcomes, expressed as an estimate of the number of crimes deterred by preventive action. The advantage of translating scheme outcomes into such a metric, compared to other statistical measures such as the mean change or an odds-ratio, is that it offers the opportunity to conduct cost benefit analyses (CBA). It is beyond the scope of the current paper to discuss CBA in any detail, but put simply, for this type of analysis the costs associated with a scheme are compared with an estimate of the monetary value of the outcome (in this case, a reduction in crime) realised.[2] Where the former are less than the latter a scheme is said to be cost effective. As with the measurement of displacement, a number of different approaches have been adopted to quantify scheme outcomes, making comparisons across schemes impracticable. For this reason, the authors recently proposed a standardized method (Johnson et al. 2004), which will be adopted here.

To recapitulate, in the current paper we evaluate the effectiveness of an alley-gating scheme. To do this, we examine changes in patterns of burglary using both a quasi-experimental before and after design, and a dose-response analysis. In addition to examining the direct effects of the scheme, analyses are presented to determine whether or not there was evidence of some of the forms of displacement or diffusion of benefit. To further inform these analyses, using data collected relating to the cost of implementation, a rudimentary cost benefit analysis also is conducted. To complement the approach presented here, analyses of a recipient survey are reported elsewhere (S.D. Johnson et al., unpublished manuscript).

[2] The monetary value of different types of crime has been estimated by the Home Office.

DATA

Recorded Crime Data

To examine changes in the pattern of crime, spatially and temporally referenced burglary data were obtained from the Merseyside County Police Department. These data covered both a historic period (1 January 1998–31 December 1999) and the period of implementation (1 January 2000–31 June 2003). Each burglary record contained the following fields of information: a) unique crime reference number; b) address of the offense; c) grid reference of offense (*x* and *y* coordinates); d) date of offense; f) time of offense; g) the type of property victimized (e.g. terraced house); and h) the point of entry (e.g. back door).

Implementation Data

Data were also acquired regarding the exact location and timing of the installation of alley-gates. The gates installed protected 106 distinct blocks of adjacent housing, typically containing around 362 residential properties (S.D. = 417, range 6–3190). For the period evaluated, a total of 3178 gates were installed. The geographical boundaries for these areas, shown as Figure 1, were identified and digitised using a geographical information system (GIS).[3] All of the alley-gates considered in this analysis are located in the City of Liverpool in Merseyside County, north-west England. In relation to the timing of implementation, data were available regarding the dates on which installation was completed at each block.

RESULTS

Effects on Burglary

Using a GIS, for each quarterly time period it was possible to identify how many burglaries were conducted in the alley-gated areas and in a comparison area. Whilst a variety of comparison areas may be used, perhaps the simplest approach is to use the wider police department area (PDA) in which the scheme is located. Some of the advantages of using this area are as follows: 1) it is easily identifiable and easily accessible crime data exists at this level; 2) policy changes in policing (and to some extent for other public sector organisations) will be identical for both the scheme and comparison areas; and, 3) this area is large enough for trends to be reliable. In some ways, using the PDA may be thought of as considering a series of comparison areas which are used to determine the general trend

[3] These boundaries were produced by Mott MacDonald, a local information agency.

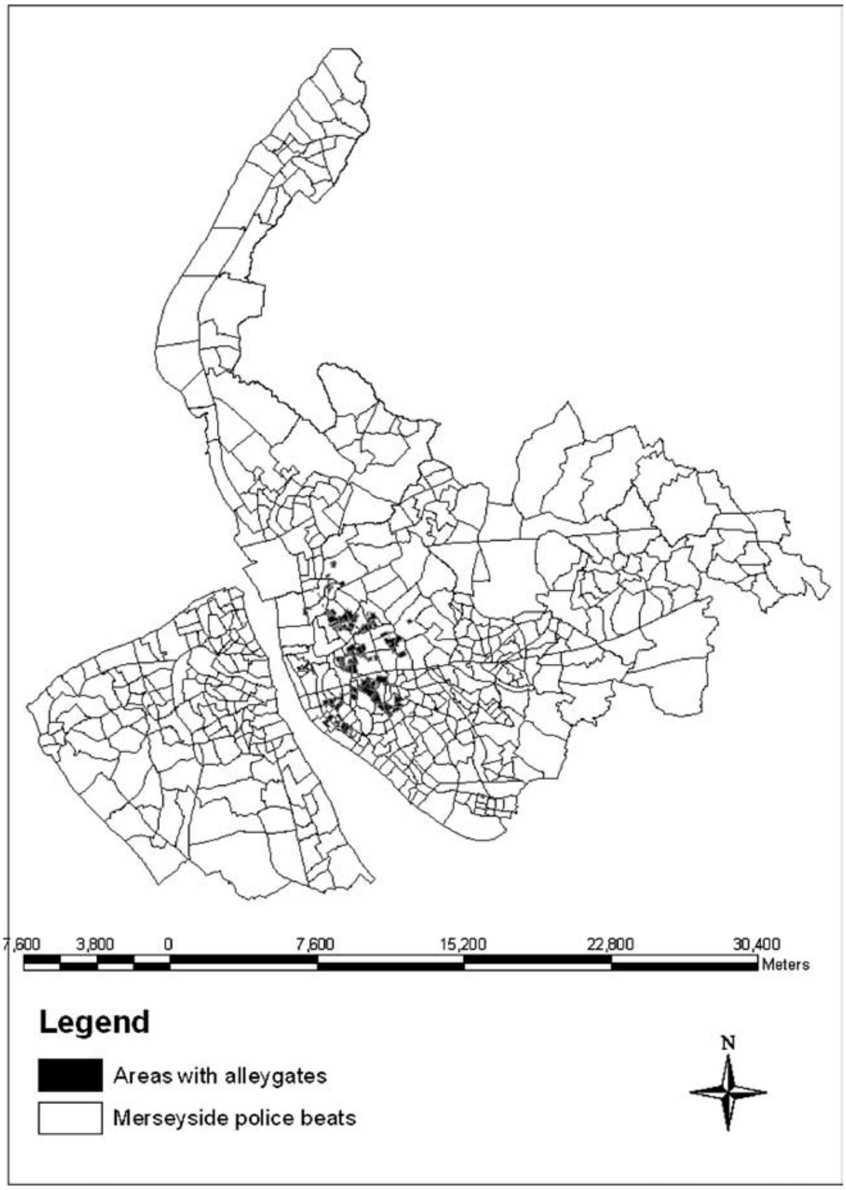


Figure 1. Map of Merseyside police beats and alley-gated areas.

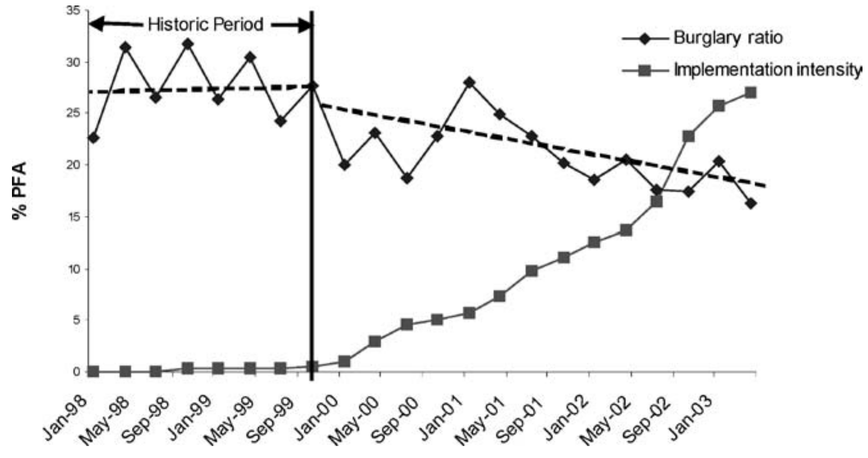
observed in the surrounding area – the purpose of using comparison areas (for examples of where this approach has been adopted elsewhere, see Griswold 1984; Johnson and Bowers 2003; McGarrell et al. 2001). Consequently, the comparison area used was the Merseyside Police Department Area (PDA) minus the action areas and suitable buffer zones.

To assess the suitability of the comparison area selected, we examined changes across the action and comparison areas for the historic period before implementation began. A related samples *t*-test confirmed that the ratio of the concentration of burglaries in the two areas (action/comparison area) was stable over the two years (1998–1999 and 1999–2000) considered ($t(25) = 0.47$, $P = 0.64$, two-tailed), thereby demonstrating that they followed the same trend before the alley-gated scheme was implemented and, consequently, validating the approach.

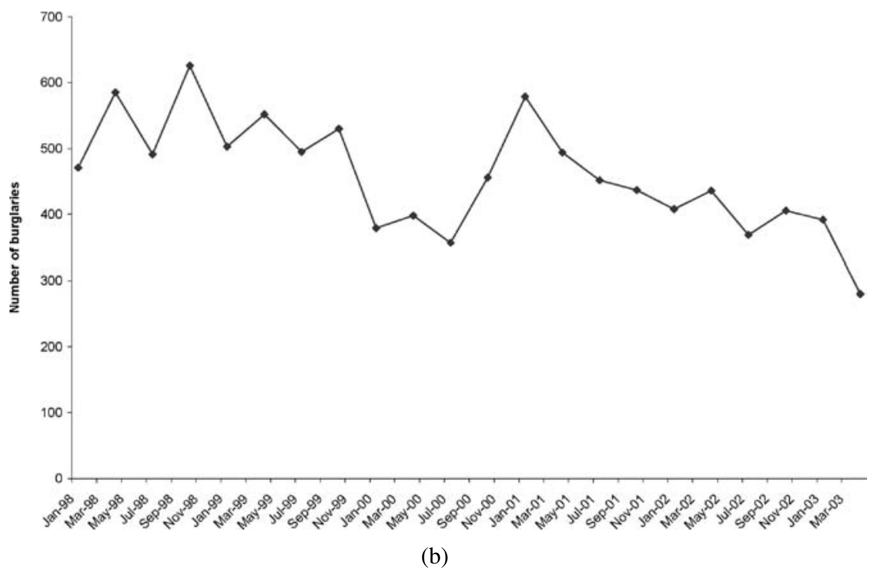
Using the available recorded crime data it was possible to compute burglary ratios. These are simply the count of crime in the action area divided by the count of crime in the comparison area. This ratio is tracked for the entire period of the evaluation and covers all the alley-gating scheme areas at all times (independently of whether they have received treatment or not). The advantage of using this metric is that any trends identified are net of those apparent in the wider PDA, and thus specific to the action area. It is apparent from the results, shown as Figure 2a, that for the period prior to intensive implementation (the historic period), the burglary ratio was relatively stable. Following this period there was a steady decline in the burglary ratio. Figures 2b and c shows the raw data used to calculate the burglary ratios. It is evident from these three figures that there was a noticeable decline in burglary in the alley-gated areas, whereas burglary in the comparison area remained reasonably stable over time, showing that there was in fact an absolute decline in burglary in the alley-gated areas, as well as a relative one.

To examine the effectiveness of the scheme it is necessary to compare the burglary ratios in the action areas before and after implementation. One way of doing this is to compute burglary ratios for each block of houses protected by gates both before and after the inception of the scheme, and compare the average ratios. This approach is fairly straightforward where a scheme begins on a specific date and all measures are implemented within a short time scale. However, where a rolling implementation schedule is adopted over a period of years, as was the case here, it is more complicated. This is because the before and after periods vary across schemes, meaning that the use of a single cut-off date would be misleading. For instance, if a universal start date of January 1, 2000 were used for all schemes, but many did not start until January 1, 2001 (as was true here), for the latter schemes part of the 'after' period would, in reality, effectively be a 'before' period as no gates would have been implemented. Thus, for the period January 2000–January 2001 we would not anticipate any evidence of reduction that was attributable to these schemes, even though this period was included in the evaluation 'after' period.

One solution to this problem has been suggested elsewhere (Johnson et al. 2001; Bowers et al. 2003). Briefly put, this involves producing standardised *opportunity dependent* counts, derived by simply dividing the counts of burglary observed before and after the *actual* implementation of the interventions by the amount of time that elapsed during these periods. Thus, if 10 burglaries occurred before and after implementation, and data were available for a period of three years before and one year



(a)



(b)

Figure 2. (a) Burglary ratios for the historic and implementation periods (trend lines shown as dotted lines). (b) Burglary trend in the alley-gated areas. (c) Burglary trends in the comparison area.

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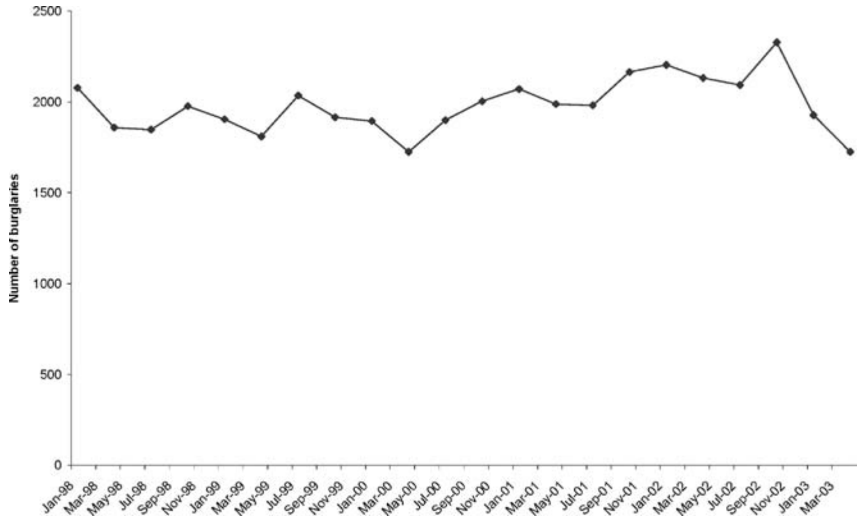

(c)

Figure 2. (Continued).

after, the annual opportunity counts would be 3.33 and 10, before and after implementation, respectively. In this way, the counts of burglary for the before and after periods are not contaminated but, as a result of standardising them by the amount of time elapsed, are comparable.

To do this, for each gated block of houses we computed how many burglaries occurred before and after installation of the gates and, how many months elapsed in these two time periods. To translate these figures into *opportunity dependent burglary ratios*, we also computed how many burglaries occurred within the comparison area for the same time periods for each scheme individually, and divided counts in the action area by those in the comparison area. Hence the time period used for the comparison varied according to the particular scheme being considered.

Examination of the timing and intensity of implementation revealed that many of the gates had only been installed for a short amount of time, meaning that their effects would not have been realised during the evaluation period. For this reason, separate analyses were conducted for all alley-gated areas, those for which the gates had been installed for at least 6 months, and those for which the gates had been installed for 12 months or more. The results, presented in Table I, demonstrate that for all analyses there was a reduction in the opportunity dependent ratio, and indicated that, relative to the comparison area, the risk within the action areas had fallen by up to 37%. To test the statistical significance of the change in the opportunity dependent rates it was necessary to use a non-parametric wilcoxon rank sum test, as the distribution of the before and after opportunity rates did

TABLE I

Opportunity dependent ratio before and after implementation.

	Opportunity dependent ratio			
	Before	After	Change (%)	z-value
All areas (3174 gates)	164.00	109.43	33.27	−6.70*
6 months (2943 gates)	159.82	110.69	30.74	−6.43*
12 months (1363 gates)	142.52	89.71	37.05	−5.66*

Note: The ratios have been multiplied by a factor of 1,000,000 to make them easier to interpret.
$^{*}P < 0.001$.

not conform to a normal distribution. The z-scores for each analysis, also shown in Table I, confirm that the changes were statistically significant.

Crime ratios can also be used to translate the change observed in the burglary rate into a further measure of scheme outcome – an estimation of the number of burglaries prevented by the scheme. The rationale underlying this procedure is that in the absence of the scheme, the burglary rate in the scheme area would have followed a similar trend to that in the comparison area. Thus, multiplying the number of burglaries observed in the action area prior to implementation by the ratio of change in the comparison area should provide a reasonable estimate of the number of burglaries expected in the action area. The number of burglaries prevented is simply the number of burglaries expected minus the number observed (for a full discussion, see Johnson et al. 2004). This process was conducted for each of the alley-gated areas separately, using the relevant start date. Note that here also the before and after periods for the comparison area varied according to the start dates of the particular schemes.

The results of the analysis, presented in Table II, suggest that up until the end of the evaluation period, across all the schemes included in the analysis, a total of 875 incidents of burglary were prevented. Having estimated the outcome of the scheme in this way, it is also possible to conduct a rudimentary cost benefit analysis (CBA), by comparing the cost of the scheme with the benefits realised. The challenges here are 1) deriving an estimate of the financial value to society of the crimes prevented; and, 2) estimating the cost of the scheme. Fortunately, it is possible to address both issues. In relation to the former, Brand and Price (2000) have estimated that the average cost to society of one burglary, including processing through the criminal justice system and the value of goods stolen, is £2,300. Apropos the latter, using detailed information on the monetary costs of the scheme, the cost of the installation of one gate, including the resident consultation process, manufacture and installation, was estimated at £659. The results

TABLE II

Analysis of the number of burglaries prevented.

	Burglaries prevented	Number of gates	Cost of gates[a]	Values of burglaries saved	Cost benefit ratio (CBR)
All areas	875	3174	£2,094,302	£2,013,967	0.96
6 months	852	2943	£1,939,437	£1,960,252	1.01
12 months	727	1363	£898,217	£1,671,041	1.86

[a]This was the number of gates multiplied by the cost of installation of a gate (£659).

of the cost benefit analyses are also shown in Table II. The final column of Table II is the cost benefit ratio (CBR) for each sub-group of gates. This is simply the financial benefit of the scheme divided by the cost of implementation. CBRs are readily interpretable. For instance, a CBR of 1 indicates that for every dollar spent a return of one dollar was realised, meaning that the scheme broke even. A value greater than one indicates that the scheme was beneficial, a value of less than one that it was not. It is evident that for all areas considered together, the scheme just failed to break even, whereas for the analyses that consider only those areas where the gates had been installed for 12 months or more, the scheme was considerably cost beneficial realising a return of £1.86 for every £1 spent.

The Relationship Between Intensity of Implementation and Burglary Reduction

The results presented above suggest that the scheme had a substantial impact on burglary. However, the scheme was not implemented in a policy vacuum. For instance, across Merseyside County a variety of other crime reduction schemes and police department initiatives were operational during the period of implementation. Thus, it is possible that the changes evident could have been attributable to other crime prevention activity. Perhaps the most scientific way to examine this issue is to conduct a variation of dose-response analysis. Used prevalently in medical research, the rationale for such an analysis is that if a particular treatment (or intervention) is effective, then (up to a certain threshold) increasing doses of it should produce increasing responses (for a further discussion, see Ekblom and Pease 1995). This type of analysis is particularly suited to situations where a variety of interventions, complementary or competing, are implemented over similar time periods as it allows the relative contributions of each to be isolated.

One challenge in this type of analysis is the identification of an appropriate metric to operationalize the measure of dosage, or implementation intensity. For instance, where a series of different types of interventions are used, such as alley-gates and an offender based scheme, what would be the appropriate measure of intensity? In such cases, the quarterly costs of the scheme, or a standardised measure of output may be appropriate (e.g. see Bowers et al. 2004), but such measures are often difficult to construct. Fortunately, in the current research no such problem exists as only one type of intervention was evaluated. Here, three possible measures of intensity were available, these being the number of gates installed, the number of blocks of houses gated or the actual number of houses protected.

Thus, to conduct the analysis, we measured the association between the cumulative number of gates installed (or blocks or houses protected) in each quarter, the intensity of the scheme, and the simple burglary ratio for the alley-gated areas as a whole (the dependent variable). Figure 2a shows this relationship visually for the cumulative number of gates installed. A cumulative measure of intensity was adopted here as the impact of each gate was assumed to be fairly long-term (i.e. years rather than months) meaning the effects of the gates would be additive and not transitory. The results of the regression analyses (shown in Table III) confirmed that there were strong significant negative relationships between all three measures of intensity and the crime ratio, and demonstrated that the intensity variables explained 48–55% of the variance in the dependent variable. The table also shows that of the three measures, the strongest relationship was between reductions in the burglary ratio and the number of blocks protected.

Separate analysis conducted using an alternative measure of intensity, the number of gates fitted (or blocks or houses protected) per quarter, produced a similar pattern of results. In this case the amount of variance explained was between 29–37%, somewhat less than that explained by the cumulative measure (see Table III). This provides some evidence that the gates do in fact have longer-term effects and that it is useful to judge their effectiveness over longer periods of time. Once again, the strongest relationship was between the burglary ratio and the number of blocks protected. In general, therefore, the results of these dose-response analyses indicate that whilst

TABLE III

Regressions between measures of intensity and burglary reduction.[a]

Intensity measure	Cumulative measure	Quarterly measure
No. of gates fitted	$R = 0.69, F = 18.37, P < 0.0001$	$R = 0.54, F = 8.33, P < 0.01$
No. of blocks protected	$R = 0.74, F = 24.88, P < 0.0001$	$R = 0.61, F = 11.92, P < 0.005$
No. of houses protected	$R = 0.71, F = 20.16, P < 0.0002$	$R = 0.54, F = 8.21, P < 0.01$

[a]Autoregressive terms were initially included in each equation but proved to be non-significant and hence were excluded from the final analysis.

the pattern of burglary reduction apparent may have been influenced by factors other than the scheme, there is persuasive evidence that it was, at least partly, attributable to the intervention.

Geographical Displacement/Diffusion of Benefits

As noted in the introduction, rarely do crime reduction schemes impact upon recipients alone. Recent research suggests that where schemes are successful at reducing crime, a diffusion of benefit, rather than geographical displacement is the more likely outcome, although, these two phenomena may co-occur. To investigate this issue, we adopted a procedure discussed elsewhere (Bowers and Johnson 2003). Using this technique, changes in both the action area and a suitable buffer zone that surrounds it are compared with those within a comparison area, and then related to each other, using the following formula:

$$\mathrm{WDQ} = \frac{B_{t1}/C_{t1} - B_{t0}/C_{t0}}{A_{t1}/C_{t1} - A_{t0}/C_{t0}}$$

where, A: action area, B: buffer zone, C: comparison area, t_1: after implementation, t_0: before implementation.

The resulting descriptive statistic, the weighted displacement quotient (WDQ), gives an indication of whether there was evidence of displacement (as indicated by a negative value) or diffusion of benefit (as indicated by a positive value). Increasing values reflect the degree, relative to the change in the action area, to which either were apparent. Thus, a value of $+1$ (-1) would indicate a situation where there was a reduction (increase) in the buffer zone of equal magnitude to the change observed in the action area.

A series of (seven) 200 m concentric buffer zones,[4] shown in Figure 3, were generated using the 'Buffer' command in a GIS. Using the 'spatial join' command in the GIS it was possible to count how many burglaries were recorded in each buffer zone for each quarter of the evaluation period. Next, WDQs were computed for the entire buffer zone, and for each of the seven rings separately. The results indicated that, in line with the research literature, overall there was evidence of a diffusion of benefit. The WDQ value for the entire buffer zone was $+0.13$ indicating that the reduction apparent within the buffer zone was much smaller than that within the alley-gated areas. Considering the individual buffer zones, there was considerable evidence of a diffusion of benefit within the first buffer zone (WDQ = $+0.40$). Across the next three buffer zones there was less evidence of this phenomenon (WDQs ranged from 0.22 to 0.08). In the fifth and sixth buffer

[4] This size of buffer zone (1.4 km) was chosen due to recommendations arising from our earlier work.

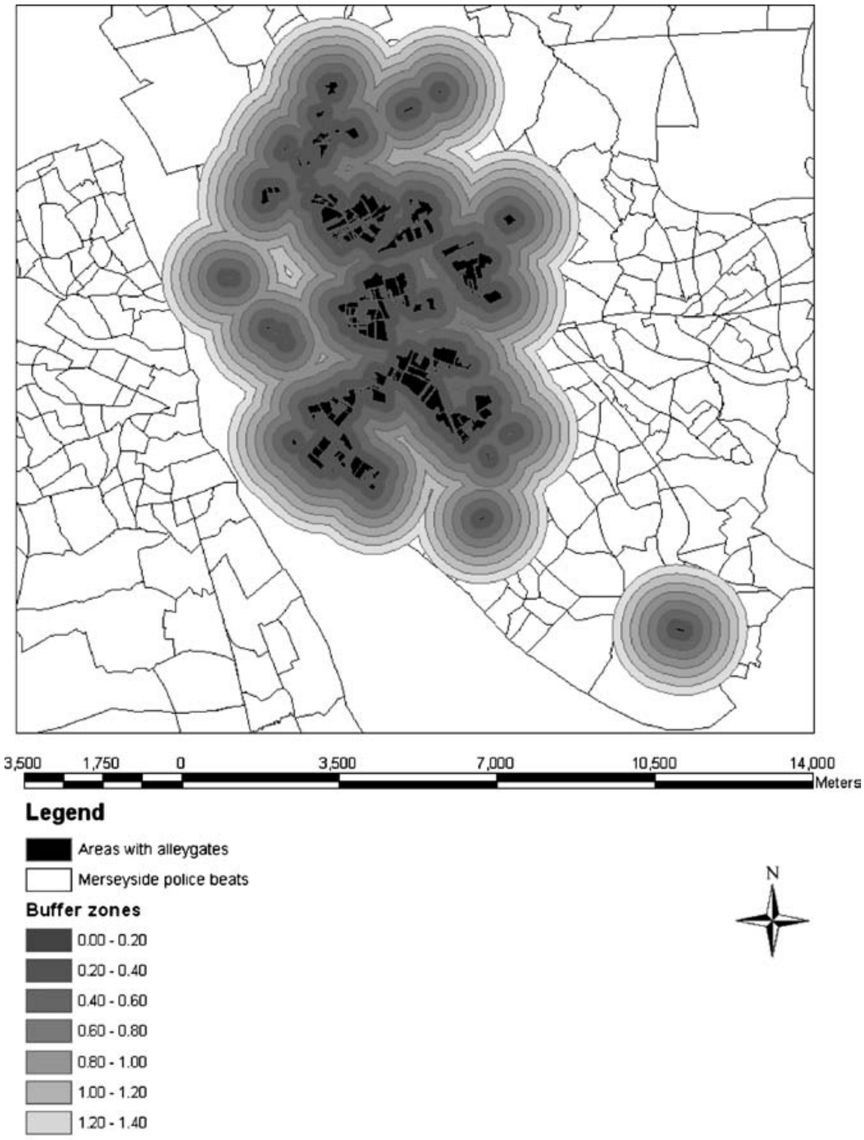

Figure 3. Map of the alley-gated areas and buffer zones.

zones there was some evidence of displacement (WDQs $= -0.03$ and -0.27, respectively), and in the final buffer zone, there was little change (WDQ $= 0.04$).

Further analyses were conducted to examine the timing of these patterns in relation to implementation intensity. To do this, burglary ratios were computed for each of the seven buffer zones (buffer zone/remainder of the PDA) for every quarter, and the results correlated with the measure of

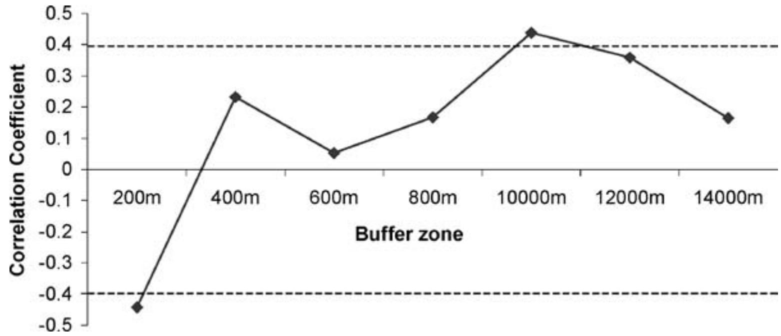

Figure 4. Relationship between changes in the buffer zone and implementation intensity (dotted line indicates a significant correlation coefficient, two-tailed).

intensity, the number of areas gated.[5] The results, summarised in Figure 4, demonstrate that the changes within the first and fifth buffer rings, for which there was evidence of a diffusion of benefit and geographical displacement respectively, were coincident with the intensity of implementation. In both cases the intensity variable explained just over 16% of the variance in the burglary ratio. This suggests that the changes in these areas were at least partly attributable to scheme activity. Across the other buffer zones, there was no relationship between implementation intensity and the changes apparent.

Tactical (Modus Operandi) Switch Displacement

By restricting access to residents' alleyways, opportunities for offending should be reduced in a very specific way. That is, as a result of alley-gating, offenders should be substantially deterred, or prevented, from breaking and entering from the rear of properties. Thus, if the changes apparent in the alley-gated areas *are* attributable to the intervention, in addition to observing a reduction in burglary in these areas, we would also anticipate to see a particular reduction in the proportion of crimes committed via the rear of properties.

To examine this issue, using modus operandi (MO) data, we computed for how many burglaries committed within the action and comparison areas the point of entry used was the front and back (or other entry point) of the house. Of the 71,151 burglaries recorded over the evaluation period, the relevant MO data were available for 82% of incidents.

During the historic period, for 65% of the burglaries committed within the alley-gated areas, access was gained via the rear of the property (32% from the front of the house and 3% from other entry points). For Merseyside

[5] The same pattern of results was obtained using the alternative measures of intensity.

as a whole, the figures were 59, 34 and 7%, respectively. This demonstrates that for the majority of burglaries, access was gained via the rear of properties. This type of access is particularly common in action areas, and therefore validates the rationale for the intervention.

Following the inception of the scheme, these proportions changed slightly across Merseyside as a whole, the respective figures being 54, 39 and 7%. In the alley-gated areas, the figures changed somewhat more, access being gained via the rear of the property for 55% of incidents, via the front for 41% and elsewhere for 4%. However, this type of analysis fails to reflect that there was also a significant reduction in burglary within the alley-gated areas. For this reason, MO burglary ratios were constructed for each point of entry (back, front and rear) for the periods before and after implementation. For example, the burglary MO ratio for entry gained via the rear of the property was the number of burglaries of this nature in the alley-gated areas divided by those of this nature in the comparison area. The results, shown as Table IV, confirm that relative to the remainder of the PDA (PDA-action area-buffer zone), there was a reduction in the proportion of burglaries for which entry was gained via the rear of the property in the alley-gated areas. In contrast, there were increases for entry via the front of the property and elsewhere. However, the final row of Table IV indicates that the actual decrease in the burglary MO ratio was greater for the back of the property than the increases in the other MO ratios. Moreover, the finding that there was a considerable reduction in burglary across the alley-gated areas (presented earlier), demonstrates that any displacement of this kind was not absolute.

To examine the changes in more detail, MO burglary ratios were computed for each quarter and are shown as Figure 5. For entry via the rear of properties, the pattern of results is commensurate with the simple analysis of the before and after figures, demonstrating that before the start of the scheme, the burglary MO ratios were relatively stable, and changed in line with expectations after implementation began, showing a steady decline over time. However, for entry via the front and elsewhere, although there

TABLE IV

Burglary MO ratios in the alley-gated areas before and after the inception of the scheme.

	Point of entry		
	Back	Front	Other
Before	0.358	0.182	0.071
After	0.226	0.244	0.106
Difference	−0.132	+0.062	+0.035

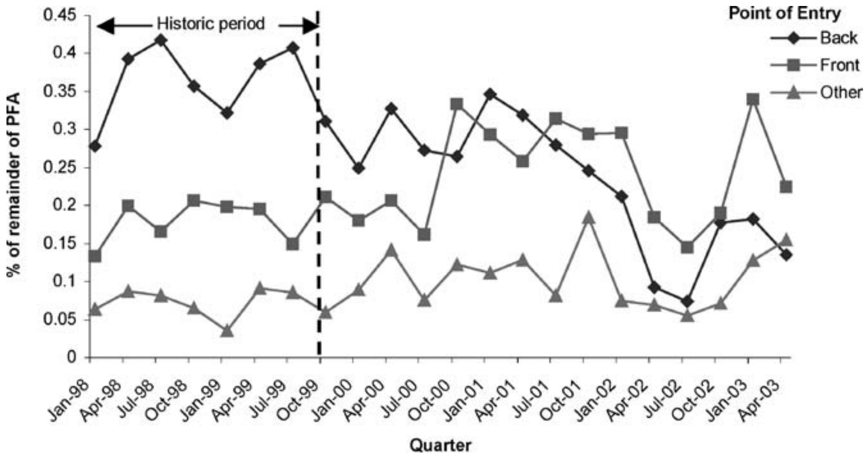

Figure 5. Analysis of the point of entry of burglaries committed within the alley-gated areas.

were increases following the start of the scheme, these appear to be more random.

A dose-response analysis was conducted to explore the changes more systematically. This revealed that for entry via the rear of properties, the measure of intensity, the cumulative number of areas gated,[6] explained 62% of the variance in the burglary MO ratio variable ($R = 0.80$, $F = 34.65$, $P < 0.001$). For entry via the front of the property ($R = 0.35$, $F = 2.73$, $P = 0.114$, $R^2 = 0.08$) and elsewhere ($R = 0.33$, $F = 2.48$, $P = 0.13$, $R^2 = 0.07$), the amount of variance explained was uniformly low and non-significant. Thus, it would appear that whilst the reduction in the proportion of burglaries committed via the rear of properties was coincident with the timing and intensity of implementation, this was not the case for the increase in burglaries where access was gained elsewhere. This is not to say, however, that any tactical switch observed was not attributable to the scheme, but that it may have been influenced by other factors as well.

CONCLUSIONS

This paper has demonstrated that the implementation of alley-gating is associated with burglary reduction. Using a technique that controls for differences in the timing of implementation (which is vital for accurate interpretation of the effect of crime prevention measures on the burglary rate), we have demonstrated that within the alley-gated areas levels of burglary were reduced by up to 37%, and that the changes were statistically

[6] The same pattern of results was obtained using the alternative measures of intensity.

significant. Importantly, this reduction was net of more general changes in the surrounding area. Evaluations that do not account for such general changes tend to overestimate (or underestimate) the effects of the measures implemented; this is particularly true with property crime, which has been decreasing in England and Wales for some time (e.g. Budd 1999).

Using methods explained in detail elsewhere (Johnson et al. 2004), we also estimated the number of burglaries prevented by the scheme. This allowed us to conduct a rudimentary cost benefit analysis. The results of this showed that in the areas where the gates had been installed for some time (12 months or more) the cost benefit ratio was around 1.86, indicating that there was a substantial financial saving. The inclusion, in this analysis, of areas in which the gates had been installed for shorter periods of time reduced the cost benefit ratio substantially. This suggests that the intervention should not be seen as a 'rough and ready' measure to reduce crime in the short term, but more as a long-term solution in areas that have been afflicted with a high burglary rate for some time.

Importantly, the finding that implementation intensity was highly associated with reductions in burglary increases confidence in the conclusion that the reduction was attributable to the intervention. Moreover, the finding that the cumulative measure of intensity was most strongly associated with changes in the burglary rate, provides support for the idea that the effects of the gates endure for some time, rather than decaying shortly after installation. This is important for two reasons. First, it suggests that the crime reductive effects of the gates are sustainable. And, second, as many of the gates were installed in the final 12 months of the evaluation period, the impact of these gates will only have been marginally realised, and hence the analyses presented here will have considerably underestimated the benefits of the scheme in crime reductive and financial terms. Consider also that if the positive effects of the scheme are sustained for a number of years, then, at no additional implementation cost, additional crime reductive effects will be realised, increasing the cost benefit ratio of the scheme still further.

It should be pointed out here that although this method resembles the type of dose-response analysis typically used in medical trials, there are some significant differences. In particular, in medical trials different doses of treatment are randomly assigned to participants at different times, and the effects of one dose of a drug (e.g. aspirin) are anticipated to be short-lived. In contrast, the alley-gated areas were not randomly selected and the impacts of the intervention were designed to be cumulative and to endure over time. As a result of this departure from the standard dose-response design, there exists the possibility that other changes in the characteristics of the area in which the gates were installed could potentially have caused the changes in the burglary rate.

However, perhaps the most convincing evidence that the reductions realised were attributable to the intervention comes from the analyses of the point of entry used in the burglaries committed in the alley-gated areas. The results indicated that following the implementation of the scheme, relative to the comparison area, there was a reduction in the number of burglaries for which access was gained via the rear of the property. Moreover, there was a strong inverse relationship between the intensity of implementation and the frequency with which this MO was used. Whilst other interventions may have been operational within the action area during this period, and other characteristics of the area may have changed, it is unlikely that these would have had the same selective impact upon burglary in the area. The rationale of alley-gating is, after all, to reduce burglary specifically by closing off opportunities to offend via the rear of properties. Nor is it likely that the implementation of other schemes or changes in area characteristics would have been entirely coincident with that of the alley-gates. Thus, taken together the results strongly suggest that the alley-gates had a crime reductive effect within the action areas.

The latter finding raised the possibility that there may have been an increase in the number of burglaries for which access was gained via entry points at locations other than the rear of the property (tactical displacement). In line with this, analyses revealed that there was an increase in the proportion of burglaries for which access was gained at the front of the houses, or elsewhere. However, this type of displacement was far from absolute as, despite this effect, a significant reduction in burglary was still realised within the action area. Moreover, the fact that the dose-response analysis demonstrated that the (temporal pattern of) increase in the popularity of the use these points of entry was not coincident with the timing and intensity of implementation may suggest that the apparent change was not solely attributable to the intervention.

In relation to geographical displacement, in line with the findings of other research (e.g. see Bowers and Johnson 2003), overall there was evidence of a diffusion of benefit. More detailed analyses revealed that the most dramatic reduction was within 200 m of the scheme boundary. This pattern is also in line with previous research on geographical displacement (e.g. see Johnson et al. 2001), and may reflect the fact that offenders were not aware of the precise geographical boundary of the scheme, or that they believed that the initiative was not limited to (highly visible) alley-gates within and nearby the alley-gated areas. Importantly, the finding that the changes apparent within this area were coincident with the timing and intensity of implementation, confirms that at least some of this reduction was attributable to the scheme.

There was also limited evidence of displacement within buffer zones slightly further away from the operational boundary of the scheme, those at a distance of 800 m–1.2 km away. A dose-response analysis revealed that the changes within the 800 m–1 km ring, but not those further away, were significantly associated with the timing of implementation.

Relevant here is Eck's (1993) concept of familiarity decay. According to this theory, when deterred from offending at one location, offenders will choose the next most similar target. Consequently, if displacement occurs there is likely to be a 'displacement gradient', with offenders committing offences at nearby (similar) locations. This appears to chime partially with the findings presented, which suggest that immediately adjacent to the scheme there was evidence of a diffusion of benefit (a 'no-go' zone), whereas, slightly further away there was some evidence of displacement, the effect of which appeared to dissipate over greater distances.

At this point, however, it is important to reiterate that there was more evidence of a diffusion of benefit than displacement, suggesting that, not only was the displacement observed not absolute, but also, that, overall, the scheme had a positive effect on properties in the surrounding, as well as within scheme area. Equally important to note is that the identification of the distance over which the scheme had positive and negative impacts has important implications for the planning and implementation of future initiatives of a similar nature. For instance, in the implementation of such schemes, it may be wise to implement complementary crime reduction measures within buffer zones around 800–1200 m away from the operational boundary of schemes. Such a strategy may eliminate the relatively minor evidence of geographical displacement found here and extend the effects of the schemes still further.

There has been much debate about the relative merit of situational crime prevention measures such as alley-gating in reducing crime, in comparison to using other methods such as educational programmes or criminal justice interventions (e.g. Goldblatt and Lewis 1998). Situational measures are often criticised for ignoring the underlying causes of crime and for concentrating only upon reducing criminal opportunities. Often such measures are also accused (without scientific foundation) of causing crime displacement (Barr and Pease 1990) or of being territorially unjust because they "exclude" certain communities or people (e.g. White and Sutton 1995). Contrary to this, this paper has demonstrated that alley-gating is a particularly effective way of closing off opportunities for crime and represents a robust burglary reduction measure. Furthermore, the evidence indicates that a diffusion of benefit is a more likely outcome than a displacement of crime to other areas.

We have also demonstrated the practical application of new techniques for measuring the impact of crime prevention schemes on the temporal and spatial distribution of crime. Here, we have used the methods on a particular type of intervention (alley-gating), in a particular geographical location (Merseyside, UK). However, the beauty of the types of method that have been discussed here is that they are flexible, and can be used in many localities where crime prevention activity is present. Whilst the authors acknowledge that there may not be specific interventions elsewhere which operate in a similar way to alley-gating, there will be many situations where these techniques could prove useful in policy evaluation. The adoption and further development of quantitative methods for isolating the impact of policy is essential if we are ever to distinguish effective from ineffective crime prevention measures and to add to the evidence based on what works, for whom, where, when and at what cost.

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