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Gating Alleys to Reduce Crime: A Meta-Analysis and Realist Synthesis

*Aiden Sidebottom**, Lisa Tompson,
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Kate Bowers and Shane D. Johnson

Alley gates are designed to limit access to alleys and the crime opportunities they afford. Informed by the acronym EMMIE we sought to: (1) systematically review the evidence on whether alley gates are Effective at reducing crime, (2) identify the causal Mechanisms through which alley gates are expected to work and the conditions that Moderate effectiveness, and (3) collate information on the Implementation and Economic costs of alley gating. The results of our meta-analysis suggest that alley gating is associated with modest but significant reductions in burglary, with little evidence of spatial displacement. We also identified six mechanisms through which alley gates might plausibly reduce crime, and the conditions in which such mechanisms are most likely to be activated.

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Background

Alleys are a familiar feature of urban environments. They have long been associated with crime, incivilities and undesirable behaviors. Seymour, Wolch, Reynolds, and Bradbury (2010, p. 380) put alleys in the same category as “free-way shoulders, train yards, abandoned waterfronts, and parking lots” which tend to be “underutilized, underdeveloped, and often deteriorating spaces”. The negative perceptions associated with alleyways even affect property prices. Guttery (2002) shows that properties in Dallas (USA) that are located on alleyways sell for approximately 5% less than otherwise identical homes, which he attributes, in part, to greater levels of crime and disorder associated with alleys.

Alleys may influence crime in several ways. They can *attract* offenders because of the perceived high likelihood of available crime opportunities (e.g. the presence of drug markets and prostitutes). They can *generate* crimes such as robbery and assaults by providing a convergence setting for motivated offenders and potential targets in the absence of capable guardians (Cohen & Felson, 1979). They may *facilitate* crime through providing inconspicuous access to alley-adjacent properties and a means by which to escape and evade detection. And, where uncertainty exists over their management and ownership, alleys can provide tempting and convenient sites to illegally discard waste, conduct arson or paint graffiti.

For these reasons, there have been diverse attempts to reduce the undesirable behaviors associated with urban alleys. Drawing on evidence from seven US cities, Newell et al. (2013) review several “alley greening” schemes whereby unkempt and rarely used alleyways were redesigned in an effort to promote human activity and provide a welcoming environment to facilitate neighborly interactions. Focusing specifically on crime prevention, Kaplan, Palkovitz, and Pesce (1978) describe dedicated police patrols of problem alleyways. Similarly Clarke (2004) discusses examples of high crime streets and alleys being temporarily or permanently closed in a bid to prevent crime and disorder.

The focus of this review is on a situational crime prevention (SCP) technique known as alley gating. Alley gating refers to the installation of lockable gates, usually made of steel or iron, which restrict entry to an alley or network of alleys. Alley gates can take several forms reflecting the different uses and users of urban alleyways: opaque or transparent, self-closing or manually operated, single-leaf or double-leaf, plain or ornate and so on. Despite the diversity in alley gate design, the shared intention is to restrict alley access to legitimate users in possession of a key or passcode and to control access to potential crime targets.

Alley gates are generally conceived as burglary reduction measures that restrict access to the alleys behind rows of homes. However, by limiting access

alley gates may also reduce other crime opportunities, such as prostitution, dog fouling and drug dealing. Additional objectives include a reduction in feelings of insecurity and improvements in community cohesion. As Rogers (2013, p. 106) writes,

Alley-gates have been packaged as a panacea for the evils of domestic as well as non-domestic burglaries, a raft of anti-social behavioral issues and an important device in regaining control of rarely used spaces along with the regeneration of urban communities.

Objectives of the Review

Studies are available on the challenges associated with implementing alley gates (Armitage, 2006; Johnson & Loxley, 2001), their impact on crime (e.g. see Bowers, Johnson, & Hirschfield, 2004; Haywood, Kautt, & Whitaker, 2009) and on community perceptions (Armitage & Smithson, 2007; Rogers, 2013; Staunton, 2006). To date, however, there has been no attempt to systematically review the evidence on whether alley gates effectively reduce crime. This is the first objective of this review. Our second objective, informed by EMMIE (an acronym denoting Effect, Mechanism, Moderators, Implementation, and Economics, see Johnson, Tilley, & Bowers, 2015), is to identify the causal mechanisms through which alley gates are expected to reduce crime and the conditions under which they have been found to be effective, ineffective and/or to produce unintended negative effects. To this aim we undertake a largely qualitative review drawing on the principles of realist evaluation and considering a wider range of alley gating studies. Our third objective is to review information on the implementation and costs of alley gates.

The paper is organized as follows. First we provide a brief summary of EMMIE and describe how it informed our review. Next we report our methods and search strategy. The results then follow, organized according to EMMIE. We finish by discussing the main findings of our review and reflect on our attempt to integrate meta-analysis and realist synthesis methods.

EMMIE and its Relevance to the Current Review

This review was conducted in support of the *What Works Center for Crime Reduction*, hosted by the UK College of Policing (CoP). One aim of the CoP is to promote and facilitate evidence-based policing, defined as “a method of making decisions about ‘what works’ in policing: which practices and strategies accomplish police missions most cost-effectively” (Sherman, 2013, p. 377). The past decade has witnessed a noticeable turn towards developing evidence-based approaches to policing. This can be seen in several recent

developments. In the US, the National Institute of Justice has launched *CrimeSolutions.gov*, an online resource that distils and grades research evidence on the effectiveness of various criminal justice programs. Lum, Koper, and Telep (2011) have likewise generated the *Evidence-Based Policing Matrix*, an interactive evidence translation tool which usefully summarizes a large number of crime prevention evaluation studies. In the UK, the CoP has developed the *Crime Reduction Toolkit*, which rates and summarizes evidence reported in systematic reviews of crime reduction interventions. These three initiatives share a common purpose: that of assembling and presenting research evidence in a manner that is relevant and accessible to crime prevention practitioners and policymakers.

As with other areas of evidence-based policy (most notably medicine), systematic reviews lie at the heart of the evidence-based policing movement. This is expected: systematic reviews are generally considered to be one of the more trustworthy sources of evidence and occupy the top position of many “hierarchies of evidence” (see Elamin & Montori, 2012). This is usually in the form of systematic reviews with meta-analysis whereby evidence from eligible studies is synthesized to compute an overall effect size and determine whether the “what” was reliably found to “work”.

Part of the motivation for proposing EMMIE was to highlight that knowledge of “what works” (or has been found to work) is likely to be insufficient to achieve the sort of evidence-informed decision-making envisaged by proponents of evidence-based policing. EMMIE was devised as an acronym to encapsulate the types of evidence that studies might provide to inform decision-making concerning the funding and/or implementation of crime prevention schemes, in addition to and including information on intervention effectiveness (Johnson et al., 2015).

The initial E of EMMIE refers to the “effect” size of a policy, program, practice or intervention. The first M refers to the “mechanism” through which a policy, program, practice or intervention brings about its effect. This is important in determining whether what has been done needs to happen if a given outcome is to be reproduced (or to be avoided). The second M refers to “moderators” (or “contexts”)—the conditions that need to be in place for a policy, program, practice or intervention to activate the mechanisms necessary to produce intended effects. The I refers to issues of “implementation”. Decision-makers need to know whether and how a policy, practice, program or intervention can be put in place and what may facilitate or impede this (Laycock & Tilley, 1995). Finally, the second E refers to “economics”—what the intervention will cost in relation to outputs, outcomes or benefits (see Manning, Johnson, Tilley, Wong, & Vorsina, 2016). There are always limited resources that can be put to alternative uses and decision makers need to determine how best to disburse those available to them.

In this review we use EMMIE as a guiding framework for the types of evidence we might usefully collect (or acknowledge the absence of). To achieve this aim, we combine two distinct approaches to evidence synthesis.

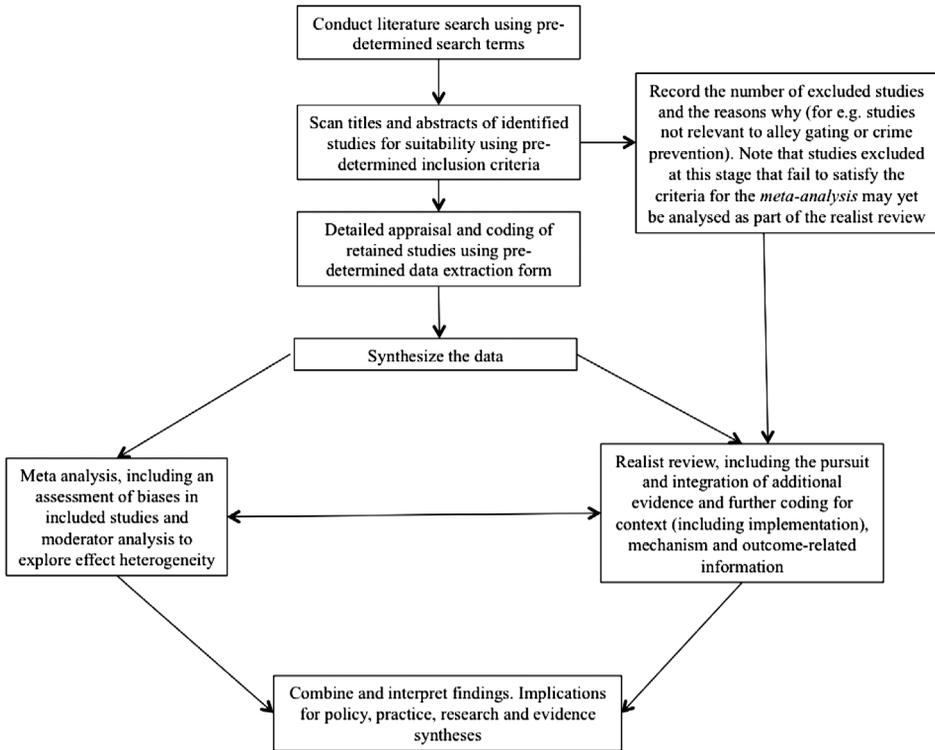


Figure 1 Flowchart of the review process.

For information concerning the effectiveness and cost effectiveness of alley gates we use conventional meta-analytic methods. For information concerning the mechanisms, moderators and implementation of alley gates, we turn to realist synthesis (Pawson, 2006), an alternative approach to evidence synthesis derived from the principles and methods of realist evaluation (Pawson & Tilley, 1997). Realist evaluation is chiefly concerned with determining the causal mechanisms responsible for outcome patterns and the contextual conditions under which those mechanisms operate (or do not operate). In the context of realist reviews, the aim is therefore to assess what the available evidence says about how and under what conditions the intervention of interest (here alley gating) has been found to be effective, ineffective and to produce unintended negative effects. Increasingly advocated and used in improving health-related evaluation and reviews (see, for example, Berwick, 2008; Best et al., 2012; Davidoff, 2009; Kastner, Perrier, Hamid, et al., 2015; Wong, Pawson, & Owen, 2011), this is relatively new territory in criminology (an exception is van der Knaap, Leeuw, Bogaerts, & Nijssen, 2008). A novel feature of this review is our proposed method for integrating meta-analysis and realist synthesis (Figure 1), which we will elaborate on in the sections that follow.

Methods

Criteria for Considering Studies for this Review

We used the following criteria in selecting studies for this review:

- (a) *The study must have reported an explicit goal of reducing crime through the use of alley gates.* Studies implemented by any stakeholder (law enforcement, government agencies, private entities, citizens, etc.) that reported the effects of alley gates implemented in isolation or as part of a wider package of interventions were included.

To be included in our meta-analysis, a study had to satisfy point (a) above and:

- (b) *report at least one quantitative crime outcome measure.* These could comprise official measures (police recorded crime data, calls for service) or unofficial measures (self-reported levels of offending and/or victimization). Studies that only reported non-crime-related outcome measures (e.g. changes in pedestrian flow) were excluded.
- (c) *report original research findings.* Systematic reviews were not included. Where the same findings were reported in multiple publications, the study reporting the most detailed information was included. Where necessary, any dependency in the data was dealt with appropriately.
- (d) *employ a research design that enabled the computation of a reliable effect size* (for example, a (quasi) experimental study with control group or a suitable interrupted time series design).

The shortage of experimental and quasi-experimental studies in criminology is well recognized, particularly for evaluations of situational interventions (Eck, 2006; Guerette, 2009). This can limit the number of studies eligible for meta-analysis. Consequently, while following the above criteria we also considered studies that measured the impact of alley gating using simple before and after designs and no control area. Where such studies are used in the analysis that follows, this is clearly indicated, along with the familiar concerns regarding the internal validity of such study designs.

Items (b), (c) and (d) did not constitute the inclusion criteria for the realist branch of our review which, as mentioned previously, draws on a wider range of studies in pursuit of relevant information concerning Mechanisms, Moderators and Implementation. To be included in our realist synthesis, studies had to satisfy point (a) above and report substantive information on *at least one* of the items below:

- (e) Crime-related causal mechanisms activated by alley gates.
- (f) The conditions needed for alley gates to activate crime-related causal mechanisms.
- (g) Theoretical content concerning alley gates and crime-related outcomes.
- (h) The implementation of alley gates.

It should be noted that for the realist branch of our review, studies were included if they *reported* substantive information; we did not insist on studies reporting *empirical evidence* on Mechanisms, Moderators and Implementation. This decision was based on prior research which found that information concerning these factors are seldom reported in the crime prevention literature, let alone empirically examined (van der Knaap et al., 2008).

Search Strategy for Identifying Studies and Methodological Approaches

This paper draws on studies identified as part of a wider exercise to review the evidence on the effectiveness of access control as a method of reducing crime in the physical environment. The keywords used when initially searching the electronic databases were therefore broader than (although included references to) alley gating. Overall we used four search tactics to identify relevant studies: (1) A keyword search¹ of electronic databases including gray literature and dissertation databases²; (2) A hand search of relevant journals³; (3) A keyword search of publications by relevant government, research and professional agencies⁴; and (4) forward and backward citation searches of all studies that met our meta-analysis inclusion criteria. No date restrictions were applied. Due to available resources studies did, however, have to be available in English. Our final list of studies was then checked by two recognized experts on alley gating.

1. Search terms available on request.

2. ASSIA (Applied Social Sciences Index and Abstracts), Criminal Justice Abstracts, Criminal Justice Periodicals, ERIC (Education Resources Information Center), IBSS (International Bibliography of Social Sciences), NCJRS (National Criminal Justice Reference Service), ProQuest theses and dissertations, PsycINFO, PsycEXTRA, SCOPUS, Social Policy and Practice, Sociological Abstracts, Web of Science, CINCH (Australian Criminology Database).

3. These were *Police Practice and Research: An International Journal* and *Policing: A Journal of Policy and Practice* which, unlike most criminology journals, do not routinely feature in electronic databases and were therefore searched manually.

4. Center for Problem-Oriented Policing (Tilley Award and Goldstein Award entries), Institute for Law and Justice, Vera Institute for Justice (policing publications), Rand Corporation (public safety publications), Police Foundation, Police Executive Research Forum, The Campbell Collaboration reviews and protocols, Urban Institute, European Crime Prevention Network, Swedish National Council for Crime Prevention, UK Home Office, UK College of Policing (Polka), Australian Institute of Criminology, Swedish Police Service, Norwegian Ministry of Justice, Canadian Police College, Finnish Police (Polsi), Danish National Police (Politi), The Netherlands Police (Politie), New Zealand Police, US National Institute of Justice. We also searched: Google, Google Scholar, Academic Search Premier (EBSC), ProQuest Sociology, Rutgers Criminal Justice Gray Literature Database, OSCE Polis Digital Library.

Data Extraction for Meta-Analysis

For those studies eligible for meta-analysis, two study authors independently extracted an explicitly defined list of items, where the information was available. This included information concerning date, location, research design, implementation, costs and statistical outcomes.⁵ Any disagreements in coding were resolved through discussion.

Quantitative Data Analysis

The quantitative analyses reported in this review included a proportional change analysis (for studies using a before and after design), and a statistical meta-analysis to produce a weighted mean effect size from individual effect sizes. Odds ratios or relative risk ratios are commonly used in systematic reviews of place-based crime prevention interventions (e.g. Johnson, Guerette, & Bowers, 2012; Welsh & Farrington, 2008), and are also used here. We acknowledge that there is some controversy surrounding some aspects of this metric, such as whether the computed statistic is in fact a risk ratio or an odds ratio,⁶ and whether it sufficiently deals with over-dispersion in the data (see Marchant, 2004, 2005). To address this, we use an approach adopted elsewhere (Farrington et al., 2007; Weisburd, Telep, Hinkle, & Eck, 2008) of multiplying the standard error by an inflation factor (IF, in this case two) when calculating confidence intervals.⁷

Another form of analysis often reported in evaluation studies of SCP concerns the geographical displacement of crime—or diffusion of benefits—to untreated nearby areas (see Clarke & Weisburd, 1994; Hesseling, 1994; Guerette & Bowers, 2009). Displacement and diffusions of crime control benefits are also increasingly considered in systematic reviews (see Bowers, Johnson, Guerette, Summers, & Poynton, 2011; Braga, Papachristos, & Hureau, 2012; Johnson et al., 2012; Telep, Weisburd, Gill, Vitter, & Teichman, 2014). We do so here where data permit. Research also suggests that benefits can be diffused in time as well as space, whereby the effects of intervention persist after an intervention has ceased (Sherman, 1990) or are realized before implementation begins or is completed (Johnson & Bowers, 2003; Smith, Clarke, & Pease, 2002). Regarding the latter, the staggered implementation of alley

5. A full list of these items is available on request.

6. Farrington, Gill, Waples, and Argomaniz (2007) have recently referred to the test statistic as a measure of relative effect size when evaluating place-based interventions rather than an odds ratio. As the distinction may be seen as largely semantic, we use the term odds ratio here but acknowledge the issue.

7. Doing so leads to larger confidence intervals and a more conservative test. However, it should be acknowledged that it is still possible that the true effect size will not be captured by the intervals derived.

gates might have anticipatory benefits insofar as offenders aware that prevention activity was planned would be uncertain of where or when treatment would next occur, thus avoiding untreated areas too. Although plausible, in practice none of the studies we identified considered or attempted to measure anticipatory benefits.

Realist synthesis

The approach taken in the realist branch of our review differed from that of our meta-analysis. A key objective of any realist synthesis is to better understand the conditions in which different outcome patterns are generated. To this end, proponents of realist synthesis argue that a wider range of evidence types can legitimately be drawn on than with a meta-analysis (see Pawson, 2006). The issue is that of eliciting and refining working theories and assembling the strongest available evidence to test them. The above four search tactics produced our initial population of studies. Our realist review was a largely qualitative process involving three members of the research team reading, rereading and regularly discussing the full text of all identified research articles deemed relevant to alley gating. We drew on reported findings to help develop and refine working theories for alley gating as a crime reduction method. Following this, ad hoc iterative searches were made for further evidence to supplement the material assembled through the systematic search processes.

Results

Search Results and Screening

Our initial searches on the broad topic of access control identified over 10,000 potentially eligible records (excluding duplicates). Screening of the titles and abstracts resulted in 1,142 records remaining. We then narrowed our searches to those reports relevant to alley gating. The full text of eighty-nine candidate studies were sought and examined independently by the same two review authors using the inclusion criteria described previously. Disagreements were resolved by discussion and, where necessary, through the involvement of a third reviewer. Of the eighty-nine candidate studies, the full text of 3 could not be located and a further 43 did not meet our inclusion criteria. Forty-three studies were therefore judged relevant to alley gating, all of which were reviewed as part of our realist synthesis. Of these 43, 6 used a quasi-experimental design and were eligible for meta-analysis. Most studies were ineligible due to inadequate research designs with which to compute an effect size.

EFFECT: Meta-Analysis of the Impact of Alley Gating on Burglary

Pre-post studies (proportional change)

Evaluation designs that include data for a control area (or areas) are preferred to those that do not, since they allow for a more reliable estimate of the counterfactual. However, because few studies identified through our searches reported data for control areas ($n = 6$), we first analyzed the more complete pre-post data before focusing solely on the data from quasi-experimental studies, acknowledging that some of these studies had low internal validity and that the findings of our proportional change analysis should therefore be interpreted cautiously. In doing this we adopt the same strategy as others faced with similar limitations in the primary studies available (e.g. Weisburd et al., 2008).

The included studies were all based in the UK and exclusively examined the crime type of burglary. Data were available for simple counts of burglary for comparable periods of time before and after intervention for 10 geographic locations, reported across 9 studies (shown in Figure 2). To examine changes in burglary levels across these 10 locations, we computed proportional change

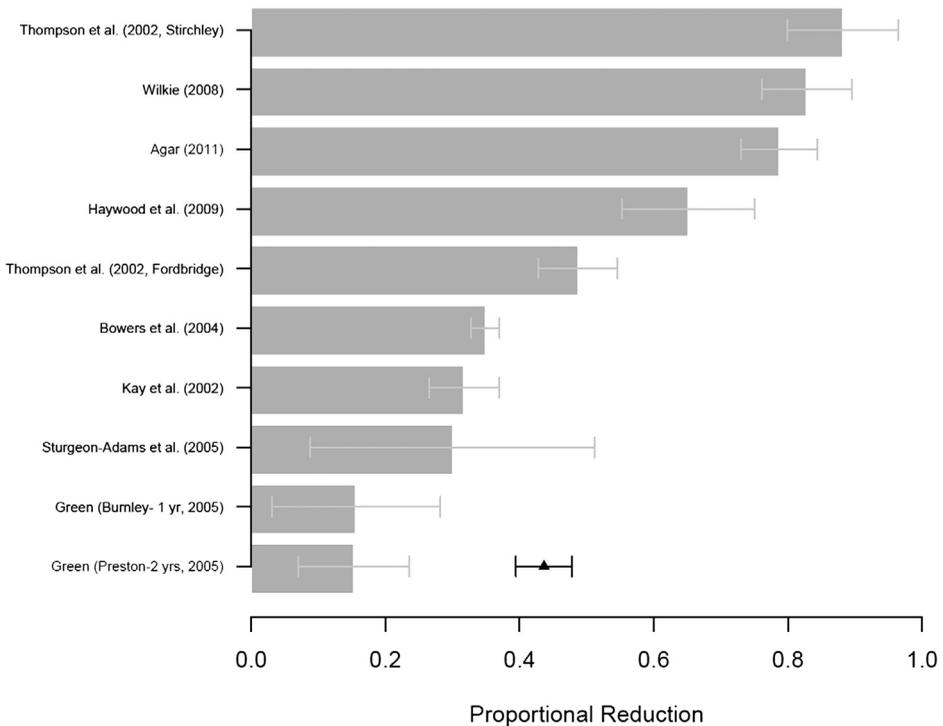


Figure 2 Proportional change in burglary following the installation of alley gates for 9 studies (10 locations) with data pre- and post-intervention.

scores and an overall mean fixed effect size. The values shown in Figure 2 range from zero (no change) to 1 (100% reduction). In addition, we computed confidence intervals for each estimate, and an overall measure of effect size (also shown in Figure 2). Confidence intervals were computed using the method described in Lipsey and Wilson (2001), which is summarized in Appendix 1, as is the formula used to compute the overall weighted measure of effect.

Consistent with the hypothesis that alley gating is associated with reductions in burglary, relative to the period before intervention the count of burglary was lower in all treatment areas post-intervention. The overall weighted mean effect size suggests that this reduction was typically about 43% (fewer burglaries), while the 95% confidence intervals suggest that the effect was reliable and that the true effect ranged between 39 and 48%.

Quasi-Experimental Studies

Six studies reported data for the count of burglary pre- and post-intervention in both treatment and control areas, thereby permitting a more reliable meta-analysis. Locational information for each study is given in Table 1, but in summary, all schemes took place in the UK; two in the North of England, three in the Midlands and one in the South. Before presenting the findings of our meta-analysis, we first report a re-analysis of data originally collected by one of these six eligible primary studies (Bowers et al., 2004), in which the data structure enabled us to estimate the effect of alley gating for a smaller unit of analysis than was possible with the other studies, namely a gated street block rather than an entire area. Given the small number of studies eligible for meta-analysis, we decided to take advantage of access to these primary data to investigate how consistent the overall effect of alley gating was across these individual housing blocks.

Table 1 Estimated cost of alley gates across six British studies

Author and date	Setting	Number of alley gates installed	Cost per gate (in £)
Agar (2011)	Enfield, UK	88	1,090.91*
Bowers et al. (2004)	Merseyside, UK	3,178	659.00
Kay et al. (2002)	Selly Oak, UK	103	158.00
Sturgeon-Adams et al. (2005)	Hartlepool, UK	14	1453.21*
Thompson et al. (2002a)	Fordbridge, UK	44	792.73
Thompson et al. (2002b)	Stirchley, UK	62	212.00

Notes. The costs reported here relate to the time the study took place. Inflation and other financial changes over time might therefore mean that these price estimates might not be directly comparable between studies.

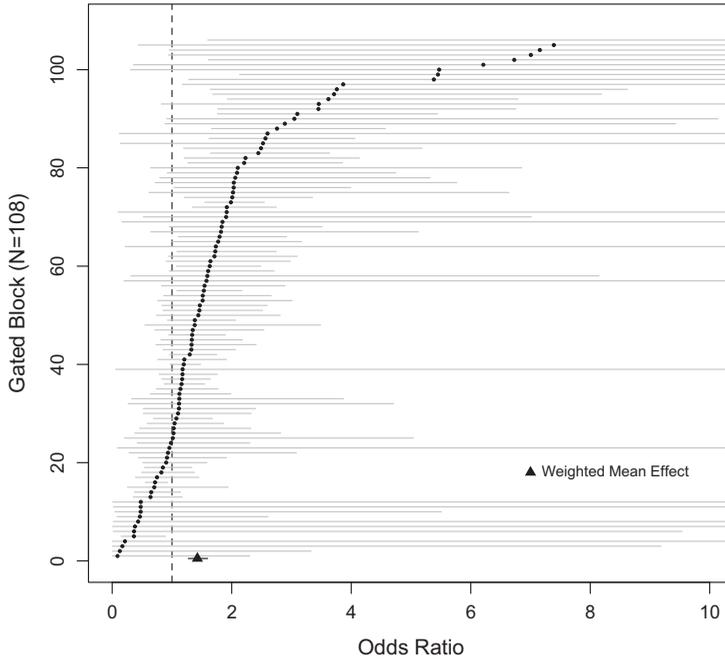


Figure 3 Odds ratios for the 108 alley-gated blocks evaluated in Bowers et al. (2004). Ratios over 1 favor treatment—those with CIs not including 1 are statistically significant.

Reanalysis of data reported in Bowers et al. (2004)

Bowers et al. (2004) report changes in burglary levels observed in 108 gated blocks of around 362 homes in Merseyside (standard deviation = 417, range = 6-3,190 households per block) and those for the wider police force area, minus the treatment areas and a surrounding buffer zone into which crime might have been displaced or crime control benefits diffused. Implementation of alley gates reportedly took place over a period of three years and so there was no simple before and after period. Consequently, in the present analysis we examined the changes observed in each of the 108 housing blocks for the relevant pre- and post-intervention periods and compared these to the changes in the control area (see above) for the same intervals of time. Figure 3 shows odds ratios (ORs) and confidence intervals for these 108 gated blocks. The black dots represent the point estimates, whilst the horizontal lines show the 95% confidence intervals within which the actual value of the OR is most likely to fall. It is apparent that the ORs are overwhelmingly positive, suggesting that, relative to the wider police force area, the rate of burglary (per unit time) typically declined faster in the gated street blocks. Note a weighted

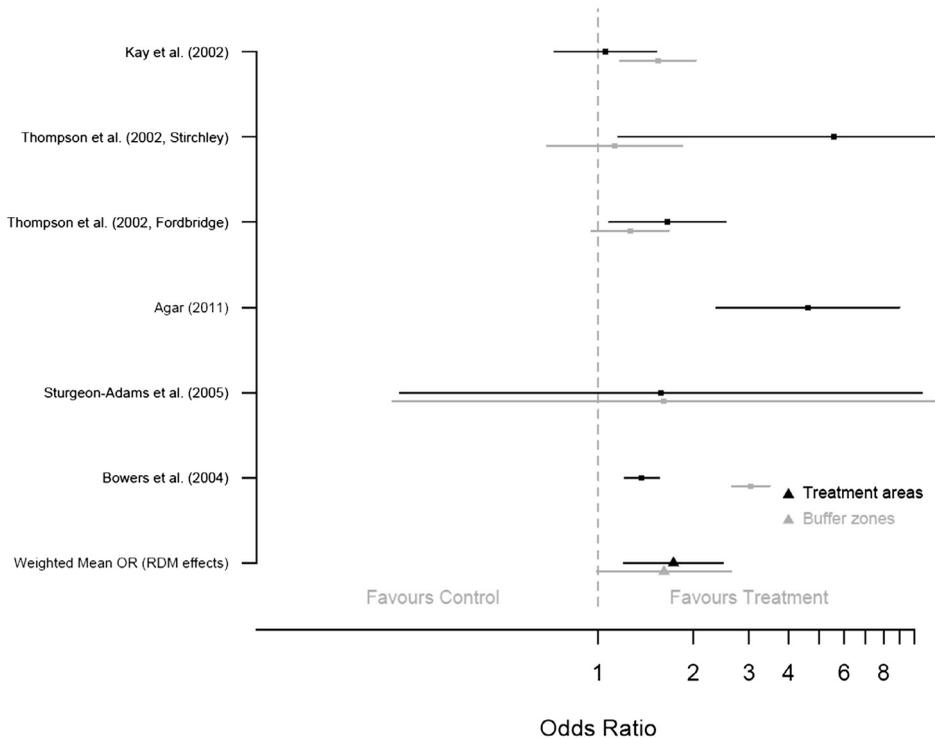


Figure 4 Forest plot of the findings for the six locations where alley gates were installed and where data were available.

mean effect size is shown in the figure but this should be treated with caution as there was dependency in the control area data.⁸

Meta-analysis

We now examine the trends observed in the six alley gating studies where data were available for both treatment and control areas, before and after alley gates were installed. For most of the studies there was a simple before and after period. However, in the case of the Bowers et al. (2004) study, as noted above, implementation was gradual—only 5 per cent of alley gates were fitted

8. As noted, for each of the 108 blocks, the before and after periods differed. However, the control area was always the wider police force area minus the treatment and surrounding buffer areas. As such, the same area (albeit at different times) was used as a control for each gated block. For the 108 individual point estimates considered independently, this is unproblematic. However, when computing an overall estimate of effect size, one assumption of the approach used to estimate the standard errors is that the observations are independent. Since the same geographic area is used as the control area in each case, this assumption is violated and the standard error of the overall estimate of effect size may be underestimated.

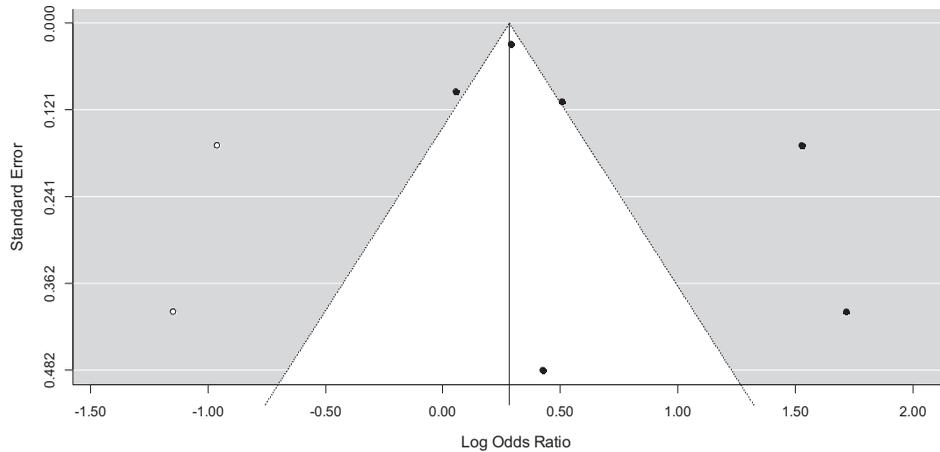


Figure 5 Funnel plot with two further imputed studies (white dots).

in the first year of the study. To deal with this, we aggregate across the blocks and consider the “post” data as the final year of implementation.

For each study, we estimated the intervention effect by computing an odds ratio and a confidence interval that are shown (in black) in Figure 4. An overall weighted mean effect size was computed using a random effects model⁹ (see Appendix 1). The results suggest that, relative to the changes observed in the control areas, burglary numbers fell at a faster rate in the areas in which alley gates were installed. In four of the locations, this effect was statistically significant. The overall weighted mean effect size of 1.73 (confidence intervals: 1.21-2.48) was also statistically significant and suggests that, relative to the control areas, burglary declined substantially in the areas where alley gating was implemented.

We looked for any dependency in the data, explored heterogeneity and outliers, and checked for possible publication bias. We did not detect outliers and therefore are reassured that exceptions did not have an impact on the analysis. All but one of the studies (Bowers et al., 2004) appeared in gray (i.e. non-academic) literature and thus the findings cannot logically be attributable to publication bias, in the narrowest sense.¹⁰ However, as so few studies were available for analysis, it is possible that the results so far presented do not capture the variation that would be observed in a larger population of studies (if it existed).

9. An estimate of the heterogeneity observed across studies ($Q = 13.18$, $df = 5$, $p < 0.05$) indicated that the use of a random effects model was warranted. Each study contributed one effect size only to the mean effect size so dependency in the data was not an issue.

10. Publication bias can arise not just from ignoring the gray literature, but also because of selective reporting on the part of authors or the preferential publication of positive results by scientific journals.

A variety of procedures are available to examine the possible effects of missing studies. We used the trim and fill algorithm proposed by Duval and Tweedie (2000) which imputes the “missing” points on a funnel plot using an iterative procedure which continues until the plot is symmetric. This yields an adjusted estimate of effect size. Figure 5 shows the trim and fill results, suggesting that 2 studies are missing (these are the two additional points, illustrated here as white dots). The original point estimate in log units was 0.717 and confidence intervals 0.171-1.264.¹¹ The adjusted point estimate is 0.283, with confidence intervals of 0.231-0.335. These results demonstrate that accounting for possible missing studies, whilst resulting in a slightly more modest effect size, does not affect the initial conclusions of the meta-analysis.

Two issues warrant mention at this point. First, publication bias exercises such as trim-and-fill are potentially less reliable with smaller populations of studies (for a discussion see Duval & Tweedie, 2000). Second, and of particular relevance to the study of context (or Moderators), is the fact that publication bias is only one of a number of sources that can lead to asymmetry in funnel-plots. Others are documented by Egger, Smith, Schneider, and Minder (1997) and include: location biases, true heterogeneity (e.g. intensity of intervention or differences in underlying risk making the effect size truly differ with study size), data irregularities and measurement artifacts. Ideally, we would conduct a moderator analysis to see if the effect of alley gates varies systematically with particular study characteristics (e.g. in different contexts), and use this in funnel plot interpretation. Unfortunately low numbers preclude such an analysis. However, these caveats aside, the analysis above illustrates that if funnel-plot asymmetry were due to missing studies their addition would not significantly adjust the findings of the meta-analysis. Hence this serves as a useful sensitivity test.

Spatial displacement

Since alley gates may be associated with spatial displacement or the diffusion of crime control benefits, where possible we examined any changes in crime outcomes in the immediately surrounding areas (or buffer zones). Data were available for five of the six studies (not Agar, 2011). In four of these studies, changes were examined in a surrounding buffer area and for one period of time post-intervention. In the case of the Bowers et al. (2004) study, data were available for two different buffer areas that surrounded the treatment area (200 m and 1 km wide). In what follows, we analyze the data for the former (200 m) and define the post-intervention period as the final year of implementation (see above).

11. Note that these estimates do not compensate for over-dispersion.

For each buffer area we computed an odds ratio in the way described above. This procedure—computing a separate effect for the buffer area—was recently used in a systematic review of spatial displacement (Bowers et al., 2011). The results of these analyses are shown in gray in Figure 4. In each case, burglary declined in the buffer areas at a rate that exceeded observed changes in the control areas. There are two leading explanations for this finding. The first is that rather than displacing crime, alley gating appeared to be associated with a diffusion of crime control benefits. That is, the positive effect of the intervention spread to an area wider than the treatment boundary. The weighted mean effect size (computed using a random effects model) of 1.62, also shown in Figure 4, was statistically significant.¹²

An alternative explanation is that the reductions in burglary observed for both treatment and buffer areas can be attributed to a selection bias, whereby, for example, there might be a general tendency for both treatments and buffers to be situated in high crime areas, and are therefore likely to both experience regression to the mean, falsely presenting as reduction effects. Whilst the latter seems less likely as a result of measurement of change against a control, it can't be ruled out with the exclusive use of before and after quasi-experimental designs. Bowers et al. (2004) reported the findings of a time series analysis, which demonstrate a clear dose-response association between the timing and intensity of implementation and crime reduction. This study also examined changes in the spatial distribution of crime more precisely, and showed that the reductions observed in the buffer zone exhibited a distance decay pattern, with the majority of the reduction being evident immediately adjacent to the treatment areas.

Mechanisms

We now turn to the realist branch of our review, drawing on a broader range of studies than those included in our meta-analysis. To reiterate, “mechanisms” are taken here to describe “how” and “why” alley gates might contribute to crime reduction. On reading the 43 studies judged relevant to alley gating, we identified six dominant mechanisms through which alley gates might generate positive crime prevention outcomes, though in many scenarios these causal mechanisms might be expected to operate in concert. In addition, we identified a seventh mechanism through which alley gates might contribute to *increases* in crime.

It should be noted that the studies considered as part of our realist review did not contain *empirical evidence* on mechanisms (or moderators), a point we return to in the Discussion. For that reason, what follows is not a quantitative

12. A similar but marginally non-significant mean effect size was found using the 1 km in Bowers et al. (2004).

summary of the evidence but instead more of a qualitative review of what was described in the primary studies. Moreover, oftentimes study authors did not describe alley gating using terms such as “mechanisms”. Consequently, it was often difficult to attribute suggested mechanisms to particular studies. In this review, however, where studies did explicitly refer to the causal mechanisms presumed to be responsible for the observed outcomes, this is acknowledged.

Increasing the Effort

Alley gating puts physical barriers in the way of would-be offenders and, quite literally, makes it harder to commit crime (e.g. see Agar, 2011; Armitage, 2006; Green, 2005; Kay, Hearnden, Millie, Mallender, & Kingsnorth, 2002). Prospective offenders are unable to access targets (such as properties to burgle) or use the alleys as a way to escape the scene of crime and evade detection. The gates also restrict access to the alley which previously provided an enclosed and potentially unsupervised location to commit crime (such as drug dealing). Crucially, unlike other mechanisms described below, an increase-effort mechanism places relatively little emphasis on the actions of affected residents. The only requirement this mechanism makes of residents is the diligent closure of the gates.

Territoriality, Guardianship and Surveillance

Alley gates might prevent crime by increasing the (perceived) risk of committing an offense by extending guardianship and assisting natural surveillance. Theories of “defensible space” suggest residents assume little control or responsibility for areas occupied by numerous anonymous residents (Newman, 1972). Gating—especially if coupled with signs of ownership (such as the hanging of plants in the gated alley)—might function to generate a sense of ownership or territoriality. The creation of community sentiments around well-defined shared space behind gates may increase informal social control (Sampson, Raudenbush, & Earls, 1997). By converting once public space into private space, residents could be motivated to take greater pride and care for the alley (through, say, installing plants and trellises and maintaining a clean environment) and feel more empowered to act as capable guardians (Cohen & Felson, 1979), challenging those who may have no legitimate reason to be present.

Many alley gating initiatives in Britain were funded by monies designed to encourage community regeneration and cohesiveness, which speaks to this mechanism (Rogers, 2013). However, changes in potential indicators of guardianship and surveillance before and after the installation of alley gates were not systematically tested in the literature reviewed. Johnson and Loxley (2001) report anecdotal evidence of residents in Manchester (UK) installing

plants and shrubbery in reclaimed (gated) alleys, which they interpreted as signs of affected residents taking greater pride in their area, and evidence of residents in Liverpool (UK) being reportedly more comfortable using alleyways once they are gated. Unlike the increase-effort mechanism discussed above, to operate effectively this mechanism asks a great deal of affected residents. It assumes that changes to the physical environment (gating) will initiate a change in the behavior of residents, in particular a willingness to display greater “togetherness” in ways that resonate with and ultimately deter prospective offenders.

Removing Excuses

Through blocking off alleys, gates might create symbolic indicators of private space which would mean that anyone crossing the newly created threshold would feel at an increased likelihood of being challenged by affected residents. If this mechanism were activated, it would not matter if alley gates failed to comprise a physical barrier that made entry more difficult or if the gates were not always locked; gates could simply act as a persuasive indicator of a boundary between public and private space, within which resident intervention is more likely.

“Fixing” Broken Windows

The “broken windows” mechanism suggests that failure to control antisocial behavior leads to signs of incivility that in turn creates the impression that further criminal behavior is normal and permissible (Keizer, Lindenberg, & Steg, 2008; Wilson & Kelling, 1982). In the context of alley gates, open access alleys often comprise unregulated spaces where signs of disorder are produced, creating a permissive environment for crime. Closing them creates orderly space providing cues to suggest that this is not a suitable place to offend because the risk of disruption, detection or arrest is high. Operated effectively, the gate leads to the maintenance of an ordered environment that is cared for because residents take ownership of it.

The activation of this mechanism is dependent on whether residents and various local authorities (i.e. refuse collectors, street cleaners and potentially the police) are motivated to keep the space within gated areas tidy and cared for. Information from residents collected several years after the installation of alley gates suggests that such ordered environments are indeed commonplace. Armitage and Smithson (2007), for example, found residents in gated areas reported encountering considerably fewer cases of littering, dog fouling and public urination than they had previously.

Deflection

Areas known to have many alleys may attract offenders because of the tempting crime targets expected there. Alley-rich areas therefore feature in the “awareness spaces” of more individuals, some of whom will act upon available crime opportunities (Brantingham & Brantingham, 2008). Gating some alleys could reduce the attractiveness of the area more generally and remove it from the awareness space of criminally inclined individuals (Armitage, 2006). If activated, this mechanism might plausibly generate crime falls in local streets without alley gates as well as those with them—a diffusion of benefits—as was suggested by our meta-analysis.

Increases in Offending Through Reductions in Guardianship

Through extending guardianship and improving natural surveillance, alley gating is presumed to generate positive crime prevention outcomes. However, potentially they could function to increase offending. If alleys were previously well-used by residents in their day-to-day routines, gating them might function to reduce their usage, increase offenders’ perceptions of anonymity and thereby increase crime. Haywood et al. (2009) described how gating led to a reduction in footfall in an alley which had previously conveniently linked a park and food store. This was thought to be associated with an increase in burglaries because would-be offenders felt less likely to be observed than before. Indeed, gating might remove alleys from the routine surveillance of passers-by who would otherwise act as capable guardians. Rogers (2013) found that residents in gated areas in Wales seldom used alleys following the installation of gates, particularly after dark. Unfortunately the study was unable to assess whether reduced usage of alleys was associated with increases in crime.

Moderators

The mechanisms underpinning crime reduction measures are rarely, if ever, activated unconditionally. Moderators are understood here to refer to distinct contexts (such as different physical locations) or to the *pre-existing* conditions of a place (for example, the historical nature of a particular neighborhood) that might facilitate or undermine the success of interventions. Moderators are distinct from matters associated with implementation which are discussed in the next section.

Neighborhood Context

The profile of the residents affected by alley gates is important. Gating will be less effective if access to gate keys is not well-regulated or if the number

combination of gate key-pads (where used) becomes widely known. Where neighborhoods have a high turnover of residents, the number of residents with access to keys and passcodes may increase. For example, Millie and Hough (2004) describe a burglary reduction project where alley gates were installed in an area characterized by a large student population. It proved difficult to keep track of the keys and residents often propped the alley gates open.

Resident and Community Investment in Alley Gating

To operate effectively, gating is often dependent on changes in the behavior of residents. Where residents do not take personal responsibility for informally regulating the gates, effectiveness is likely to be reduced (Haywood et al., 2009). Residents need to incorporate the gates into their daily routines, something which is likely to take time. The existence of committed residents might not be essential but their presence will likely increase the probability of activating crime reduction mechanisms. In this regard, Haywood et al. (2009) note that gates are not simply physical interventions but “living” ones relying on ongoing buy-in and commitment from residents to make them work.

Community Cohesion

Gating programs might function to generate community cohesion which in turn might lead residents to work together cooperatively to prevent crime. However, this cannot be assumed. The successful installation and operation of alley gate schemes might be dependent on the existence of an already cohesive community. High crime rate neighborhoods are often associated with low social cohesion (e.g. Hirschfield & Bowers, 1997; Sampson, Morenoff, & Gannon-Rowley, 2002). Given that gating programs are generally—though not always—proposed as a solution in high crime areas, this may make them difficult to deliver. Areas characterized by high numbers of renters or a high turnover of residents might be especially problematic as they may lack the interest in working together to introduce safety measures in a locale where they have little long-term investment. Indeed, home ownership is so important that it may inoculate against many of the problems of implementing alley gating in high crime rate locations (Rogers, 2013).

In some circumstances bringing residents together has undermined community cohesiveness. Mutual hostility between advocates and opponents of schemes may generate a fractured community. Haywood et al. (2009) reported the deleterious effect that one awkward resident, who would not close the gates and made threats when approached by residents to do so. Concerted efforts to resolve the situation made little difference to the behavior of this aggressive individual. The sense of fear amongst residents participating in Haywood et al.’s (2009, p. 376) research “demonstrates the dependency of the implementation

and operation of gating on the cooperation of all scheme residents and the fragile nature of community cohesion". Indeed, in the most extreme cases alley gates, although mooted to promote cohesiveness, may function to generate a more exclusive and divided society (Rogers, 2013), in ways similar to those found for gated communities (see Addington & Rennison, 2013).

Physical Environment

The "broken windows" mechanism described previously assumes that alley gates help contribute to an orderly and cared for environment, which might function to discourage offenders. However, evidence suggests that this can be undermined. Build-up of rubbish behind gates appears frequently in the literature and could be interpreted as indicating an uncared for area (Rogers, 2006). This may occur where residents are unable to access the alleys or where refuse collectors decline to enter the gated alleyways or do not have access to them. This can be linked to the design and installation of the gates themselves. Some gates have proved unfit for purpose. In one study in the UK, Thompson et al. (2002a, p. 14) observe that "it became clear the gates were inappropriate. They quickly became rusty and unsightly, could be "kicked in" relatively easily and residents found them very noisy".

Implementation

There are a number of guides concerned with the implementation of alley gates (see Armitage, 2006; Beckford & Cogan, 2000; Johnson & Loxley, 2001). The following is a narrative summary of the practical tasks intended to create conditions within which alley gating is most likely to work effectively.

Consultation with and Consent of Residents

A precursor to successful alley gating is gaining the consent of residents. Many studies discuss this matter, underscoring its importance (Adamson, 2005; Armitage & Smithson, 2007; Bowers et al., 2004; Haywood et al., 2009; Johnson & Loxley, 2001; Rogers, 2006, 2007, 2013; Sturgeon-Adams, Adamson, & Davidson, 2005). In Britain, agreement by local residents is necessary before alleys can be gated and the majority of property owners must consent.

Resident Commitment to and Use of Gates

Following on from the above, residents need to agree on the rules that govern the use of the gates and be committed to following those rules if alley gates

are to function effectively. Studies have revealed that where residents do not consent they will not use them properly, leaving them insecure (Haywood et al., 2009; Rogers, 2013). Residents commonly express concerns about the arrangements for the maintenance of gates, the impact on services and utilities such as rubbish collection, noise, access to keys, wheelchair access, safety and a stigmatizing impact on a neighborhood (Adamson, 2005; Johnson & Loxley, 2001; Millie & Hough, 2004; Sturgeon-Adams et al., 2005). Community buy-in and engagement needs to be both initially stimulated and maintained. Since the population will invariably change over time, the nature of problems in an area may evolve and residents might otherwise “forget” what the gates were originally intended to achieve and why (Rogers, 2007, 2013).

Routine Gate Locking

The literature is replete with examples of residents failing to lock or close the gates and in so doing undermining potential crime prevention mechanisms (although this is not always the case, see Armitage & Smithson, 2007). This can happen for a number of reasons. Otherwise supportive residents may become frustrated where the gates are inconvenient and leave them open. It follows that resident misuse of the gates might be compounded where unsuitable gates and lock procedures have been implemented. Some gates are not easy to lock. Others are not self-closing. Unsupportive residents may purposefully undermine gating initiatives.

Consultation with Local Authorities

Early consultation with local service providers potentially affected by the gating of alleys is necessary to ensure that local services (from emergency services to the regular collection of rubbish) are not compromised and relevant personnel can access the alleys. There may also be matters to do with cables, wires, pipes and sewers under the ground that need to be considered.

The “Status” of the Alley

Implementation of alley gates rests on identifying ownership of the alleys. In the UK alleys are sometimes public rights of way owned by local authorities. In these circumstances establishing alley gates can be complex and time consuming. The local authority may be required to transfer ownership of the land to local residents who in turn would be responsible for maintaining it. This process can be costly and requires negotiations with the local authority. In other circumstances alleys are owned privately by one or more residents. If so, they can be closed with permission of all home owners, provided they can be identified.

Features of the Gates

Successful operation of alley gating will depend on whether a suitable gate/lock combination has been selected. Where the gates are operated with locks and keys, decisions need to be made about who distributes the keys (for example the police or a residents' group), how to handle keys/key codes in areas where turnover of residents is high and when to change key pad codes (which are likely to become widely known over time). The gate needs to be appropriate for the site (potentially to facilitate access to vehicles and people). It needs to be strong (but not so solid that it inhibits natural surveillance) and large enough to prevent people jumping over it. The gate might need lighting at night time. There needs to be consideration of esthetics as well as security. The design will depend on the physical dimensions of the alley, the amount of money available to pay for it and what local residents want. In some circumstances gates need planning permission from a local government authority. Reaching agreement for who is responsible for maintaining the gate over time is important.

Economics

Our final section is based only on those six studies included in our meta-analysis, all of which included information on the costs associated with alley gating (Table 1). The financial costs of gating include installing, insuring and maintaining the gates as well as consulting the community, particularly those whose property is likely to be most affected. These costs will also vary according to the number of affected residents, the number of gates installed, the type of alley gate and so on. In four of the six studies, the estimated cost per gate was reported and, in the remaining two (marked by an asterisk), we were able to compute costs by dividing the reported total expenditure on alley gating by the number of gates installed. Across the six studies the median cost per alley gate was £726. The lowest reported cost per gate was £158 and the highest was £1,453.

The significance of any financial outlay should be judged in light of expected returns on investment. For crime prevention, returns ordinarily refer to the financial gains associated with crimes averted following intervention. This can be calculated using published estimates on the average cost of various crime types (see Dubourg & Hamed, 2005). Five of the six studies included in our meta-analysis generated cost-benefit ratios to assess the total amount spent on burglary reduction against the estimated savings (or losses) associated with burglary reductions (or increases). These are displayed in Table 2 and denote the financial loss or savings for every one pound spent.

Four of the five studies reported cost beneficial results. Several points should be kept in mind, however, when interpreting the findings in Table 2. Firstly, cost benefit analysis was performed using the total amount spent on *all*

Table 2 Cost-benefit ratios of amount spent on alley gating and the associated costs of burglary reductions and/or increases

Author and date	Setting	Cost benefit ratio
Bowers et al. (2004)	Merseyside, UK	£1:£1.86
Kay et al. (2002)	Selly Oak, UK	£1:£2.79*
Sturgeon-Adams et al. (2005)	Hartlepool, UK	£1:£2.19
Thompson et al. (2002a)	Fordbridge, UK	£1:£1.24*
Thompson et al. (2002b)	Stirchley, UK	£1:£1.87*

Notes. Those studies marked by an asterisk denote *modeled costs* which “convert costs to a common price base (GDP deflated) and point in time (discounted). The modeled costs for Stirchley SDP use net present values at April 1999 prices, and the cost of capital items (equipment) as an amortized value, based on the expected life of the assets and the time they were used in the intervention. This assumes that the assets continue to be available beyond the end of the project” (Thompson et al. 2002a, p. 25).

burglary reduction measures and not specific interventions (such as alley gates). Moreover, in all but one study (the exception being Bowers et al., 2004) alley gates were evaluated as part of a suite of measures designed to reduce burglary. Second, Kay et al. (2002) and Thompson, Hearnden, Millie, Mallender, and Kingsnorth (2002a, 2002b) report several cost benefit ratios which vary according to whether displacement/diffusion of crime control benefits is included or not. In some cases the difference is considerable. In Fordbridge, for example, Thompson and colleagues report cost benefit ratios of £1.24 to £9.32 for every £1 spent, the former incorporating changes in burglary in the surrounding buffer area and the latter focusing on burglary as a proportion of all acquisitive crime in the target area. Here, we report only the lower estimates.

Discussion

Using the EMMIE framework (Johnson et al., 2015) to structure our review, we collated information on the effectiveness of alley gates, the causal mechanisms through which they are expected to work, the conditions necessary to activate such mechanisms, the implementation of alley gates and the economic costs and benefits of doing so. The results from our meta-analysis suggest that alley gating has been an effective burglary reduction measure, albeit with modest effect sizes. Our findings are interpreted cautiously, however, given the small number of studies eligible for meta-analysis ($n = 6$), all of which used quasi-experimental designs. This warrants mention given previous research has found that non-randomized study designs are more likely to produce positive results than when randomisation is used (see Weisburd, Lum, & Petrosino, 2001).

Table 3 Hypothesized context-mechanism-outcome configurations for alley gates

Context	Mechanism	Outcome
Offenders select targets with which they are familiar	Closing alleys removes vulnerable properties from likely offenders' awareness spaces	Reduction of domestic burglary, non-domestic burglary and theft from the garden and yards accessible from the alley
Alleys provide easy access to targets or a means of escape for offenders	Closing alleys might increase the effort for the offender and increase the risk of being apprehended	
Public alleys provide a legitimate excuse for would-be offenders to survey properties	Closing alleys removes excuses for loitering	
Alley gates are installed in high crime areas with little social cohesion	The process of securing agreement for alley gates to be installed builds social cohesion, enhancing mutual protection	
Open access alleys generate disorder and facilitate further crime and disorder	Closing alleys creates orderly space, providing cues to suggest that this is not a suitable place to offend because the risk of detection/apprehension is high	Reduction of drug use and dealing, prostitution, arson, the accumulation of litter, robbery and anti-social behavior
Open access alleys are unregulated, no-one takes proprietary interest and disorderly behavior goes unchallenged	Alley gates create defensible space where those backing onto the enclosed alleys display territoriality over it	
Alley gates are installed in high crime areas with little social cohesion	The process of securing agreement for alley gates to be installed builds social cohesion, enhancing mutual protection	
Areas known to have many rear alleys attract would-be offenders	Alley gating reduces the attractiveness of the neighborhood for offenders who are looking for vulnerable targets	A diffusion of benefits

There are also several factors that limit the generalizability of our findings. First, all of the studies included in our meta-analysis examined British alley gating programs. It is unclear whether the generally positive results obtained from these studies can be generalized to other settings with different street layouts and housing characteristics. Second, in five of the six studies (the exception is Agar, 2011) the alley gates were purchased using funds provided by the same government scheme (the Reducing Burglary Initiative, see Homel, Nutley, Webb, & Tilley, 2004). In some cases the injection of money was considerable. This is noteworthy since efforts to replicate well-resourced demonstration projects at a reduced cost often fail to produce the same positive outcomes (see Tilley, 1993). Third, we found that alley gates tended to be implemented as part of a package of crime prevention measures. Although this is true of many crime prevention schemes, and is a familiar limitation when synthesizing such studies (see Bennett, Holloway, & Farrington, 2008), we must reiterate the consequent inability to reliably determine the comparative effectiveness of individual interventions.

Drawing on a wider range of studies than those included in our meta-analysis, this review also attempted to identify the causal mechanisms through which alley gating might plausibly reduce crime. Table 3 outlines the outcomes that are expected to follow from the activation of particular mechanisms in given contexts.

By far the most commonly assumed mechanism in the literature we reviewed was that alley gates produce a crime reduction effect by increasing the effort of prospective offenders. A virtue of this mechanism is that it requires little input from affected residents, its only requirement is that the gates are regularly closed—it is therefore comparatively *context-insensitive*. The same cannot be said for some of the other mechanisms through which alley gates might conceivably reduce crime—such as removing excuses for loitering, creating orderly spaces and so on—all of which have higher thresholds of activation and variously require input from affected parties (such as residents, municipal agencies, police).

Although we contend that the information reported here on mechanisms and moderators is relevant to decision-makers who are considering investing in alley gating, an important observation in undertaking this review is that the evidence gleaned is, for the most part, only *suggestive* of how alley gating may produce effects and the conditions that are relevant to variations in outcome patterns. It had been hoped that the data collected from primary studies included in our meta-analysis would enable a moderator analysis to test conjectures about mechanisms and the conditions for their activation. Unfortunately, such data were not available in the studies reviewed here. Future primary evaluation studies should focus on collecting data on these underlying mechanisms (see also Weisburd, Hinkle, Braga, & Wooditch, 2015). This may require primary data collection, such as the use of observation methods to determine the extent to which alley gates are closed or the presence of signs of disorder before and after the installation of alley gates. Collecting such

numbers would enable a much sharper evaluation of alley gating, and help in determining whether the putative mechanisms described here are responsible for the outcome patterns associated with alley gating.

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Meta-Analytic Formulae

Proportional Change Analysis

To compute confidence intervals:

$$ES_i = \frac{p}{k} \tag{1}$$

$$SE_i = \sqrt{\frac{p(1-p)}{n}} \tag{2}$$

$$\omega_i = \frac{1}{SE_i^2} \tag{3}$$

where ES_i is the measure of effect size for study location i , SE_i is the standard error of the estimate, ω_i is the inverse variance weight, n is the count of crime pre-intervention, and p is the proportional change.

To compute an overall weighted measure of effect size we use the equation:

$$\bar{ES} = \frac{\sum_{i=1}^n \omega_i * ES_i}{\sum \omega_i} \tag{4}$$

Odds Ratios

The odds ratio is computed as follows:

$$OR = \frac{Trt_{before} \times Ctrl_{after}}{Trt_{after} \times Ctrl_{before}} \tag{5}$$

For computational reasons, the standard error (SE) is calculated for the natural logarithm of the OR ($LOR = \log_e(OR)$) rather than the raw OR. The formula for computing the SE for LOR is as follows (IF is an inflation factor, see below):

$$SE_{LOR} = \sqrt{\frac{1}{Trt_{before}} + \frac{1}{Trt_{after}} + \frac{1}{Ctrl_{before}} + \frac{1}{Ctrl_{after}}} \times 2 \quad (6)$$

Confidence intervals are then computed in the usual way (by multiplying the SE by 1.96 and adding and subtracting this value from the LOR to get the upper and lower estimates of the interval) and the estimates are then exponentiated to make the values easier to interpret in terms of the difference in likelihood of a positive outcome between the treatment and control groups.

Estimating Mean Effect Sizes

To do this, we first computed the inverse variance weights for each OR using the formula shown in (3). The weighted mean effect size is then simply:

$$\bar{OR} = \frac{\sum(\omega_j \times LOR_j)}{\sum \omega_j} \quad (7)$$

The above (fixed effects) formula assumes that any variation in effect sizes observed across locations (or studies) is due to sampling error alone. However, it is also possible that there is real variation across studies (e.g. due to contextual differences). To account for this, we use a random effects model to compute the weighted mean effect size (see Lipsey & Wilson, 2001), this essentially serves to increase the estimated standard error of the estimates.