



Final report

Reducing burglary through Secured by Design: a meta-analysis

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Executive summary

Secured by Design (SBD) is a UK-based crime prevention initiative that sets security standards for the built environment. Buildings that meet prescribed security standards are awarded SBD status and, all things being equal, are assumed to be at lower risk of crime. SBD draws heavily on the principles of situational crime prevention and crime prevention through environmental design.

This study reviewed the evidence on whether properties that meet SBD standards experience less crime than properties that do not. Following a comprehensive search of the literature, nine UK studies were included in our review. Eligibility was limited to studies that (1) measured levels of crime in SBD developments compared to matched non-SBD developments or (2) reported changes in crime before and after SBD accreditation in a treatment and control group. This sample included studies of new dwellings built to SBD standards ($n = 7$) and existing dwellings refurbished to SBD standards ($n = 2$).

Our analysis found that eight of the nine included studies reported that SBD accreditation was associated with lower levels of burglary. In only three of these studies was the observed effect statistically significant, however. Synthesising data across all nine studies, our results indicate that overall, burglary in SBD properties was 53% lower than in non-SBD properties. The impact on burglary was greater still when focussing only on *new properties* built to SBD standards. For this sample of studies, we found a 65% relative reduction in burglary compared to matched non-SBD properties. No statistically significant effect was observed for other forms of crime and disorder. Insufficient data meant that we could not measure the impact of SBD specifically in refurbished properties nor could we test for crime displacement or diffusion of crime control benefits. Importantly, there was no evidence of SBD leading to an *increase* in crime and disorder.

The findings from this review support the general hypothesis that more secure households experience less burglary. They also lend further support for the basic idea that crime can be reduced by removing opportunities to commit it. From a policy perspective, our results re-emphasise the importance of engaging with manufacturers and design professionals to ensure that crime prevention is considered at the design and planning stage of urban developments. More specifically, our findings indicate that upgrading and/or maintaining

effective SBD standards is a sensible strategy to reduce burglary. Simply put, properties which met SBD standards were significantly less likely to be burgled.

Although the results of this study are generally positive, there are some notes of caution. Only a small number of studies met the inclusion criteria for this review ($n = 9$). This was most pronounced for studies of existing properties refurbished to SBD standards ($n = 2$). Of those studies that were included in the review, weaknesses were identified concerning study design and limited data were provided on factors such as the quality of SBD measures and crime displacement. Moreover, three of nine included studies are now over twenty years old and relate to SBD standards that have been superseded. Relatedly, the quality of security in *non-SBD* properties has, in general, improved over time, influenced by a combination of technological developments, policy changes and, potentially, the growth and influence of SBD itself. One consequence of these general improvements is that the difference in security between SBD and non-SBD developments may, on average, be less pronounced today than it was thirty years ago. This raises concerns about the equivalence of SBD vs non-SBD evaluation sites; some properties might meet SBD standards despite not being SBD accredited, thereby muddying SBD vs non-SBD comparisons. Newer high quality evaluation studies are therefore needed to determine whether the positive effects reported here hold in contemporary urban environments, with greater attention paid to the quality of household security being compared across SBD and non-SBD developments. Furthermore, to advance the evidence base for SBD, we recommend that future evaluation studies include a wider range of outcome measures (including displacement) and put data collection measures in place better to understand what specific aspects of SBD affect crime, through what mechanisms and under what conditions.

Background

Incidents of domestic burglary in England and Wales have fallen by over 60% since the early 1990s (Tseloni et al. 2017). Despite these notable reductions, worry about experiencing burglary continues to be common concern among the public (Pohl & Buil-Gil, 2023). Moreover, the experience of burglary is associated with a range of emotional and physical harms (Budd, 1999; Shaw & Chenery, 2007) as well as significant financial costs (Heeks et al., 2018).

Multiple lines of evidence demonstrate the importance of opportunity in explaining burglary patterns. First, research indicates that burglary is highly concentrated in certain places (Tseloni, 2006) and on certain properties (Johnson et al. 2007). Second, weak security is identified as a robust risk factor for burglary victimisation (Pease & Gill, 2011; Tseloni et al. 2017). Third, interviewed offenders consistently identify good security features as a strong deterrent to burglary (Armitage, 2017). Fourth, improvements in and the spread of household security measures provides a persuasive account for the dramatic falls in burglary observed in many countries in recent decades (Tseloni et al. 2017). And fifth, increases in household guardianship as a result of COVID-19 lockdown restrictions was associated with large reductions in domestic burglary (Langton et al. 2021).

Anticipating crime opportunities can be tricky, however. Ekblom (2017) describes the long history of places, properties and products designed without due consideration to their inherent crime risks. 'Crime harvests' invariably follow, whereby offenders take advantage of easy opportunities until retrofitted solutions are put in place (Pease, 2001). Encouraging industry, policy-makers and design professionals to consider crime as part of the design and planning process remains a considerable challenge, especially when the costs of insecure products and properties falls mainly on third parties rather than the designers themselves (Brooke; 2013; Ekblom, 2017).

Secured by Design (SBD) is a UK-based police initiative aiming to reduce crime by improving the physical security of the built environment. Established in 1989 amidst rising burglary rates, SBD, amongst other things, encourages housing developers to design and build properties to specified security standards (see Brooke, 2013). The rationale for and principles of SBD are rooted in Situational Crime Prevention (SCP, Clarke, 1997) and Crime Prevention

Through Environmental Design, defined as ‘the design, manipulation and management of the built environment to reduce crime and the fear of crime’ (Armitage, 2013, p. 23).

At the time of writing, SBD specifications are provided in three sections of their design guides. Section 1 specifications relate to the design and layout of urban spaces, drawing mainly on the CPTED principles of natural surveillance and defensible space. Section 2 and 3 specifications relate to the physical security of the built environment, most notably doors, windows, fences and lighting. These specifications speak mainly to the CPTED concept of target hardening. Achieving SBD accreditation typically involves working closely with police Designing Out Crime Officers (or DOCOs)ⁱ to ensure that housing developments are designed and built to specified security standards (Monchuk, 2019). Adhering to the standards prescribed in Sections 1, 2 and 3 equates to SBD Gold award status, meeting Sections 1 and 2 denotes Silver award status and meeting just Section 2 (physical security) denotes SBD Bronze award. Informed by crime opportunity theories, developments that meet prescribed security standards are, all things being equal, assumed to be at lower risk of crime.

SBD is widely advocated in the UK. It has the support of the Home Office and National Police Chiefs’ Council. It is cited in key planning documents both in England (in 2015) and Scotland (in 2018) (for a review see Armitage, 2018). A similar scheme – Police Label Secure Housing – has been implemented in the Netherlands (Armitage, 2017). Since its inception, the remit of SBD has also expanded to include a wide range of security products and services – at the time of writing, there are 658 SBD member companies covering 6,413 SBD products. From a research perspective, several studies are available on the implementation of SBD (Monchuk, 2019; Monchuk, Pease & Armitage, 2018), the cost-effectiveness of SBD (Armitage, 2000; Association of British Insurers, 2006; Teedon et al., 2009) and the impact of SBD on crime and disorder (Brown, 1999; Pascoe, 1999; Armitage, 2000, Teedon et al., 2009; Armitage & Monchuk, 2011). These studies converge on the finding that adhering to SBD standards is generally associated with both cost savings and reductions in crime, particularly domestic burglary. To date, however, there has been no attempt to systematically review the evidence on whether SBD is associated with changes in crime. That is the objective of this study.

Methods

Eligibility criteria for inclusion in this review

To be included in this review, studies had to meet the following criteria:

1. explicitly mention SBD as the central focus of the research. For this reason, studies concerned with related concepts such as CPTED or SCP but which did not mention SBD were excluded.
2. report quantitative crime outcome measures following SBD accreditation, be that using police recorded crime data or victimisation surveys.
3. report original research findings.
4. use a research design that enabled the computation of an effect size. This could take the form of reporting measures of crime either (a) before and after SBD accreditation in a treatment and control area or (b) after SBD accreditation in a matched treatment and control area.
5. take place in the UK.
6. have a publication date of 1989, which is when SBD was established, or later.
7. be available in English.

It can be seen from the inclusion criteria above that eligibility for this review was not limited to evaluation studies using quasi-experimental or experimental (randomised) designs. This is a departure from the approach commonly adopted in systematic reviews. We have taken this decision because previous research has highlighted the lack of experimental and quasi-experimental studies in SCP in general (Eck, 2006; Guerette, 2009) and SBD in particular (Armitage, 2017). This decision warrants mention, however, since previous research has identified systematic differences in the results of randomised versus non-randomised studies (Deeks et al. 2003). Moreover, Weisburd, Lum and Petrosino (2001) show that evaluation studies using non-randomised designs are more likely to report positive outcomes than when randomisation is used. This is a point we return to in the Discussion.

Literature search

Eligible studies were sought in five ways: (1) a keyword search of eight electronic databases was undertaken in August 2016 and updated in January 2021 (Annex A and B); (2) a keyword

search of publications by relevant government, research and professional agencies (Annex C); (3) forward and backward citation searches of included studies; (4) consultation with SBD members of staff and (5) consultation with an opportunist sample of police officers and staff attending the 2017 SBD national conference.

Screening process to determine eligibility

A two-stage screening process was followed to determine study eligibility. First, two researchers simultaneously screened returned records for eligibility based on title and abstract and using the inclusion criteria above. Once ineligible records had been excluded, the second round of screening, also undertaken by two researchers, involved assessing eligibility based on reading the full text of each document. Coding decisions were compared and discussed and any inconsistencies were resolved through discussion with the third author.

Data extraction

Data from eligible studies were independently extracted by two researchers. This included information on publication date, location, housing type (new build or security upgrades), research design, data source, time period and crime and disorder outcomes. As above, data extraction decisions were compared and any inconsistencies were resolved through discussion with the third author.

Assessment of methodological quality in eligible studies

To assess the strength of evidence concerning the effects of SBD, we also coded for several measures of methodological quality. Informed by previous reviews of SBD (Armitage, 2013), our assessment of study quality comprised five questions: (1) is there a comparison group or condition? (2) if so, was randomisation used to assign units of analysis to treatment and control conditions? (3) is there evidence to indicate that treatment and control conditions were equivalent at baseline? (4) is there evidence that the quality of SBD was assessed as part of this study? And (5) were there any noted implementation issues that may have affected the results? Each question was rated as 'Yes' or 'No'. We acknowledge at this point that our assessment of study quality may relate more to a study's descriptive validity (what is reported)

rather than its internal validity (Farrington 2003). Moreover, we recognise that our assessment of study quality, while performed by multiple authors independently, may involve an element of subjectivity. This is though true of all evidence appraisal and we undertook this exercise to assess the strength of the evidence on which our conclusions are based.

Quantitative data analysis

Meta-analysis was used to synthesise the quantitative data reported in included studies. First, all outcome data reported in individual studies were extracted to create individual effect sizes. Second, effect sizes were converted into odds ratios with associated confidence intervals. Meta-analytic models were generated only when there were three or more studies that reported comparable outcome data. Consistent with recommended practice (see Lipsey & Wilson, 2001), when combining the individual effect sizes into the pooled mean effect, we used inverse weighting thereby affording greater weight to studies with larger sample sizes, which are generally taken to have more stable results and hence have a larger influence on the overall mean effect size (see Annex D). All outcomes were analysed using random effects models and heterogeneity among the primary study effect sizes was quantified using Q and I² statistics (see Lipsey & Wilson, 2001).

Results

As shown in Figure 1, a total of 426 articles were identified through our search of electronic databases (n = 188), other sources (n = 221) and through discussions with SBD staff and practitioners (n = 17). After assessing the title and abstract of these articles, 260 were excluded. Following the second stage of screening – assessing the full-text of remaining articles against our inclusion criteria – we identified nine eligible studies which reported quantitative data on the impact of SBD on crime and disorder.

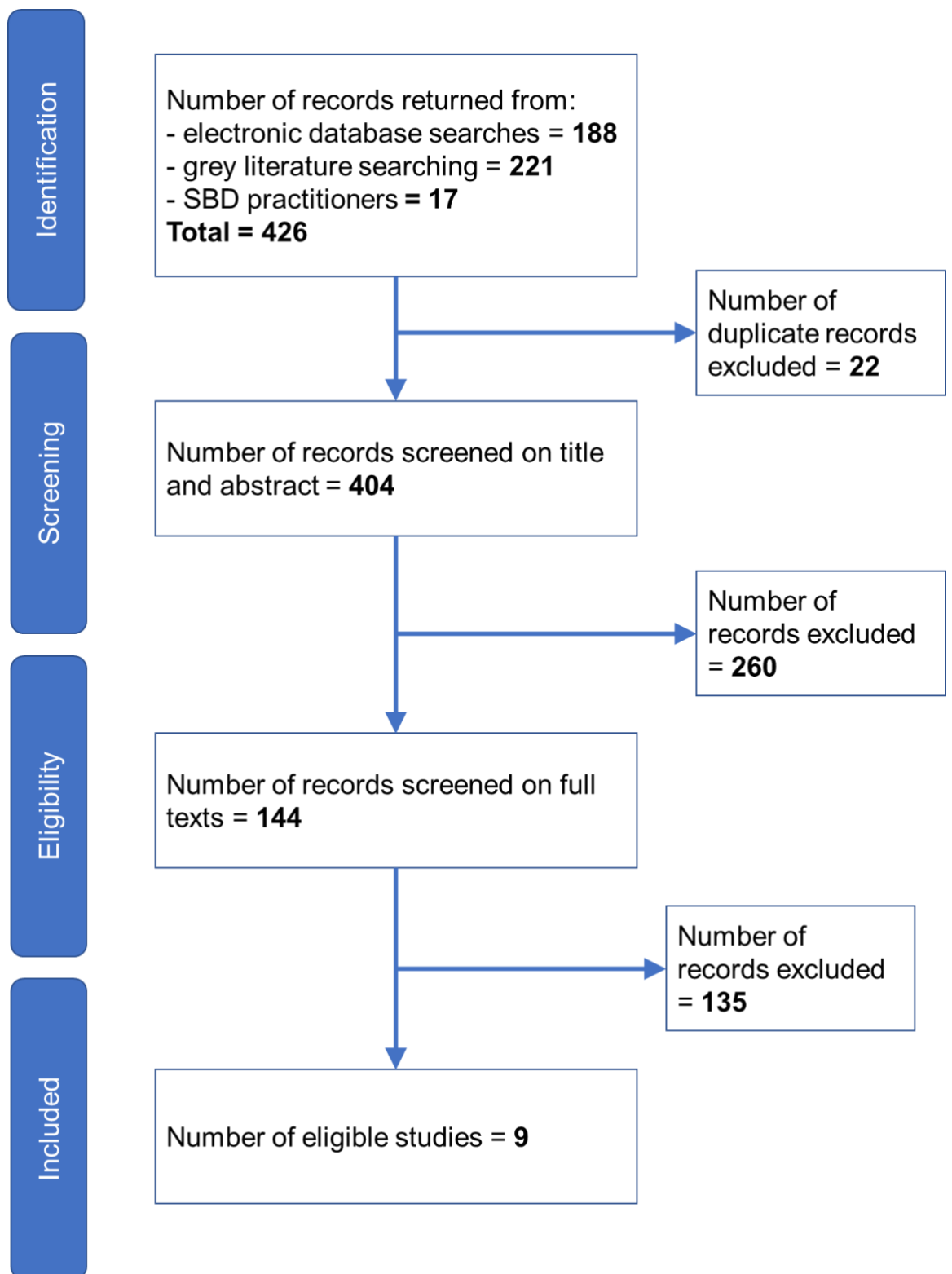


Figure 1. PRISMA flow chart of records through the screening process

A summary of the nine eligible studies is provided in Table 1 (see also Annex E). It shows that these studies covered a range of time periods (1994 to 2018), locations and crime types.

They also relate to two different elements of SBD: security upgrades to existing properties (Teedon et al. 2010; Jones et al. 2016) and new housing developments built to SBD standards.

Five additional study features are considered noteworthy. The first concerns publication status. To the best of our knowledge, only three of the nine included studies were published in scientific journals, either in their current form or in follow-up papers (Armitage & Monchuk, 2011; Armitage, 2006 based on the findings reported in Armitage, 2004 and Teedon et al., 2010). The remaining studies were found in the 'grey literature': a master's thesis (Brown, 1999) and industry reports (Armitage 2018a, 2018b; Bone et al. 1994; Jones et al. 2016; Ward 2017). This warrants mention for two reasons. On one hand, articles published in scientific journals are generally considered to be of higher methodological quality than those published in the grey literature as a result of them being subjected to an independent peer-review process (although see Wilson, 2009). On the other hand, it is well-documented that scientific journals tend to favour publishing positive findings over negative findings (Kim, 2022). For this reason, it is recommended practice when doing a meta-analysis to seek out relevant unpublished studies – as we did here. Failure to do so, and thereby drawing conclusions based only on the findings of published studies, may generate distorted and overly positive results. In this sense, since our final sample of eligible studies is made up mostly of unpublished evaluations, we can be more confident that our results are not affected by publication bias.

The second feature of note concerns study design. The methods used to assess the impact of SBD took two main forms. The first were *case-control studies* which compare crime in SBD developments to crime in matched non-SBD developments. This type of evaluation design is typical of studies assessing crime in *new* housing developments built to SBD standards. In this sense, there is no pre-intervention period. This contrasts to the second form of study design, in which measures of crime were taken before and after intervention in both SBD developments and non-SBD developments (Jones et al., 2016; Teedon et al., 2010). For obvious reasons, this form of evaluation design was typical of studies assessing the impact of SBD security upgrades to *existing* properties. Both forms of study design have their respective strengths and weaknesses. Case-control studies rely on existing data and therefore tend to be both cost and time effective. They do not, however, effectively rule out many threats to internal validity nor do they help establish causal ordering: that the

introduction of SBD *caused* any observed change in crime. Pre-post studies with treatment and control groups overcome these weaknesses. They are, however, prone to selection effects if treatment and control groups are not equivalent at baseline (i.e., before SBD upgrades were installed). This was a noted concern in Jones et al. (2016) but less so Teedon et al. (2009). None of the identified studies used a randomised design, which to some extent controls for possible selection effects.

The third feature of note concerns crime displacement. Efforts to reduce crime by changing the physical environment are often criticised for simply displacing crime elsewhere. In response, evaluations of SCP often include an assessment of whether falls in crime following an intervention have led to spatial displacement (see Guerette & Bowers, 2009; Bowers, 2023). It is noteworthy therefore that although several studies included in our sample made reference to possible displacement effects (see for e.g., Brown, 1999; Teedon et al., 2009; Jones et al., 2016), none reported a formal assessment of spatial crime displacement. Moreover, there were suggestive signs of some tactical displacement whereby offenders adapted their modus operandi in response to improved household security. Jones et al. (2016), for example, report how improvements in window security – upgrading single-glazed windows to double glazed SBD-compliant windows – was associated with an overall reduction in burglary but a small increase in burglaries where access is gained via the door (an aspect of household security which was not changed as part of the evaluated SBD initiative).

The fourth observation relates to SBD quality assurance. Only a handful of studies reported assessing the *quality* of the SBD being evaluated (Armitage, 2004; 2018a; 2018b). This is important in light of evidence that failure to maintain crime prevention interventions can reduce their effectiveness (Bowers et al. 2009). In the context of SBD, Armitage (2018b) reported that prior to data analysis, all SBD sites were assessed to determine whether their design, build and maintenance *actually* reflected SBD principles. Importantly, site assessors noted that despite all sites having SBD accreditation, there was notable variation in the quality of SBD across these sites, perhaps owing to differences in the SBD accreditation process and/or because of poor maintenance over time post-accreditation. We draw attention to this finding as it has implications for the conclusions drawn from evaluation studies, specifically whether a finding of, say, no effect is the result of an ineffective SBD intervention or the poor implementation and/or maintenance of the SBD intervention.

The final feature of note concerns funding. Of the nine eligible studies included in our review, four reported being funded, in whole or in part, by SBD (or its predecessor the Association of Chief Police Officers) (Armitage, 2018a, 2018b, Bone et al. 1994; Teedon et al. 2009). This warrants mention since commentators have expressed concerns that the source of research funding can represent a conflict of interest which may affect 'the design, conduct, analysis and reporting of a trial' (Boutron et al., 2023). This is most pronounced in medicine, where the funders of interventions stand a great deal to gain from commissioning studies which report positive findings (see Lundh et al., 2017). To be clear, in our sample of studies we found no evidence to suggest that SBD-funded studies were more likely to report positive effects. Indeed, two recent studies by Armitage (2018a, 2018b), both funded by SBD, reported non-significant effects of SBD. Moreover, it is important to note that SBD is a non-profit organisation and so there is no financial gain to be realised from positive study results. However, in the interests of transparency, we felt that this pattern deserves mention and, in the same vein, remind the reader that this review was also commissioned by SBD (see potential conflict of interest, page 26).

Table 1. Summary of included SBD evaluation studies (n = 9)

| Study | Location | Published (Yes/No) | Research design | Sample | New build (Yes/No) | Data | Time period | Outcomes | Main reported findings |
|-------------------------------|--------------------|--------------------|---------------------------------|---|--------------------|---------------------------------|----------------------------------|--|---|
| Armitage ⁱⁱ (2004) | West Yorkshire | No | Matched comparison | 25 SBD and 25 non-SBD housing estates | Yes | Police | This varied across matched pairs | 36 crime categories including property and violent crime | Burglary and total crime rates were lower in SBD estates vs. non-SBD estates. These differences were not statistically significant, however. |
| Armitage (2018a) | Devon and Cornwall | No | Matched comparison | 25 SBD and 25 non-SBD housing estates | Yes | Police | April 2012 to Dec 2016 | Burglary, theft, criminal damage and vehicle crime | There were no significant differences in crime rates between SBD and matched comparison estates. |
| Armitage (2018b) | Hertfordshire | No | Matched comparison | 25 SBD and 25 non-SBD housing estates | Yes | Police | Jan 2012 to Dec 2016 | Burglary, theft, criminal damage, vehicle crime and ASB | There were no significant differences in crime rates between SBD and matched comparison estates. |
| Armitage & Monchuk (2011) | West Yorkshire | Yes | Several evaluation methods used | 342 SBD properties vs wider police force area; 16 SBD developments vs 16 non-SBD developments | Yes | Police and resident survey data | Aug 2007 to July 2008 | 9 crime categories including property and violent crime | Burglary rates within SBD properties were significantly lower than across West Yorkshire. Comparing matched SBD and non-SBD developments, there were no significant differences in crime. |
| Bone et al. (1994) | Hertfordshire | No | Matched comparison | 90 SBD properties and 74 non-SBD properties | Yes | Police | Jan 1991 to Mar 1994 | Burglary, car crime | There were no significant differences in burglary between SBD and non-SBD properties. |

| | | | | | | | | | |
|----------------------|------------|----|--------------------------------|---|-----|--------|---|--|--|
| Brown (1999) | Gwent | No | Matched comparison | 1,682 SBD properties and 7,491 non-SBD properties | Yes | Police | April 1996 to Mar 1998 | Burglary, TOMV criminal damage | All crime types were significantly lower in SBD properties compared to non-SBD properties. Suggestive evidence of tactical displacement observed |
| Jones et al. (2016) | Nottingham | No | Pre-post with comparison group | 1,520 SBD properties across two housing estates | No | Police | 1 financial year pre-installation (2007/08) compared 1 financial year post installation (2009/10) | Burglary | There was a statistically significant reduction in domestic burglary (42%) among SBD but burglary also fell (by 38%) in the control sites. Suggestive evidence of tactical displacement observed |
| Teedon et al. (2010) | Glasgow | No | Pre-post with comparison group | 2,028 SBD properties across local housing areas compared to 14,185 non-SBD properties | No | Police | 1 year before SBD installation compared to the second calendar year after installation | Housebreaking crime | The installation of SBD-compliant doors and windows was associated with significant reductions in crime. Variation in the levels of crime reduction was observed across the four study sites, attributed to unmeasured variables such as implementation. |
| Ward (2017) | Fife | No | Matched comparison | 3,190 SBD properties and 3,055 non-SBD properties | Yes | Police | Jan 2007 to Dec 2016 | (Attempted) theft by housebreaking, Sneak in theft; Theft by opening Lock-fast Place (OLP) | All four crime categories were lower in the SBD sample compared to the control sample. 1.8% of all SBD properties experienced crime compared to 14.2% of non-SBD properties over the study period. |

Meta-analysis

Owing to the data reported in the primary studies, our main meta-analysis focussed on the impact of SBD on residential burglary as measured using police recorded crime data. For each effect we selected the data for the most reliable control group (when more than one was reported). That is, the control group with the greatest similarity to the treatment group was selected if multiple control groups were used.

We first estimated the effect of SBD by computing an odds ratio and confidence intervals for each individual study. The effect sizes were then assessed for heterogeneity, which generated a statistically significant Q-statistic of 30.30 (d.f. = 8, $p < 0.05$). Since Q-statistics are influenced by both the number of studies and the sample size within those studies, it is prudent to interpret this in the context of other statistics that estimate heterogeneity. We therefore also computed the I^2 statistic, which describes the percentage of variation across studies that is due to heterogeneity rather than chance. I^2 is a relative measure, but with a helpful rule of thumb to interpret (Higgins et al., 2003). Here we report both statistics in combination. In brief, they indicated that heterogeneity for the main sample of data was moderate, thus justifying subgroup analysis to explore whether the observed heterogeneity could be reduced (described below).

The results of our main meta-analysis are shown in Figure 2, which has a logged scale. It shows that following our reanalysis of the original data, eight of the nine included studies have individual effect sizes denoting a positive effect of SBD: properties that met SBD standards experienced less burglary than comparison non-SBD properties. However, the observed effect was statistically significant in only three of these studies (Armitage 2004, Brown, 1999; Ward, 2017), denoted by the confidence interval not crossing the vertical line at odds ratio 1. The data reported in Jones et al. (2016) was the only study to generate a (non-significant) effect that favoured the control group.

The overall weighted mean effect size is shown as a black diamond (with confidence intervals) at the bottom of Figure 2. Since the upper bound of the confidence interval does not cross the vertical line at odds ratio 1, the overall result is statistically significant (ES: 0.47, CI: 0.23-0.95). This result can thus be interpreted as, on average, burglary in SBD properties was 53% lower compared to non-SBD properties.

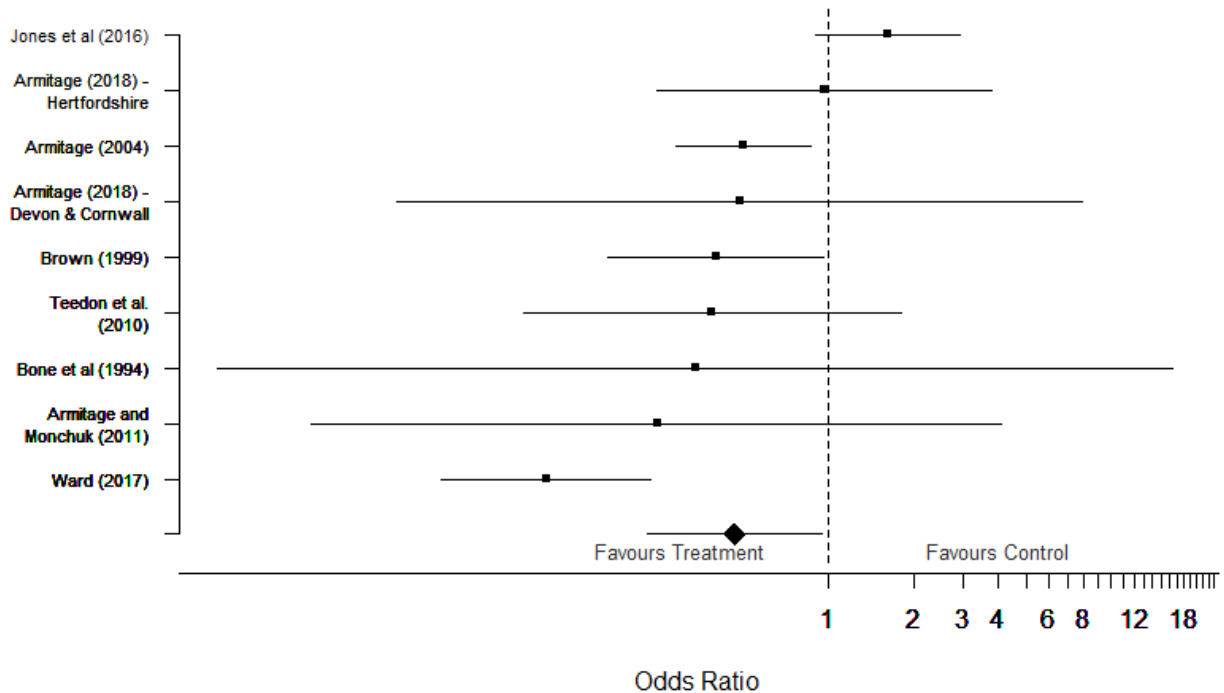


Figure 2. Forest plot of the effect sizes from studies which used police recorded data to measure the effect of SBD on burglary (n = 9)

As described above, computed Q and I^2 statistics indicated notable heterogeneity within the sample of studies included in our meta-analysis. This means that the studies are dissimilar, which is not optimal for statistical aggregation. Consequently, subgroup analyses, using the same procedure as that reported above (random effects model), were undertaken to explore the results when meta-analysing comparable subgroups of included studies. To reiterate a point made previously (see page 9), meta-analysis was only performed here when there were three or more comparable studies. For this reason, our sub-group meta-analysis did *not* examine the effect of SBD on existing properties refurbished to SBD standard (n = 2).

In addition to the overall meta-analysis (shown in Figure 2), we meta-analysed six further subgroups of studies, namely studies that: exclusively had a post-intervention study design with matched treatment and control groups (#2); used police-recorded *attempted* burglary data as the outcome (#3); used a matched pairs design, combining burglary outcomes from police-recorded crime data and victimisation survey data (#4), only included new-build properties (#5); used 'all crime' as an outcome measure (#6) and combined outcomes for vehicle crime from police-recorded data and victimisation survey data (#7).

The results of our subgroup meta-analyses are presented in Table 2. A key finding concerns the impact on burglary in newly built SBD properties (#5). Focussing only on this subset of studies, the overall weighted mean effect size is 0.35 (CI: 0.18-0.69). This indicates that, on average, burglary in newly built SBD properties was 65% lower compared to burglary levels in matched non-SBD properties. Other mean effect sizes that were statistically significant came from samples using only attempted burglary as the outcome measure (mean ES: 0.23, CI: 0.10-0.54; #3), matched-pair designs that used police-recorded and victimisation surveys to measure burglary outcomes (mean ES: 0.35, CI: 0.19-0.65; #4) and matched-pair designs that used only police-recorded burglary (mean ES: 0.5, CI: 0.18-0.69). Non-significant effects were found for all crime outcomes (#6) and vehicle crime outcomes (#7).

Table 2. Results of sub-group meta-analysis

| # | Research design | Data type | Crime type | N | Q stat | I ² interpretation | Mean ES | CI 95% | % relative reduction | Significant? |
|---|--|-------------------------------|-------------------------|---|--------|-------------------------------|---------|-----------|----------------------|--------------|
| 1 | All | Police | Burglary | 9 | 30.30 | Substantial heterogeneity | 0.47 | 0.23-0.95 | 53% | Y |
| 2 | Post-only | Police | Burglary | 7 | 12.16 | Moderate heterogeneity | 0.35 | 0.20-0.62 | 65% | Y |
| 3 | All | Police | Attempted burglary only | 3 | 3.37 | Low-moderate heterogeneity | 0.23 | 0.10-0.54 | 77% | Y |
| 4 | Only matched pairs | Police & victimisation survey | Burglary | 7 | 12.15 | Moderate heterogeneity | 0.35 | 0.19-0.65 | 65% | Y |
| 5 | Only matched pairs of new build SBD and non-SBD properties | Police | Burglary | 6 | 12.15 | Moderate heterogeneity | 0.35 | 0.18-0.69 | 65% | Y |
| 6 | Only matched pairs | Police | All crime | 3 | 15.61 | Substantial heterogeneity | 0.50 | 0.19-1.33 | 50% | N |
| 7 | Only matched pairs | Police & victimisation survey | Vehicle crime | 4 | 1.21 | .iii | 0.48 | 0.16-1.41 | 52% | N |

Discussion

SBD is a UK-based police initiative that sets security standards for the built environment. This study reviewed the evidence on whether properties that meet SBD standards experienced lower levels of crime. Following a systematic search of the published and unpublished literature, and through consultation with SBD staff and practitioners, we identified nine primary studies that met our inclusion criteria. Synthesising data across these nine studies, we found that burglary in SBD properties was 53% lower than in comparison non-SBD properties, with no evidence of any statistically significant backfire effects. Similar crime prevention effects were observed when focussing only on new build properties, with burglary found to be 65% lower compared to matched non-SBD properties. No significant effects were found for other types of crime and disorder.

Our findings are consistent with the now large body of evidence demonstrating that improved household security is associated with lower levels of burglary (see Tseloni et al., 2017). They also lend further support for the basic idea that crime can effectively be reduced by removing opportunities to commit it. From a policy perspective, our findings, consistent with theoretical expectation, provide support for the use of SBD as a burglary reduction measure, and suggest that investing resources in upgrading and/or maintaining effective security standards across the built environment will likely reduce burglary.

Although the results of this review support the use of SBD, it is important to emphasise that the evidence presented here is considered promising, not conclusive, for several reasons. First, only a small number of studies met our inclusion criteria. This was particularly pronounced for existing properties refurbished to SBD standards ($n = 2$). Second, three of the nine eligible studies are now over 20 years old, and relate to housing and SBD standards which have been superseded, reflecting changes in policy and improvements in building materials and technology. Third, there were concerns about methodological quality among our sample of studies, such as failures to rule out threats to internal validity and uncertainty about the comparability of treatment and control sites. Fourth, the levels of crime among treatment and control groups in some studies was very low (for e.g., Bone, 1994; Armitage & Monchuk, 2011), thereby raising concerns about a lack of statistical power. And fifth, few studies reported efforts to determine the *quality* of the SBD being evaluated. This is a concern

given studies have found notable variation *within* the quality of SBD developments, likely owing to differences in the SBD assessment process and/or failure to maintain SBD standards post-accreditation (see Armitage & Tompson, 2023). The issue of quality assurance also has relevance to *non-SBD* properties and developments acting as control sites in evaluation studies. In the thirty five years since the inception of SBD, the quality of household security has generally improved, influenced by a combination of technological developments, policy mandates and, potentially, the growth and influence of SBD itself as a vehicle to drive up security standards. One consequence of these general improvements is that the difference in security levels between SBD and non-SBD developments is, on average, less pronounced today than it was thirty years ago. This raises concerns about the equivalence of SBD vs non-SBD evaluation sites; some properties might meet SBD standards despite not being SBD accredited, thereby muddying SBD vs non-SBD comparisons. To address this concern, future research could independently assess the quality of SBD (treatment) and non-SBD (control) sites to more accurately determine what is being compared.

In reviewing the literature on SBD, we identified several areas where future research might usefully be directed to advance the evidence base. The first knowledge gap concerns crime displacement and/or diffusion of crime control benefits (Guerette & Bowers, 2009). As pointed out earlier, while several studies alluded to displacement (particularly tactical) (Brown, 1999; Teedon et al., 2009; Jones et al., 2016), no studies empirically examined nor reported sufficient data for us to retrospectively assess this. Established methods are now available to detect the presence of displacement (Bowers, 2023), and so future research could helpfully determine whether the installation of SBD leads to any form of crime displacement or diffusion of crime control benefits, be that at the individual property level or across nearby housing developments.

The second knowledge gap relates to outcome measures beyond residential burglary. It is plausible that improvements in the design and security of housing developments could give rise to various positive outcomes, related both to crime (e.g. reductions in car crime or anti-social behaviour) and non-crime measures (e.g. reduced maintenance costs, improved social cohesion and so on). Evidence from individual studies partially support this claim. Armitage and Monchuk (2011), for example, found that SBD was associated with statistically significant reductions in assault and theft from vehicles. Crime data beyond residential burglary are

presently limited, however, and in the case of non-crime outcome measures, largely absent. In response, it is suggested that future evaluation studies assess a wider range of crime and non-crime outcome measures that might plausibly follow the introduction of SBD security standards. Evaluation attention might specifically focus on the effects of SBD on refurbished properties, where the evidence base is less advanced.

Finally, as is true of many systematic reviews in crime prevention (see Tompson et al., 2021), we found limited evidence relating to the mechanisms through which SBD initiatives might lead to crime reductions (e.g. increased effort through target hardening; increased risk through improved lighting, and so on), and the conditions in which these mechanisms are more or less likely to be activated. Future primary evaluation studies should therefore focus on collecting data on the proposed underlying mechanisms better to understand what it is about SBD that affects crime and related outcomes, and under what conditions SBD works best.

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*Denotes those studies included in the meta-analysis (n = 9).

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Declarations concerning conflicts of interest

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Annex A: Keyword search terms for electronic databases

We used the following terms when searching the electronic databases. Where necessary, search terms were adapted to suit the requirements of particular databases:

“Secured by Design”, “Secure by Design”, “SBD” and “Police Label Secure Housing”.

Searches were conducted on 11 August 2016 and updated on 12 January 2021.

Annex B: Electronic and grey literature databases searched in this review

1. ASSIA (Applied Social Sciences Index and Abstracts)
2. Criminal Justice Abstracts
3. IBSS (International Bibliography of Social Sciences)
4. NCJRS (National Criminal Justice Reference Service)
5. ProQuest Criminal Justice Database
6. ProQuest theses and dissertations (UK)
7. PsycINFO
8. Social Policy and Practice

Annex C: Other sources searched in this review

In collaboration with an information specialist (Phyllis Schultze, Rutgers University) we searched the publications of the following government, research and professional agencies:

1. Center for Problem-Oriented Policing (Tilley and Goldstein Award entries)
2. Institute for Law and Justice
3. Vera Institute for Justice (crime and victimization publications)
4. Rand Corporation (public safety publications)
5. Police Foundation
6. Police Executive Research Forum
7. The Campbell Collaboration reviews and protocols
8. Urban Institute

9. European Crime Prevention Network
10. Swedish National Council for Crime Prevention
11. UK Home Office
12. UK College of Policing (Polka)
13. Australian Institute of Criminology
14. Swedish Police Service
15. Norwegian Ministry of Justice
16. Canadian Police College
17. Finnish Police (Polsi)
18. Danish National Police (Politi)
19. The Netherlands Police (Politie)
20. New Zealand Police
21. US National Institute of Justice

We also searched

1. Google
2. Google Scholar
3. Academic Search Premier (EBSC)
4. ProQuest Sociology
5. Rutgers Criminal Justice Grey Literature Database
6. OSCE Polis Digital Library
7. WorldCat

Annex D: The formulae used in the meta-analysis

Odds ratios were computed as follows:

$$OR = \frac{\text{Action households victimised} \times \text{Control households not victimised}}{\text{Action households not victimised} \times \text{Control households victimised}}$$

For computational reasons, the standard error (SE) is calculated for the logarithm of the OR (LOR = log(OR)) rather than the raw OR. The formula for computing the SE for LOR is as follows:

$$SE_{LOR} = \sqrt{\frac{1}{A_{hhlds\ victimised}} + \frac{1}{C_{hhlds\ not\ victimised}} + \frac{1}{A_{hhlds\ not\ victimised}} + \frac{1}{C_{hhlds\ victimised}}} \times 2$$

As noted by others (see, for example, Weisburd et al., 2008), studies that employ *places* as the unit of analysis may not generate a Poisson distribution of values, as is the assumption in standard meta-analysis. For this reason an inflation factor of 2 is used when calculating the standard error of the log odds (as seen in the equation above), to make the confidence interval estimates more conservative.

Confidence intervals are then computed in the usual way (by multiplying the SE_{LOR} 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, as logged values can be difficult to understand.

To estimate the mean effect size we first computed the inverse variance weights for the OR for each study using the formula:

$$\omega_i = \frac{1}{SE^2}$$

Where,

ω_i is the inverse variance weight, and
 SE_i is the standard error of the estimate

The weighted mean effect size is then:

$$\overline{OR} = \frac{\sum(\omega_i \times LOR_i)}{\sum \omega_i}$$

As is now standard practice in meta-analysis (see Deeks et al, 2023), we exclusively used random-effects meta-analytic models in our analysis. This is because the random-effects model will give identical results to the fixed-effects model when there is no heterogeneity among the included studies. However, our sample of studies indicated there was noteworthy heterogeneity so random-effects models were a prudent choice.

Annex E: Assessment of risk of bias

| Study | Comparison group ^a | Randomisation ^b | Equivalence at baseline ^c | Assessment of SBD quality ^d | Implementation concerns ^e |
|---------------------------|-------------------------------|----------------------------|--------------------------------------|--|--------------------------------------|
| Armitage (2004) | Y | N | Y | Y | Y |
| Armitage (2018a) | Y | N | Y | Y | Y |
| Armitage (2018b) | Y | N | Y | Y | Y |
| Armitage & Monchuk (2011) | Y | N | N | N | N |
| Bone et al. (1994) | Y | N | N | N | Y |
| Brown (1999) | Y | N | Y | N | Y |
| Jones et al. (2016) | Y | N | N | N | Y |
| Teedon et al. (2010) | Y | N | Y | N | Y |
| Ward (2017) | Y | N | Y | N | N |

^a Is there a comparison group? (Y/N)

^b If so, was randomisation used to allocate units of analysis to treatment and control groups? (Y/N)

^c If so, is there evidence that the treatment & control groups were comparable at baseline? (Y/N)

^d Is there evidence that the quality of SBD was assessed as part of this study? (Y/ N)

^e Were there any noted implementation issues that may have affected the results? (Y / N)

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ENDNOTES

ⁱ Various known as Architectural Liaison Officers (ALOs) or Crime Prevention Design Advisors (CPDAs).

ⁱⁱ Readers should note that Armitage (2004) reported several evaluations of SBD using different methods and data sets. For the purposes of this review, each separate evaluation was appraised by the reviews authors and that which was considered to be the strongest methodologically was included in our meta-analysis. Moreover, whilst for reasons of completeness we focused in this review of Armitage (2004) – a PhD thesis – it should also be noted that the results from this study also featured in a subsequent peer-reviewed publication (Armitage, 2006).

ⁱⁱⁱ This cell is blank (zero) because the Q statistic is smaller than its degrees of freedom (See Huedo-Medina et al. 2006).