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Preventing Repeat Victimization: A Systematic Review

brå
National Council for Crime Prevention

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Report prepared for
The Swedish National Council for
Crime Prevention

Brå – a centre of knowledge on crime and measures to combat crime
The Swedish National Council for Crime Prevention (Brottsförebyggande rådet – Brå) works to reduce crime and improve levels of safety in siciety by producing data and disseminating knowledge on crime and crime prevention work and the justice system's responses to crime.

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Foreword

A large proportion of all crimes are committed against crime victims who have been victimized before, a phenomenon known as repeat victimization. There is thus a potential to achieve substantial benefits by focusing crime prevention measures on individuals, institutions or objects that have previously been exposed to crime. Successful strategies of this kind would prevent repeat victimization, and thus also would prevent a substantial proportion of all the crimes committed. The crime prevention measures that are implemented to this end may take several different forms. The strategy is not primarily about specific kinds of measures, but rather involves a way of directing crime prevention measures at relevant targets. An increasing number of crime prevention initiatives have been directed at repeat victimization especially to prevent repeat burglaries. But how well do they work? What does the research tell us?

There are never sufficient resources to conduct rigorous evaluations of all the crime prevention initiatives employed in an individual country such as Sweden. For this reason, the Swedish National Council for Crime Prevention (Brå) has commissioned distinguished researchers to conduct a series of international reviews of the research published across a range of fields.

This report presents a systematic review, including a statistical meta-analysis, of the effects of initiatives to prevent repeat victimization. The work has been conducted by Lecturer Louise E. Grove of Loughborough University (UK), Senior Research Fellow Graham Farrell of Simon Fraser University (Canada), Professor David P. Farrington of Cambridge University (UK), and Professor Shane D. Johnson of University College London (UK).

The study follows the rigorous methodological requirements of a systematic review. The analysis combines the results from a number of evaluations that are considered to satisfy a list of empirical criteria for measuring effects as reliably as possible. The meta-analysis then uses the results from these previous evaluations to calculate and

produce an overview of the effects associated with initiatives to prevent repeat victimization.

The systematic review and the statistical meta-analysis presented in this report are based on a substantial number of empirical evaluations. Even though important questions remain unanswered, the study provides an accessible and far-reaching overview of the effects of initiatives to prevent repeat victimization. Generally, the results are encouraging; suggesting that appropriately targeted situational prevention measures can significantly reduce repeat burglaries.

Stockholm in June 2012

Erik Wennerström
Director General

Executive Summary

In any given year, most crimes occur against targets that have already been victimized. The crime prevention strategy deriving from this knowledge is that *targeting repeat victimization provides a means of allocating crime prevention resources in an efficient and informed manner*. This report presents the findings of a systematic review of 31 studies that evaluate efforts to prevent repeat victimization. Most of the evaluations focus on preventing residential burglary, but commercial burglary, domestic violence, and sexual victimization are also covered.

The main conclusion is that the evidence shows that repeat victimization can be prevented and crime can be reduced. Over all the evaluations, crimes decreased by one-sixth in the prevention condition compared with the control condition. The decreases were greatest (up to one-fifth) for programmes that were designed to prevent repeat burglaries (residential and commercial). There were fewer evaluations of programmes designed to prevent repeat sexual victimization, but these did not seem to be effective in general.

There are indications about what factors increase the success of prevention efforts. Appropriately tailored and implemented situational crime prevention measures, such as target hardening and neighbourhood watch, appear to be the most effective. Advice to victims, and education of victims, are less effective. They are often not prevention measures themselves and do not necessarily lead to the adoption of such measures.

The effectiveness of these crime prevention measures increased as the degree of implementation increased. There were many problems of implementation, including poor tailoring of interventions to crime problems, difficulty of recruiting, training and retaining staff, breakdown in communications, data problems, and resistance to tactics by potential recipients or implementers.

The main conclusions of this report are that:

- A systematic review of the evidence suggests that repeat victimization can be prevented and overall crime thereby reduced.
- The impact on crime varies with the effectiveness of prevention tactics and their implementation.
- Appropriately-tailored situational crime prevention tactics appear to be most effective.
- Advice and education for victims are often not effective.
- The effectiveness of programmes depends on the effectiveness of their implementation.
- The success to date suggests that there is an urgent need for further research into the prevention of repeat victimization for different crime types, and into how to overcome implementation problems.
- Key other areas for future prevention efforts may be a focus upon the most victimized supertargets, upon across-crime-type repeats, and upon near repeats (similar crimes, often committed nearby, soon after, against similar targets).

1. Background

This report reports a systematic review of efforts to prevent repeat victimization. The repeated criminal victimization of persons, places, and other targets, however defined, accounts for most crime, and the topic is an increasingly prominent area for criminological research. A recent annotated bibliography summarized over 140 selected studies. It included studies showing that similar patterns of repeats have been found in most places where reliable data are available, including Australia, Canada, Denmark, France, Germany, Hungary, Japan, the Netherlands, New Zealand, Malawi, Poland, Spain, Sweden, the United Kingdom and the United States (Grove and Farrell 2011). Likewise, while repeats appear to be even more prevalent for personal than property crimes, they occur in all crime types adequately studied (except murder). These range from street crimes, including burglary, theft, assault, robbery, threats, vandalism and car crime through to obscene phone calls, sexual victimization, domestic violence, elder abuse, child abuse, fraud, commercial crimes, computer attacks, and terrorist attacks.

The evaluated prevention efforts reviewed herein were informed by a range of additional research. Laycock (2001) provided an excellent summary of the 'story' of repeat victimization research, detailing its incremental progress and the close relationship between research, policy, and prevention practice.

Two main explanations for why repeats occur have been proposed: State heterogeneity or *flag*, and event dependence or *boost*. Some targets appear or *flag* themselves as more attractive and so are victimized by different offenders. For example, some households offer visual cues that they may be easier or more rewarding targets. However, upon committing a crime, offenders learn which targets are best and this *boosts* the likelihood that they will repeat it. Of course these two mechanisms are linked because more attractive targets are more likely to induce repeat crimes by the same as well as

different offenders. That is, a *flagged* offence must occur before a *boosted* offence is possible.

The evidence, including surveys of victims and interviews with offenders, suggests that the boost explanation accounts for the majority of repeat victimizations for many crime types (Chenery et al. 1996; Ashton et al. 1998; Everson 2003; Tseloni and Pease 2003; Bowers and Johnson 2004). By now this is perhaps self-evident for crimes such as domestic violence, elder abuse, and child abuse, but it also holds true for other crime types. The fact that repeats tend to occur quickly, clustering rather than being randomly distributed in time, is strong indirect evidence that the same offenders return sooner rather than other offenders returning later.

This was first demonstrated for residential break-and-enter crimes in Saskatoon, Canada (Polvi et al. 1990, 1991) and it has been replicated many times elsewhere for burglary and other crime types (including by Sampson and Phillips 1992; Tilley 1993a, 1993b; Lloyd et al. 1994; Johnson, Bowers and Hirschfield 1997; Robinson 1998; Kleemans 2001; Budz, Pegnall and Townsley 2001; Moitra and Konda 2004; Daigle, Fisher and Cullen 2008). It is likely that offenders learn the risks and likely rewards. More generally, success breeds repeats. This means that bank robbers are more likely to return to the same branch if they get away with a lot of money (Matthews, Pease and Pease 2001). However, it has also been suggested that, where repeat property crime is less immediate, this may be because offenders wait for goods to be replaced by insurance payment, a delayed boost account (Clarke, Perkins and Smith 2001).

The likelihood that a repeat crime occurs increases with each subsequent victimization (Ellingworth et al. 1995, Farrell and Pease 2003). Even among targets, risk is very unevenly distributed. One classic study found that just 1% of people experienced 59% of personal crimes including violence, while 2% of people experienced 41% of property crimes (Pease 1998). This suggests that around one in eight targets appears to be what has been termed a *supertarget* (Farrell et al. 2005), here defined as a target that experiences five or more crimes per year. This is important because it means that there are greater efficiencies if prevention is focussed on the most frequently victimized targets. This has been operationalized as a graded response whereby the more victimized targets receive more prevention resources (Chenery et al. 1997; Hanmer et al. 1999; Weisel et al. 1999). Likewise, because repeat crimes are less likely to be reported to the police, it has been suggested that prevention efforts will benefit if the police gather information from victims about their previous crime experiences (Rogerson 2008).

Repeat victimization can involve multiple crime types based on the same target. Some schools, for example, are frequent targets of vandalism as well as break-ins (Lindstrom 1997). Risky targets, whether

types of facilities or other places, lifestyles, vehicles or professions, are reflective of the vulnerability to criminal victimization of particular groups of targets. Nurses, fire-fighters, police officers and those in other service or caring professions have a higher likelihood of becoming victims than other professional occupations, and within those professions certain individuals are much more frequently victimized than others (Clare, Kingsley and Morgan 2009). Lifestyle plays a role in repeat victimization (Hindelang, Gottfredson and Garafalo 1978). A person who goes out often to bars and clubs has a greater risk of experiencing theft, robbery or assault by strangers than a person who stays at home. Their unguarded home may experience a burglary during their absence. Offenders also may become victims, for example when drug dealers and customers rob each other because they have money and drugs and are unlikely to call the police.

Recent developments in repeat victimization research include the identification of high risk targets which share similar characteristics to prior victims. Following a successful burglary, a neighbouring household may be targeted in anticipation of similar success (Townsley, Homel and Chaseling 2003; Bowers and Johnson 2004; Bernasco 2008; Short et al. 2009). This is known as near repeat victimization or near repeats. The concept of 'nearness' can apply to similar targets such as the same make and model of car or mobile phone encountered in similar circumstances. In addition, hot spots of crime, that is, spatial concentrations of crime, are often caused by repeat victimization (Levy and Tarturo 2010). The result is that the study of repeats is beginning to merge with other areas of crime concentration. The key issue is the similarity of crimes. Very similar crimes afford greater potential for prediction and therefore prevention than those that are dissimilar.

In short, a range of research suggests the importance of repeat victimization for crime prevention is that it provides useful information about where and when to go, and what to do, to prevent crimes. This is because crimes tend to occur against the same or similar targets, and because, if we know how the crime occurred previously, then we can also know how to go about preventing its recurrence. Hence, the essence of this theory underpinning the efforts reviewed herein is that *targeting repeat victimization provides a means of allocating crime prevention resources in an efficient and informed manner*.

2. Methodology

This systematic review builds on those of Farrell (2005) and Farrell and Pease (2006) which focussed on repeat residential burglary, and those of Grove (2010, 2011). The crime types included here are those for which suitable evaluations were identified: residential burglary; domestic violence; commercial crime; and sexual victimization. Second responder efforts to prevent repeat family violence, which was covered by Davis, Weisburd and Taylor (2008), are not included here.

Evaluation studies were selected from those identified through systematic searches of databases, hand searches of bibliographies, and contact with other academics and practitioners working on repeat victimization. Efforts were made to include both published and unpublished studies. The databases and websites searched are listed in Table 1. The searches were completed in February 2010.

Table 1. List of Databases and Key Websites Searched.

- ASSIA: Applied Social Sciences Index and Abstracts (1987 – 2009);
- Criminal Justice Abstracts (1968 – 2009);
- National Criminal Justice Reference Service Abstracts (1975 – 2009);
- PsycARTICLES (1894 – 2009);
- PsycINFO (1806 – 2009);
- Social Services Abstracts (1979 – 2009);
- Sociological Abstracts (1952 – 2009);
- Worldwide Political Science Abstracts (1975 – 2009);
- UK Home Office; Australian Attorney General's Office;
- EThOS (Electronic Theses Online Service);
- Crime Prevention Register on the Australian Institute for Criminology's website;
- Situational Crime Prevention Evaluation Database provided by the Center for Problem Oriented Policing.

Key search terms and combinations thereof were used to identify studies within each database as follows:

(repeat** victim*******) or (multi*** victim*******) or (recidivist victim) or (repeat** burglary) or (repeat** sexual**) or (repeat** racial**) or (poly victim*******) or (repeat** target**) or (prior target**) or (multi*** target**) or (recur**** target**) or (recur**** victim*******) or (multi*** burglary) or (multi*** sexual**) or (multi*** racial**)
In order for a study to be suitable for inclusion, all three of the following characteristics had to be met:

1. Data had to be available for a period prior to the start of the intervention, as well as a comparable period either throughout or immediately after the duration of the intervention.
2. A comparison group was required, though there were no significant restrictions on how that group was defined. Pragmatic considerations meant that comparison groups comprising the rest of area were permitted, following Farrington and Welsh (2006), who found that such comparisons were generally valid.
3. A focus on repeat victimization on an individual level rather than a hot spot/area basis had to form a significant part of the study.

The most common reasons for exclusion of evaluations were: no available comparison group; no pre-post data; there was a 'hot spot' area-based approach rather than the targeting of individually identified repeat victims; or there was a paucity of information. It should be noted that all evaluations with comparison groups were included where other criteria were met, despite variation in the comparability of conditions. Perhaps this could be interpreted as a generous interpretation of the experimental requirements for a systematic review, but few studies could otherwise have been included. The number of studies identified at each stage of searching is shown as Table 2.

Table 2. Number of Studies Identified at Each Searching Stage

Number of Studies	Searching Stage
3001	Unique findings using keywords
955	Relevant to crime prevention (many were medical)
57	With a significant evaluative component
31	Included in the systematic review

Systematic coding manuals were developed following the format suggested in Lipsey and Wilson (2001). The use of a coding manual ensured that the same comprehensive information was gathered

from each study within a crime type. Monitoring of coding reliability was achieved by recoding a sample group of studies at a later stage to check that the same coding outcome was recorded. The characteristics that were coded varied between crime types, and this was a necessary adaptation to allow for the distinct differences in approaches to the different crime types. However, consistency was maintained wherever possible.

Secondary coding was conducted following the scientific realist approach, and this phase of data extraction utilized an individual approach to each study. This involved both annotation of studies and separate note-taking. At this secondary stage, useful information was gleaned from across the full range of identified evaluations, including information on implementation difficulties and study contexts. The aim here was to retain useful information, notably theory or valuable analyses of the subject, that might otherwise be lost. Implementation issues in particular are discussed later in this report.

In order to allow evaluations to be compared, an effect size was calculated for each one. Effect sizes are a way of standardizing and directly comparing effects across studies and outcomes (Gottfredson et al. 2002). A key advantage of the effect size is that

> *"It allows us to move beyond the simplistic, 'Does it work or not?' to the far more sophisticated, 'How well does it work in a range of contexts?' Moreover, by placing the emphasis on the most important aspect of an intervention – the size of the effect – rather than its statistical significance (which conflates effect size and sample size), it promotes a more scientific approach to the accumulation of knowledge." (Coe, 2002: 1)*

The effect size used here is the Odds Ratio (OR). This is "an effect size statistic that compares two groups in terms of the relative odds of a status or event" (Lipsey and Wilson 2001: 52). It has been used in a range of place-based crime prevention evaluations (Bowers et al. 2009) and in a systematic review of CCTV effectiveness (Welsh and Farrington 2009). To consolidate findings from the odds ratio for individual programmes, a weighted mean effect size was calculated using the random effects model which is explained further below.

The following formula is used to calculate the Odds Ratio:
OR = (a*d) / (b*c)
where * indicates multiplication
and a, b, c and d are the numbers of crimes, which are derived from the following:

	Before	After
Intervention	a	b
Comparison	c	d

The OR is intuitively meaningful because it indicates the relative change in crimes in the control area compared with the intervention area. For example, OR = 2 indicates that d/c (control after/control before) is twice as great as b/a (intervention after/intervention before). This value could be obtained, for example, if crimes doubled in the control area and stayed constant in the intervention area, or if crimes decreased by half in the intervention area and stayed constant in the control area, or in numerous other ways.

The variance of OR is calculated from the variance of LOR (the natural logarithm of OR). The usual calculation of this is as follows:

VAR (LOR) = 1/a + 1/b + 1/c + 1/d

In this review, we use LOR, the natural logarithm of OR, and refer to VAR(LOR). This calculation of VAR(LOR) is based on the assumption that crimes occur at random, according to a Poisson process. This assumption is plausible because 30 years of mathematical models of criminal careers have been dominated by the assumption that crimes can be accurately modelled by a Poisson process (see e.g. Barnett, Blumstein and Farrington 1987). In a Poisson process, the variance of the number of crimes is the same as the number of crimes. However, the large number of changing extraneous factors that influence the number of crimes may cause overdispersion; that is, where the variance of the number of crimes (VAR) exceeds the number of crimes (N). The overdispersion factor (D) is expressed as:

D = VAR/N.

Where there is overdispersion, VAR(LOR) should be multiplied by the overdispersion factor, D. Farrington et al. (2007) in a CCTV meta-analysis, estimated VAR from monthly numbers of crimes and found the following equation:

D = .0008 * N + 1.2

D increased linearly with N and was correlated .77 with N. The mean number of crimes in an area in the CCTV studies was about 760, suggesting that the mean value of D was about 2. However, this is an overestimate because the monthly variance is inflated by seasonal variations, which do not apply to N and VAR. Nevertheless, in order to obtain a conservative estimate of the variance, VAR(LOR) calculated from the usual formula was multiplied by 2 in all cases in this report.

3. Findings

A range of efforts to prevent repeat victimization have been evaluated but most have focused on burglary. Interventions for residential burglary and commercial burglary often included an initial security survey followed by securitization of properties. This typically involved improving locks on vulnerable doors and windows, but also other techniques such as reinforcing doors. Alarms were occasionally given or loaned to victims, including repeat victims of domestic violence. Property marking for burglary victims was often facilitated by the provision of either a microdot solution (which can be uniquely identified) or access to a property register, usually with decals (stickers) to promote deterrence. Neighbourhood Watch, or the smaller Cocoon Watch among nearby neighbours (Forrester, Chatterton and Pease 1988), was established within some repeat burglary or domestic violence projects. Less common measures included offender-focused interventions, blocking off access to rear alleys used by burglars, and media publicity to promote deterrence.

Interventions for commercial burglary were similar to those for residential burglary, although other measures included CCTV and motion sensors. The sexual victimization prevention programmes identified within this report centred predominantly on the education of victims, with practical advice given in small group settings. The sole domestic violence prevention intervention included within this report featured a tiered response of personal safety plans, police patrols and monitored alarms, based on the Killingbeck model of Hanmer et al. (1999).[1]

Key details of the features of the 31 included studies are given in Table 3. This provides the name by which the study is known here (often this is its location), the authors' names and the dates of the relevant publications or reports. The size of the intervention group is also given. For residential burglary projects this is typically the number of households in the area in which the programme took place. The nature of the comparison or control group and any differences between it and the intervention group are detailed along with information on the prevention measures, their implementation, and details of any evidence relating to whether crime was displaced or whether there was a diffusion of prevention benefits beyond the intervention group. Rather than include an extended narrative review here, the reader wishing to obtain detailed information is invited to scrutinize Table 3.

[1] The Killingbeck domestic violence project (Hanmer et al. 1999) was excluded from the meta-analysis because the evaluation component did not have a comparison group. However, it is an example of a study included in a narrative review.

Table 3: Key Features of the 31 Evaluations Included

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Kirkholt Burglary Prevention Project (Forrester et al. 1988, 1990)	Area of 2280 households	Remainder of police subdivision (Larger area; Some privately-owned houses; Lower burglary rate)	Victim-oriented: Free focused security upgrades; coin-box removal; cocoon neighbourhood watch. Offender-oriented: debt counselling; arrests. (Free – paid by project)	68% for security upgrading; close to 100% for Cocoon Watch.	Displacement examined - none found.
St Ann's (Gregson 1992)	Area of 8000 households	Remainder of police subdivision, excluding rural areas (Larger area) (p.49)	Target hardening security measures (5 lever door locks, mortise bolts, window locks and door panels – to tackle a weak spot on council house doors) provided free to "burgled council tenants" (p.20), also to housing association and private tenants where resources allowed for this.	Cocoon Watch achieved 25% coverage (p.7). Scheme suspended whilst replacement staff found.	Displacement not measured / "The project was not cited in a very high crime area" (Tilley 1993, p.6)
The Meadows (Gregson and Hocking 1993)	Area of 3936 households	Remainder of police subdivision (Larger area)	Prior victims and vulnerable households received a visit from a carpenter who installed target hardening measures (free to recipients) including as appropriate: locks to doors and windows, bolts, door chains, viewers, and strengthening of doors with plywood. Property marking.	55% of victims (187 households) received target hardening and 424 households overall. Vulnerable groups also given package. 'Lack of interest' shown by police (p.9).	Displacement not measured.
Eyres Monsell (Matthews and Trickey 1994a)	Area of 4,100 households	Larger area of three "comparable estates" (p. 2).	Target-hardening security measures (lock fitting - free); neighbourhood watch; information for residents.	415 security packages fitted – only 71 were to victims. "Delays in the fitting of locks" (p. 26). Patchy implementation and notification to eligible victims. (p.26). Neighbourhood Watch seen as powerless.	Burglary rate "considerably lower" than on national "high risk estates" (p. 2-3) "Little evidence of displacement" (p.57) within the estate. "Evidence of displacement" (p. 57) to some surrounding estates. (p. 31)

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
New Parks (Matthews and Trickey 1994b)	Area of 3,500 households	Mowmacre "comparable" estate. (p. 2)	Target-hardening security measures (lock fitting - free); neighbourhood watch; information for residents.	298 security packages fitted – only 60 were to victims (p.27). "Changes in personnel" (p. 59) affected the programme's co-ordination. Also "organisational problems" (p. 57) establishing Neighbourhood Watch schemes. Take up of free lock fitting "relatively low" (p. 20)	Burglary rate "considerably lower" than on national "high risk estates" (p. 2-3). "Little evidence of displacement" (p.57) within the estate. "Evidence of displacement" (p. 57) to some surrounding estates.
Blackburn - HomeSafe (Webb 1996)	Area of 2,287 households	Mill Hill "neighbouring Incident Location"	Target-hardening security measures (door and window locks, chains, bolts and viewers) offered free to victims and "vulnerable" (p.53). Crime prevention packs for residents. Publicity.	47% of victimized households were 'Homesafed'. Part of area had initial low crime rate.	"Increase in other types of property crime in the area". (p.50)
Burnley - HomeSafe (Webb 1996)	Area of 2,088 households	Larger area of 3 "comparison areas" (p.54).	Target-hardening security measures (door and window locks, chains, bolts and viewers) offered free (p.53) to victims of burglaries and attempts, plus one "hotspot" (p.9). Those reburgled after the target hardening offered further security (alarm, solid wood door). Publicity.	79% of current year victimized properties were 'Homesafed'. (p.15). Other initiatives were run in the area at the same time.	Overall acquisitive crime decreased in Burnley Wood, though some types did increase slightly (non-domestic burglary, theft from motor vehicles)
Lambeth - HomeSafe (Webb 1996)	Area of 1,240 households	Combination of 2 "comparison areas" (p.61).	Security offered to victims and "vulnerable" (p.60).	Unknown implementation rate: only 70 properties were 'Homesafed' due to sever implementation problems. Operation Bumblebee also ran in this area during the summer months. Author concludes changes in crime not due to intervention.	The scheme was halted twice due to "severe implementation problems" (p.60). Displacement not discussed.

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Merthyr Tydfil - HomeSafe (Webb 1996)	Area of 1,614 households	Gellideg, a "similar estate" (p.70). Suffered from a known "prolific offender" on the estate. (p.70)	Target hardening offered to victims of burglary and attempts, as well as other vulnerable groups. "Passive Infra Red Lights, stand alone alarms and dummy alarm boxes" were supplied to the "especially vulnerable" (p.13). Crime prevention packs. Publicity. (p.69)	51% of current year victims were 'Homesafed'(p. 15). 455 target hardening upgrades were given to vulnerable people. Other initiatives were run in the area at the same time.	Other acquisitive crime fell on the estate, with the exception of non-domestic burglary which rose by 9%.
Huddersfield 'Biting Back' (Anderson et al. 1995; Chenery et al. 1997)	Huddersfield police subdivision with 22,000 population	Remainder of West Yorkshire police force area. Contiguous areas used to examine displacement. (Larger area)	Graded Response system: Bronze, Silver, Gold according to risk, each with multiple tactics including letters to offenders, security, patrols, loan of alarms. (Mixed: Some free, some partially-sponsored security measures).	Interviews with victims "suggest implementation a factor in any continuing repeats" (1997; p. 17)	Displacement examined - none found.
Cambridge (Bennett and Durie 1999)	Castle - 2665 households; Arbury – 3024 households; One hot spot.	Similar non-acjacent local areas and hot spots plus some computer generated intervention and comparison groups.	Combined package of victim-oriented security, guardianship measures and offender-based measures (p. 19). (Key security measures depended on means-tested eligibility or purchase by victims).	Very low for key tactics: 3.5% (6 of 171 victims) received free Keepsafe door locks; "Some" victims acted on security advice; 9% of victims (n=15) received loan-alarms; 0% of visited victims required alley gates (p.36).	"The right medicine but the wrong dosage" (p.41). Implementation failure.
Baltimore - Hot Dots in Hot Spots (Weisel et al. 1999)	Three patrol sectors (p.25)	Patrol sectors matched on population, area, environment, housing stock, socio-economic status (p.25).	Warning cards of security advice to victims; Alert cards and warnings for neighbours; Security checks; Free property registration; Police patrols (p.27). All free but no funding for actual security.	Few process measures given: police 'distributed' cards and 'alerted' neighbours (p.27). Problems with address coding. Officers did not keep records.	Weak intervention (advice) suggests theory failure produced implementation failure (no strong preventive tactics introduced).

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Dallas - Hot Dots in Hot Spots (Weisel et al. 1999)	Part of Northeast police district - 12 square miles, 54,652 population (p. 33)	Comparison area matched on burglary rate; larger area (28 square miles) but similar size population of 45,520.	Written notification to generate victim awareness; Apartment managers notified of increased risks; home security surveys (p.35) All free but note no funding for security.	Victim survey showed: 87% implemented some crime prevention strategy ; 13% alarms; 27% moved or moving; 9% boarded windows; 18% changed or added locks (p.107). Apartment managers resistant to measures. Problems with address coding.	Weak intervention (advice) suggests theory failure combined with implementation failure (no strong preventive tactics introduced).
San Diego -Hot Dots in Hot Spots (Weisel et al. 1999)	Western Division: 26 sq, miles, 173,835 population.	Mid-City Division with "similar number of burglaries and housing stock." (p. 39)	Emphasis on better investigations; Home security checks; security brochure for victims; (p.40-41). Free but no funding for security.	Few process measures available; Changes in police personnel "challenge to implementation" (p.43); Police "skeptical" about repeat burglaries (p.43). Delays to intervention due to problems with police reporting. Address coding problems.	Weak intervention (advice) suggests theory failure produced with implementation failure (no strong preventive tactics introduced).
Beenleigh - Lightning Strikes Twice (Budz et al. 2001)	Area of 41,000 population	Non-neighbouring area matched on burglary rate and socio-demographic characteristics (p. 12).	3-tiered responses: Stop Break Response to one-time victims (security advice and materials); Hot Dot Response to two-time victims (more extensive prevention materials); Hot Spot Response to hot spot areas (home-security assessments; property marking).	Victims more likely than controls to use warning stickers (45% v 11%), property marking (42% v 12%), inventory lists (34% v 13%), and lock fitting (39% v 27%); More expensive measures (alarms; new doors or screens) more likely to be adopted than controls but still unlikely overall. (p.22). Police did not distribute advice cards and did not see the benefit. No formal training for new staff.	Displacement measured - none found. / Evaluation difficult because: "It was difficult to distinguish possible project effects from... random or seasonal fluctuations." (p. 14) and; Project was in an area with "low incidence of repeat victimization" (p. 14)

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Ashfield: Safer Towns and Cities (Taplin and Flaherty 2001)	Population of 74,604	Comparison area of Campsie, population of 90,375.	Victims received security assessments, followed by a "victim support package" consisting of property identification stickers, property register and crime prevention pamphlet (p.7). "Immediate neighbours" were informed (p.3). Repeat victims had their properties target hardened. Increase attendance of the fingerprint team. "Public education campaign about housebreaking reduction strategies" consisting of 27,000 mailouts (p.10).	70.5% of eligible victims received a security assessment. (p.48) 46.5% of victims experiencing 2 or more burglaries were known to have made security improvements (p.55). Other projects took priority. Target hardening perceived as expensive and poorly executed.	The author states that "there has been no displacement from break and enter to other property crime" (p.20)
Norwood and Tea Tree Gully, Adelaide (Henderson 2002)	Tee Tree Gully plus 3 police subdivisions: total population 207,000 (p.7)	Similar non-neighbouring comparison areas; similar neighbouring areas to measure displacement.	Security audit; informal support; referral to other agencies; referral for property marking, and; links to neighbours.	Intervention (advice) given at 31.7% of properties (p.9). Result was locks and alarms adopted by 8% and 4% of victims respectively – low implementation rate. Police reluctant to get involved initially. Victims resistant to measures.	No evidence of spatial displacement.
Liverpool (Bowers et al. 2003)	3317 households in Liverpool (p.5).	Non contiguous area with similar socio economic makeup (p.6). 2658 households (p.7)	Security survey and installation of physical security measures (door chains, door and window locks) for victims and vulnerable residents (elderly, students, low income). Smartwater property marking offered to all residents. Alley-gating. Intensive supervision of offenders.	Implementation rate not discussed. All households in target area visited – measures offered to vulnerable as well as victims. Resistance to alley-gating schemes.	Report states that "theft from car significantly increased in the area. There was no significant switch to theft from a person, taking a vehicle without the owners' consent or theft of car" (vii).

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Orange (Western Research Institute 2003)	Orange, New South Wales, Australia. No further information provided.	The remainder of the Central West area.	Based on 'Biting Back', a 3 tier intervention including security audits, temporary alarm and targeted patrols. Target hardening suggested at first visit (discount vouchers), security upgrades provided (free) at future visits. Also security audits for non victims.	"About two thirds" (p.20) of the 244 (p.17) non-victims who received a home security audit upgraded their security. The implementation rate for victims is not discussed. Cost and existing security cited as reasons for not upgrading.	No evidence of spatial displacement (p.14). Out of seven other acquisitive crime types examined, two have increased (p.15).
Hartlepool (Sturgeon-Adams et al. 2005)	2 areas of South Hartlepool, consisting of "approximately 3500 households" (p.6)	Unnamed comparison area to the north of Hartlepool town centre "chosen due to its similarity… in terms of socioeconomic composition" (p.20). The comparison area is used for overall burglary, however repeat burglary is compared to the only available comparison of Hartlepool Division 1.	Crime prevention survey and related upgrades (free). Hot spot alley gating. Property marking and plug in timers. Other initiatives: diversionary programs for young people; intervention for offenders; community development; education and awareness campaign for local residents.	Only 24 repeat victims were identified during the course of the project. 5% of all burglary victims over the 2 year project received target hardening. Property marking had previously taken place in the target area. Alley gating met with resistance.	A diffusion of benefits was seen to the 600m buffer zone. Due to data difficulties, no crime type displacement was measured.
Bentley and Morley (Cummings 2005)	2 of Perth's suburbs: Bentley and Morley	"Metropolitan Perth" (p.32) a larger area. Surrounding suburbs were examined for displacement and diffusion of benefits.	A "range of community and policing initiatives" (p.14) including: free home security audits for victims; distribution of crime prevention material to neighbours; public awareness campaign; targeting of known offenders; encouraging local council workers to report suspicious behaviour; targeting truancy and providing recreational programs for at risk youths.	17.7% of the 631 burgled residences had security audits which went to completion. 114 follow up surveys were conducted with security audited burglary victims. Of these, 72 had target hardened their properties. This is 63% of those surveyed but only 11.4% of burglary victims. Initial high drop out of volunteers. Victims reluctant to receive visits.	Spatial displacement did not occur in Bentley, but "may have occurred in Morley to a limited extent" (p.36). Other crime types in the area also saw "sharp declines" (p.36). It is suggested that diffusion of the program's effects may have occurred (p.32).

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Commercial burglary					
Multnomah (Pearson 1980)	435 non-residential properties (p.5), of which 198 were victims in 2 month pre-intervention period (p. xii)	225 previously victimized non-residential properties (p. xii), geographically similar, similar business type (p. 11).	Security survey was conducted at the premises, and a written report with detailed suggestions on physical security improvements was given to the participants (p. 2)	31.7% compliance with security suggestions overall.	Not measured
Leicester (Tilley and Hopkins 1998)	680 businesses in Belgrave and 701 in West End	Remainder of subdivisions (larger area)	Chronically victimized businesses (10+ incidents) had risk assessments and security reviewed ("A little money was made available to encourage businesses to implement suggested measures." p. 5). Repeat burglary victims had "tailored graded measures" (p.4) to reduce burglary. Businesses that were identified as being "most severely affected by customer theft, abuse and violence" (p.5) were provided with "fact packs" (p.5)	Of 49 businesses identified as chronic victims, 23 were visited, plus 19 referred by the police (42 in total). 28/42 were "receptive" to risk reduction suggestions (p.5).	At the time of the report, non-domestic burglary was not separated out from commercial burglary in police figures (p.9)
Merseyside (Bowers 2001)	105 businesses visited by a crime prevention officer.	The remainder of the businesses surveyed twice: those that did not qualify for a CPO visit – 221 in total. (p.26)	Crime Prevention Officer visited medium and high risk businesses: gave risk assessment and tailored advice on improving security. Financial assistance of 50% (max £1500) was provided towards any recommended target hardening measures.	Target hardening implementation rate of 60% for eligible businesses.	No evidence of spatial, offence, or tactical displacement (p.41). The non intervention group consisted of businesses with a lower crime risk than the intervention group. Several other crime types also experienced a reduction in prevalence.

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Domestic violence					
NDV Project Evaluation (Morgan 2004)	Two areas: Port Adelaide and South Coast	Compilation of "other metropolitan areas" (p. 60)	3 tier (flexible) interventions focused on victim and offender. Victim received information, personal safety plan and in some cases "duress alarm" (p. 5). Targeted police patrols.	Intervention provided regardless of gender or age. Of the 1269 incidents at levels 1,2 and 3, 85% were given a letter and victim information kit, 73% were provided with a safety plan. There were additional incidents classed as level X. These were unusual cases that did not fit into the generic model, and were given a more tailored intervention, details of which are not available.	None discussed.
Sexual victimization					
Hanson and Gidycz 1993	181 female undergraduate psychology students, of which 59 were prior victims of "moderate sexual victimization" and 43 were prior victims of "severe sexual victimization" (p. 1050)	165 female undergraduate psychology students, of which 46 were prior victims of "moderate sexual victimization" and 48 were prior victims of "severe sexual victimization" (p. 1050)	Acquaintance rape prevention program (free to participants). Education based.	The way in which participants were allocated to intervention or control sessions was not discussed.	The program may have been effective for non-victims – those in the control group were "more than twice as likely" (p. 1050) to be victimized in the follow up period than non-victims in the intervention group.

Study (Authors)	Intervention Group	Comparison Group(s) (Any differences to intervention area?)	Intervention Tactics	Implementation – Measures and Issues	Displacement? / Other Issues Arising
Breitenbecher and Gidycz 1998	211 female psychology students, of which 132 were prior victims.	195 female psychology students, of which 133 were prior victims.	Sexual assault risk reduction program (free to participants) adapted from Hanson and Gidycz (1993). Education based.	"Experiment sessions were randomly designated as either treatment or control sessions." (p.475)	Not discussed.
Marx et al. 2001	24 undergraduate women with "a history of sexual victimization" measured from age 14 (p.26)	37 undergraduate women with "a history of sexual victimization" measured from age 14 (p.26)	Two 2-hour revictimisation prevention workshops (free to participants). Focus on risk recognition and managing risky situations.	Random assignment to intervention and control groups.	"It is unclear whether the intervention or pre treatment risk recognition was more important in determining subsequent victimization" (p.30)
Gidycz et al. 2001	459 women, of which 241 were prior victims of rape or moderate sexual victimisation	347 women, of which 158 were prior victims of rape or moderate sexual victimisation	Acquaintance rape prevention program of 50 to 60 minutes (free to participants). Education based.	The way in which participants were allocated to intervention or control sessions was not discussed.	Men were also included in this program in an attempt to reduce self reported sexual aggression
Davis et al. 2006	46 "urban women" (p.2) victims. 80% had the post test measurement.	38 "urban women" (p.2) victims. 90% had the post test measurement.	Two 2-hour rape avoidance workshops (free to participants).	Random assignment to intervention and control groups with 13 exceptions.	Not discussed.

A summary of key indicators is shown in Table 4. Studies are listed chronologically by crime type. Residential burglary is first because it accounts for 22 of the 31 studies that have been evaluated, then domestic violence, commercial burglary, and sexual victimization. Study identifiers (often the location name), the date of the publication of the evaluation, and the crime type to be prevented, are shown in the first three columns. The two main outcome indicators are the change in repeats and the change in the overall level of crime. There have been evaluations conducted where preventing repeats was part of a broader crime prevention effort but these are not included if the repeat victimization component could not be distinguished.[2]

Whether a reduction in repeat victimization was found among those receiving the crime prevention effort (the intervention group) is shown in the fourth column of Table 4. By this indicator, repeats fell in 17 out of 21 studies (81%). In the other 10 studies the extent of change in repeats was unknown or equivocal. On average, repeat victimization was reduced by more than half (mean = 60%, median = 69%) across the 9 studies where it was measured. However there was wide variation, from one project where repeats were eliminated to one where the best estimate was that repeats fell over 15%. Readers who are interested in evaluation methods should note that the change in repeat victimization was typically not measured in comparison groups.

[2] In addition, Wellsmith and Birks (2008) is the only study, to our knowledge, evaluating the prevention of near repeat burglary, and they tentatively indicated some success. Related areas of crime concentration from hot products to hot spots are not included though we suspect that the time will come when such areas are more integrated.

Table 4. Summary of Outcomes for Repeat Victimization Prevention Studies.

Evaluation	Author and Year	Crime type	Change in repeats	Change in overall crime count (incidence)	Positive (+) negative (-) or uncertain[3]
Kirkholt	Forrester et al. 1988, 1990	Residential burglary	-100%	-62.8%	+
St. Anns	Gregson 1992	Residential burglary	NA	-9.2%	+
The Meadows	Gregson and Hocking 1993	Residential burglary	-40.4%	-57.5%	+
Eyres Monsell	Matthews and Trickey 1994a	Residential burglary	Yes	-6%	+
New Parks	Matthews and Trickey 1994b	Residential burglary	-50%	+17.5%	u[A]
Blackburn	Webb 1996	Residential burglary	-68.8%	-62%	+
Burnley	Webb 1996	Residential burglary	-33.3%	-27.2%	+
Lambeth	Webb 1996	Residential burglary	NA	-80%	+
Merthyr Tydfil	Webb 1996	Residential burglary	-92%	-26%	+
Huddersfield	Chenery et al. 1997	Residential burglary	Equivocal	-30%	+
Cambridge	Bennett and Durie 1999	Residential burglary	No	+13.8%	-
Baltimore	Weisel et al. 1999	Residential burglary	No	-23.7%	u[B]
Dallas	Weisel et al. 1999	Residential burglary	No	+16%	-
San Diego	Weisel et al. 1999	Residential burglary	No	-24.7%	u[B]
Beenleigh	Budz et al. 2001	Residential burglary	>-15%	+9.9%	u[A]
Ashfield	Taplin and Flaherty 2001	Residential burglary	Equivocal	+1.8%	-
Tea Tree Gully	Morgan and Walter 2002	Residential burglary	Equivocal	+7.5%	-
Liverpool	Bowers et al. 2003	Residential burglary	-70.5%	-39.2%	+
Orange	Western Research Institute 2003	Residential burglary	-74%	-57%	+
Hartlepool	Sturgeon-Adams et al. 2005	Residential burglary	Yes	-18.3%	+
Bentley	Cummings 2005	Residential burglary	Yes	-26.2%	+
Morley	Cummings 2005	Residential burglary	Yes	+2%	u[A]
Multnomah	Pearson 1980	Commercial	Yes	-14.9%	+
Leicester	Taylor 1999	Commercial	Yes	-19.7%	+
Merseyside	Bowers 2001	Commercial	Yes	-39.2%	+
NDV[4]	Millbank and Riches 2000	Domestic violence	Yes	-8.2%	+
Sexual Assault Prevention	Hanson and Gidycz 1993	Sexual	NA	-17.8%	+
Reduce multiple sexual victimization	Breitenbecher and Gidycz 1998	Sexual	NA	-2%[5]	+
Sexual Victimization Prevention	Gidycz et al. 2001	Sexual	NA	-36%	+
Acquaintance rape prevention	Gidycz et al. 2001	Sexual	NA	+12.1%	-
New York and Seattle Field Test	Davis et al. 2006	Sexual	NA	-10.3%	+

[3] u = Uncertain where the superscript A denotes three sites where repeats fell but incidence increased, and superscript B denotes two sites where repeats did not decrease but incidence did. See text for further details.

[4] Outcomes measured as domestic violence calls to the police.

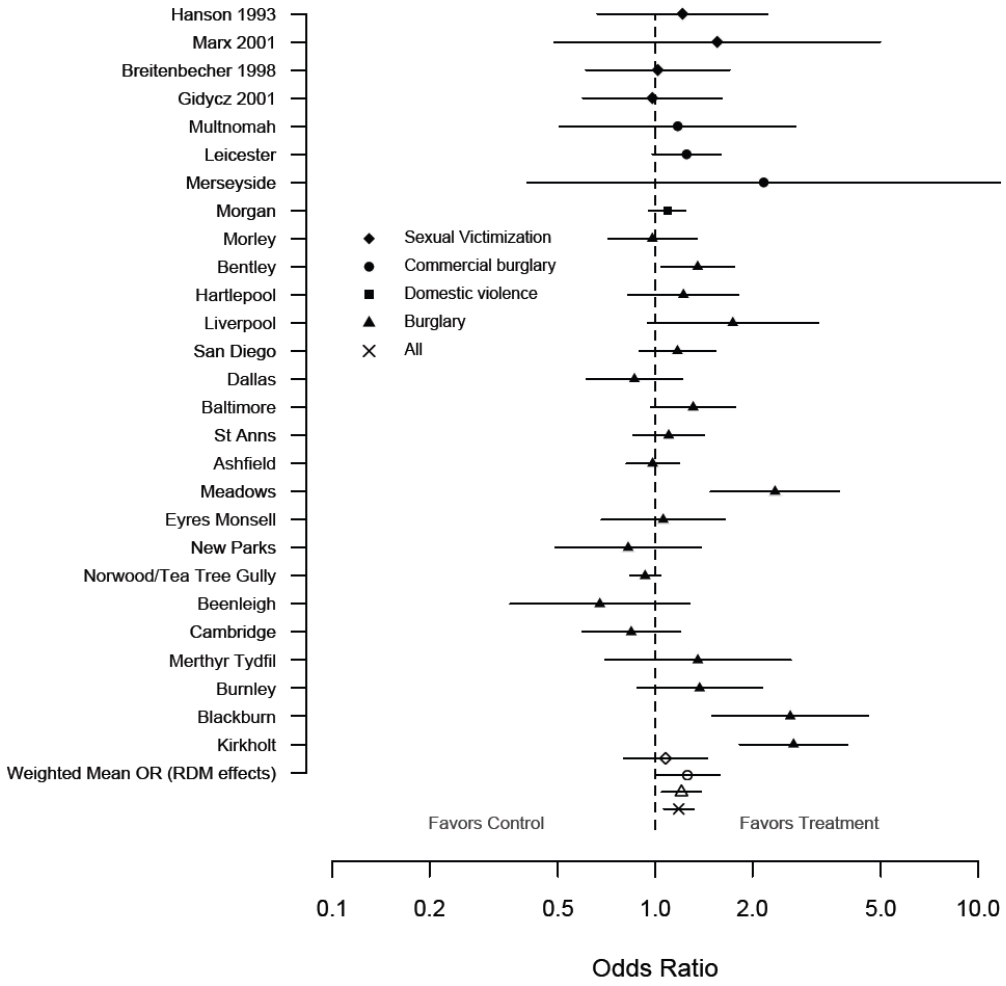
[5] Note that the five sexual victimization projects show change in crime prevalence not incidence in the fifth column.

For each study, overall crime – not just repeats - in the intervention group was compared to a similar group. The aim of such a comparison is to try to rule out the possibility that any change in crime was due to factors other than the intervention. This process of counterfactual inference is possible when both groups have all factors in common other than the intervention. For example, a regional fall in crime would be experienced in both an intervention and comparison area which means it could be distinguished from the effect of a successful intervention because the remainder of the fall in crime in the intervention area can be attributed to the intervention.

The fifth column of Table 4 shows the percentage change in crime in the intervention group relative to the comparison group. Crimes decreased in 23 out of 31 evaluations. In the 26 studies of crime incidence, crimes reduced on average across the studies by one fifth (mean and median = 21.7%).[6] The sixth column shows whether the project had a positive outcome of reduced crime, denoted by '+', or a negative outcome of increased crime, denoted by '–'. Five studies are categorized as uncertain or 'u' due to apparently conflicting indicators. With those five excluded, 21 out of 26 evaluations (81%) yielded positive outcomes.

[6] The inter-quartile range was from -39.2% to +1.9%.

Figure 1. Outcomes of Repeat Victimization Programs Based upon Crime Incidence



Another way to examine this data is represented in the Forest graph of Figure 1, which shows the impact as an effect size (the point) with confidence intervals around it (the lines) for each study. The effect size is the Odds Ratio (OR), which has a chance value of 1. As mentioned about, this indicates the relative change in the control group compared to the intervention group. All except four of the studies listed in Table 3 could be included in this analysis. This more conservative analysis suggests that 19 out of 27 studies (70%) reduced crime but only four (15%) obtained statistically significant results (those where the confidence interval did not include the value of 1).

The aggregate indicator which is generated from all possible studies is the weighted mean OR of 1.18 (95% Confidence Interval: 1.07–1.32), shown at the base of the chart along with the effect sizes for the three crime type groups which included more than one study. This value of the OR indicates that crimes in the control area increased by 18% relative to the intervention area, or conversely that crimes in the intervention area decreased by 15% (based on 1/1.18) relative to the control area. The weighted mean ORs for all of the evaluations and by crime type are detailed in Table 5 with their confidence intervals and Q statistics.[7]

The effectiveness of programmes varied by crime type. Table 5 summarizes the weighted mean effect size for the four crime types included. This suggests that efforts designed to prevent repeat residential burglary were effective. On average, crimes increased by 20.6% in the control condition compared to the intervention condition, or conversely crimes decreased by 17.1% (using 1/1.206) in the intervention condition compared to the control condition. With a lower confidence interval for the OR which is very close to 1 but on the wrong side, it cannot be said that efforts designed to prevent repeat commercial burglary were statistically significant. However, the weighted mean effect size suggests that they were effective. On average, crimes increased by 25.8% in the control condition compared to the intervention condition, or conversely crimes decreased by 20.5% (using 1/1.258) in the intervention condition compared to the control condition. Programmes designed to prevent repeat sexual victimization have not been effective, as indicated by the fact that the lower confidence interval had a value of less than 1 and the weighted mean OR was only 1.077.

[7] The Weighted Mean Effect Size (WMES) or Weighted Mean Odds Ratio (OR) gives greater weight to studies with a smaller standard error (s.e.). The Confidence Intervals shown for each study in Figure 1 were computed using 1.96 standard errors but as the s.e. is likely to be under-estimated using the standard formula they were multiplied by 2. Without doubling each s.e. (a conservative test), the WMES would be somewhat larger. Additional studies evaluating advice to victims of family violence and elder abuse have been conducted by Robert Davis and colleagues (e.g. Davis and Medina-Ariza, 2001; Davis et al. 2006). These have much in common with the work reviewed here but the studies were not part of this review. While more work is needed to integrate that body of work, if its results seem less promising, we suspect this may be a result of what is assessed here as low implementation rates and weak crime prevention mechanisms, particularly when prevention relies on education and advice rather than on tactics with stronger situational mechanisms.

Table 5. Outcomes by Crime Type with Confidence Intervals

Crime type	Q	Lower CI	Upper CI	Mean OR	N studies
All	69.19	1.063	1.315	1.183	27
Residential Burglary	66.56	1.047	1.389	1.206	19
Commercial burglary	0.427	0.998	1.587	1.258	3
Sexual	0.723	0.80	1.45	1.077	4[8]

Note: Q = heterogeneity; CI = Confidence Interval; OR = Odds Ratio

The overall conclusion is that the evidence provides strong support for the fact that repeat victimization has been prevented, and this can be said with greatest certainty in relation to burglary, which decreased by 17%–20%. However, it is clear that there is quite some variation in impact across time and place. With respect to that issue, it has been noted that:

> *"If, for a particular intervention, some studies produced large effects, and some small effects, it would be of limited value simply to combine them together and say that the average effect was 'medium'. Much more useful would be to examine the original studies for any differences between those with large and small effects and to try to understand what factors might account for the difference. The best meta-analysis, therefore, involves seeking relationships between effect sizes and characteristics of the intervention, the context and study design in which they were found." (Coe, 2002: 9)*

Consequently, the next section examines why some efforts succeed more than others.

[8] Two of these studies had multiple outcome measures, based on the severity of sexual victimization. These have been combined into the weighted mean odds ratio calculation here; the outcomes are displayed separately in the odds ratio chart for clarity and ease of reference.

4. Further Analysis

Each of the studies examined within this report had some features unique to the particular project, crime type, and context. Overall, the three common determinants of success in efforts to prevent repeat victimization were:

1. Successful conception and development of a functioning project,
2. Identification of context-specific and effective preventive tactics, and
3. Thorough implementation of those tactics.

The first of these features relates to the process of identifying an active ingredient and mechanism to reduce opportunities for repeat victimization. This process may involve 'borrowing' ideas from other projects, or be more innovative in nature. This stage also involves the identification of the appropriate means for delivery, whether this makes use of police, Victim Support, volunteers, or specifically employed project staff. Sexual victimization prevention schemes emphasized the education of repeat victims, with the provision of general advice about how to avoid or manage risky situations. The specific nature of this advice was not necessarily clear in all of the evaluation reports. However, a key problem with education seems to be that it may change attitudes without necessarily changing behaviour or situations, or if behaviour and situations are changed this was not necessarily in a way that prevented crime. The measures typically used in relation to burglary, in contrast, tended to be of the 'situational' crime prevention variety which more directly impacted upon behaviour by restricting choices and options.

The evidence suggests that the same tactics do not necessarily work in different contexts. For some of the burglary projects in particular, it seemed that 'the usual' target-hardening security measures were introduced without checking whether or not they were appro-

priate to the type of burglary problem or whether other tactics were also needed. For example, prevention measures that are appropriate to prevent burglary of inner-city apartments are not necessarily the same as those that are most effective for suburban burglary. Therefore, the types of measures needed varies by time and place and if they were not locally appropriate then effectiveness would be reduced.

A further key issue is that it is often difficult to implement prevention measures for various reasons. To explore this further we sought to empirically gauge the extent of implementation. Figure 2 shows the relationship between the implementation rate and the impact on crime for the 12 studies where both measures were available. The implementation rate is defined as the percentage of eligible units (e.g. households previously burgled) who received the preventive intervention. The impact on crime is the percentage change in crime relative to the comparison group (from column 5 in Table 4). Where the intervention was provided to victims as 'advice', the implementation rate was measured as the percentage of those eligible who followed the advice by implementing the prevention tactics.[9]

[9] The chart excludes the five studies of sexual victimization as implementation information could not be derived for them.

Figure 2. Relationship between Implementation Rate and Impact on Crime.

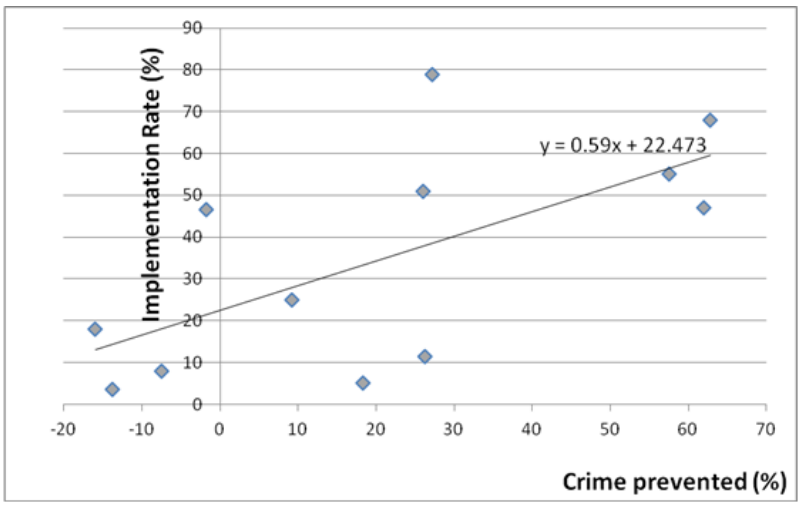


Figure 2 can be interpreted as preliminary empirical evidence that the crime prevention impact increases as the implementation rate increases. This would be in keeping with expectation based on theory. If the data were of better quality, or implementation easier to gauge, then perhaps the relationship would be stronger. The linear best fitting line does not fit the data very well (R^2=0.413). However, it suggests that a project must implement measures at a minimum of one fifth of targets (22.5%) before any impact is achieved, that every 0.6% additional increase in the implementation rate produces a further 1% reduction in crime, and that crime is eliminated when the implementation rate exceeds 81.5%. Clearly this best fitting line cannot be interpreted so literally, as there are many uncontrolled variables and a key mediating variable would be the appropriateness of the prevention measures introduced, but it may be indicative of the general nature of the relationship between implementation and impact.

Table 6 lists the generic types of difficulties experienced that were reported in the studies included in this review.[10] Two of these problems relate to the successful conception and identification of appropriate responses. Problems with the identification of context-specific prevention measures are categorized in Table 6 as *lack of tailoring*. Some burglary prevention projects were required to provide security to other sections of the population who were considered by local agencies to be vulnerable, such as elderly people and single mothers. This meant that the prevention effort lacked focus and that it was

[10] We recognise the need for further work and inter-rater reliability tests to confirm this preliminary typology of problems.

not only the prevention of repeat victimization which was being evaluated. For present purposes this is categorized as *unclear eligibility criteria*.

Four types of implementation problem appeared to arise and are shown in Table 6. *Staff problems* relate to the staff employed to implement the project. It was often difficult to recruit staff, to train staff, to retain staff, and to ensure that staff were undertaking work in the desired manner. *Communications breakdowns* could be detrimental and were quite common in multi-agency projects where different agencies and parties were involved with different goals and different means of achieving them. Projects with *inflexibility* did not tend to learn from their mistakes and failed to accommodate changing demands within the project. In some projects, there was *resistance to tactics* that were to be implemented, either from potential recipients who did not want them or from those who were required to implement them.

Data problems were a more general issue. Particularly with respect to the collation or analysis of police data sets, data problems led to difficulties in identifying how many households or persons had been victimized, and in evaluating whether crime had been prevented.

Table 6. Main Types of Problems during Project Development and Implementation.

	Development and general issues				Implementation issues		
Evaluation study	Lack of tailoring	Unclear eligibility criteria	Data problems	Staff problems	Communications breakdown	Inflexibility	Resistance to measures
Kirkholt							
Blackburn	X	X					
Meadows		X	X				X
Liverpool		X					X
Burnley					X		
Merthyr Tydfil		X					
Bentley				X			X
Baltimore			X	X	X	X	
Hartlepool			X	X	X		X
San Diego	X		X	X	X	X	X
St Anns				X			
Eyres Monsell	X				X	X	X
Ashfield	X		X	X			X
Morley				X			X
Norwood/TTG	X			X	X		
Dallas			X			X	X
Cambridge							X
New Parks	X			X	X	X	
Beenleigh	X		X	X			X
Never Again						X	
Lambeth		X	X	X	X		
Huddersfield			X				
NDV		X	X				X
Leicester			X		X		
Merseyside							X

Notes to table:
(1) Implementation data were not available for the five sexual victimization studies and for one commercial burglary study (Multnomah).
(2) 'X' indicates that this type of problem was identified in the study's report.

An informative example shows the importance of implementation. The authors of one study which was excluded from the present review were so dispirited at the failure of police officers to conduct security surveys at victimized households that they noted "If we take

the results at face value, those officers who declined to carry out the survey thereby facilitated the revictimization of many of those they were charged to help." (Thompson et al. 2008: 132).

Overall, the most effective projects were those which combined high implementation rates with strong preventive mechanisms. Appropriately targeted situational security measures aimed at preventing repeats by the same *modus operandi* were effective. Thus stronger doors and window locks plus other measures can prevent crime when appropriately targeted. However, advice and education to victims are usually not effective preventive measures themselves, but may be mainly a means of encouraging the adoption of preventive measures. This is why the level of measures adopted rather than the extent of education or advice provided is the appropriate way to gauge implementation. It is important that the results are not represented as a falsification of the theory of preventing repeat victimization if poor tactics or poor implementation meant that few or no crimes were prevented.

5. Conclusions

Many of the evaluated efforts succeeded in preventing repeat victimization. Over all the evaluations, crimes increased by 18.3% in the control condition compared to the prevention condition, or conversely crimes decreased by 15.5% in the prevention condition compared to the control condition. The most successful efforts used comprehensively implemented situational crime prevention measures. When few or no crimes were prevented, this appeared to be attributable to two main reasons. First, some prevention tactics were weak or inappropriate. In addition, well-meaning advice and education did not prevent crime, unless it resulted in the adoption of a strong prevention measure. Second, a failure to implement preventive measures, or a low rate of implementation, not surprisingly, did not prevent crime.

While repeat victimization can be prevented, for the full potential of this crime prevention strategy to be achieved the evidence suggests that there needs to be significant additional investment in research and development, and far greater attention to implementation. Problem-solving and action research approaches that develop strong prevention tactics based on careful analysis of the crime problem should be developed, and Sidebottom et al. (2012) suggest the potential of checklists to help pursue such goals. The evidence base will be improved greatly if such efforts include a broader range of crime types than have been addressed in work to date.

A portfolio of research on preventing repeat victimization may benefit from including a greater emphasis on preventing near repeats of various sorts. There is an increasingly clear conceptual overlap between the repetitive nature of crime and its tendency to cluster along whatever dimension is measured. The similarity of previous and future crimes is the common factor among these repeat crime clusters, and the more similar the crimes, the greater the potential to develop an informed and efficient prevention response. Based on the range of evidence examined, the overwhelming conclusion of this report is that further efforts to prevent repeat victimization would be fruitful for policy and would greatly benefit crime victims.

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