

SYNERGIES OF SYNTHESSES: A COMPARISON OF SYSTEMATIC REVIEW AND SCIENTIFIC REALIST EVALUATION METHODS FOR CRIME PREVENTION

by

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A Doctoral Thesis

Submitted in partial fulfilment of the requirements
for the award of

Doctor of Philosophy of Loughborough University
July 2010

Abstract

This thesis makes two significant contributions to the advancement of knowledge within crime prevention. The first of these is to evaluate the success of repeat victimisation prevention interventions. Interventions across four crime types are assessed herein, and the context-mechanisms-outcome configurations examined. The second contribution of this thesis is to assess two techniques of meta-evaluation: systematic reviews and realist syntheses. Each of these techniques is used in turn to assess the repeat victimisation prevention interventions. The contribution of each technique to the knowledge pool is then discussed, and the question of whether they are complementary or contradictory approaches answered.

The thesis is framed in the context of evolutionary epistemology, which is the philosophy underpinning both approaches to meta-evaluation addressed herein. The thesis starts, with an examination of: firstly, how the evaluation methods in question have evolved, and the background to their scientific worth; and secondly, how situational crime prevention measures have evolved over time. The thesis then examines the two competing approaches for their contribution to the evaluation ecosystem by using both to assess repeat victimisation prevention interventions. Finally, the last section poses the question of whether it is survival of the fittest, or whether co-existence or adaptation could be the key to survival for these two meta-evaluative methodologies.

Repeat victimisation prevention is revealed as an effective way of reducing crime, with a need for further research to apply the principle across further crime types. A requirement is identified for a greater breadth and depth of information to be included in future crime prevention evaluations. The systematic review is shown to be a useful way of assessing the overall effectiveness of the interventions, whilst the realist synthesis fills in the detail of why some interventions work and others fail. It is concluded that both approaches to meta-evaluation have useful contributions to make, and that a 'third way' incorporating the best elements from each method should be developed.

Key words

Meta-evaluation; repeat victimisation; situational crime prevention; realist synthesis; systematic review; evaluation methodology

Acknowledgements

Particular thanks are due to my supervisor Graham Farrell. The unwavering support of my husband Mark deserves special recognition. I would like to express my gratitude to everyone who was there for me throughout my PhD – the process would have been infinitely harder without the encouragement of my parents and friends.

Thanks to everyone for being generous with their time, expertise and contacts: Pete Bumpus, Sylvia Chenery, Alan Edmunds, Ken Pease, Robert Street, Rebecca Thompson, Nick Tilley, and Mark Wilson.

Thanks also go to the authors who were kind enough to send me copies of evaluations, your help made this possible: Rick Cummings, Stephanie Taplin, and Janice Webb

I would also like to acknowledge the support of the ESRC in funding my postgraduate studies, award number PTA-031-2006-00065.

My heartfelt apologies to anyone I've neglected to list by name, your contribution is nonetheless greatly appreciated.

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SECTION I:

INTRODUCTION AND LITERATURE REVIEW

Real knowledge is to know the extent of one's ignorance

- Confucius

Chapter One: Introduction

This report, by its very length, defends itself against the risk of being read.

- Winston Churchill

BACKGROUND

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DEFINITION OF KEY TERMS AND CONCEPTS

THE EVOLUTIONARY EPISTEMOLOGY OF THEORIES

This approach to the nature of knowledge stems from the Darwinian theories of evolution. It draws on issues such as “pragmatism, empiricism [and] the validation and legitimation of knowledge” (Plotkin, 1982:3). It is characterised by the change in focus “from science as

product to science as process” (Zammito, 2004: 122) thus acknowledging the ever changing nature of knowledge as new theories and problems are identified.

Popper (1959) is credited with the recognition that the succession of theories in science has a “similar selective elimination process” to natural selection (Campbell, 1974 [1982:75]), although it was Donald Campbell who coined the term ‘evolutionary epistemology’ (Bradie and Harms, 2008). Campbell (1974 [1982:101]) argues that the evolutionary epistemology is fully compatible with his own and with Popper’s stance of “critical hypothetical realism”. Indeed, Campbell and Popper shared much common ground in their understanding of evolutionary epistemology. This is an important notion, as each of these academics spawned one of the approaches to meta-evaluation discussed in this thesis.

EVOLUTIONARY THEORIES AND THEIR APPLICATION TO CRIME

Evolutionary theories provide a theme which can be applied throughout this thesis. The realist synthesis and systematic review approaches share a *common ancestor* in the evolutionary epistemology discussed above. They have pushed each other to develop in a way akin to *co-evolution*, where each has exerted pressures on the other, thereby affecting the way in which each evolves. Situational crime prevention has *evolved, adapted*, and occasionally taken leaps analogous to *mutation* in the years since it was first developed, often in order to face new crime challenges, for example to tackle technologically based crimes (see for example Newman and Clarke, 2003). In order to develop the field of meta-evaluation further, the thesis argues that *co-operation* between the two methodologies is required.

Beyond the confines of this thesis, further parallels can be drawn between the natural world and the criminal world. Empirical research on the behaviour of offenders has provided support for theories of offender foraging (Johnson et al. 2009). Felson (2006) discussed crime’s ecosystems; and viewing victimisation as a communicable disease aids with predicting future crime patterns (Johnson and Bowers 2004; Townsley et al. 2003). Clarke (2008: 189) discusses the concept of criminal *adaptation*, where offender *populations* “discover new crime vulnerabilities after preventive measures have been in place for a while”.

SITUATIONAL CRIME PREVENTION

Situational crime prevention provides the theoretical framework within which the practical elements (i.e. the studies examined) of this thesis sit. This approach to crime reduction emerged during the 1960-70s, and is still often perceived as a relatively *niche* area of criminology – as Clarke (2008: 192) states, “criminologists have generally shown little interest in situational prevention”. The new crime theories which underpin many of the interventions discussed throughout the thesis focus on explaining and identifying opportunities for crime, and considering *how* crime occurs (rather than *why* crime occurs – Clarke, 2004). An understanding of situational crime prevention as presented in Chapter Three allows the interventions herein to be viewed in terms of both theoretical and practical contributions to knowledge.

REPEAT VICTIMISATION

Just four percent of victims experience 38 - 44 percent of crime according to the British Crime Survey (Bridgeman and Hobbs, 1997). This repeated and chronic victimisation experienced by some individuals, places and businesses has two underlying theories: event dependence and state heterogeneity, which are explained in Chapter Three. The knowledge that once someone has been a victim once, they are more likely to be a victim again can be used to interrupt the cycle of revictimisation. By preventing a victim from becoming revictimised, not only does that prevent one crime, but because many repeat victims are chronically victimised (they suffer from many crimes over a single 12 month period) this interruption of the victimisation cycle has a disproportionate effect on overall crime incidence. This is a mechanism which situational crime prevention interventions can use to their advantage, as this targeting of limited resources to likely locations of future crime events is an effective way of 'drip feeding' crime prevention measures (Pease, 1991; 1998). The thesis examines interventions which have used repeat victimisation as a way to target their crime prevention measures, to assess how successful this can be in practice, and to identify different problems which can occur during this process. The context, mechanisms and outcomes of the repeat victimisation prevention interventions are examined to discover differences between interventions, and discuss whether these differences have had a discernable impact on the programmes examined. Chapters Five and Seven present findings both about individual programmes and overall repeat victimisation prevention, whilst lessons for future repeat victimisation programmes are drawn out in Chapter Nine.

THE RESEARCH PROBLEM

USING REPEAT VICTIMISATION TO TARGET CRIME PREVENTION MEASURES

Once a victimisation has occurred, there is more likely to be a subsequent victimisation. This premise allows the prediction of future crime events, and therefore the most viable locations to interrupt the opportunities in order to prevent that crime from occurring. The prevention of repeat victimisation was introduced as a key performance indicator to the police in the mid 1990s (Laycock, 2003). However, there remains a need to assess the effectiveness of this use of repeat victimisation to identify pinch points for crime prevention resources in practice. Farrell and Pease (2006) demonstrated the potential within repeat domestic burglary victimisation to target crime prevention measures effectively. This thesis builds on Farrell and Pease's work by conducting a full review of interventions, statistically analysing the identified studies and expanding to examine the prevention of repeat victimisation across different crime types. Repeat victimisation prevention is shown herein to be an effective mechanism of reducing crime, although a dearth of research within some crime types limits the conclusions that can be drawn.

TWO COMPETING APPROACHES TO META-EVALUATION

There are two prominent, competing, approaches which could be used to synthesise the available data on repeat victimisation prevention. The first of these, the systematic review, has been in widespread use in crime prevention since the establishment of the Campbell

Collaboration in 1999. This takes a substantially quantitative approach to reviewing interventions, and insists on inclusion of only the highest methodological quality of evaluations. On this hierarchy of evidence, randomised controlled trials are perceived as the gold standard, with anything less than a pre-post measure with comparable control group being rejected. This approach is lauded for its scientific approach to data, but criticised for losing valuable information. The second approach, realist synthesis, has not entered the mainstream within the context of crime prevention to the same extent as systematic reviews, reflected by the relatively few reviews written in this style. It takes a more qualitative, iterative approach to data, and rejects the use of hierarchies of evidence to assess the quality of evaluations. Rather than examining the question of 'whether' an intervention works, as the systematic review does, a realist synthesis focuses instead on the context, mechanisms and outcomes of an intervention (the CMO configuration). This approach is lauded for linking theories with practice to identify how an intervention works, but criticised for its lack of methodological clarity. The challenge of this thesis is therefore to consolidate these two approaches, taking the best elements from each to produce a 'realist systematic review' of repeat victimisation prevention interventions.

RESEARCH QUESTIONS

- Are both systematic reviews and realist syntheses working towards improving the knowledge and practice of crime prevention?
- What are the main differences – both practical and philosophical?
- Are systematic reviews and realist syntheses complementary approaches, or does one substitute for the other?
- Can the two approaches be reconciled in a positive way?
- Can a combination of the two approaches determine whether or not preventing repeat victimisation is a promising direction for crime reduction?

SUMMARY OF CHAPTERS

Chapter Two debates the merits of different forms of evaluation and meta-evaluation. It summarises the importance of differentiating between correlation and causation in evaluations; explains and distinguishes between controlling for internal and external validity; and introduces the concept of hierarchies of evidence. The differences between experimental, quasi-experimental and non-experimental approaches to evaluations are discussed. The two forms of meta-evaluation utilised later in the thesis are introduced here: systematic reviews and realist syntheses; and the chapter concludes with an explanation of the underpinning philosophy of both approaches. The underpinning argument of this chapter is that the methodologies discussed are all capable of making a useful contribution to the knowledge base of what works to prevent crime.

Chapter Three discusses the background and theory of situational crime prevention measures, many of which feature in the interventions assessed later in the thesis. This chapter focuses heavily on the evolution of the theory and practice of situational crime prevention, and is thus presented in chronological order. The first developments within a theory, perspective, or technique are used to place the issue within the chapter. In this way, the history of situational

crime prevention is traced from the early days of Jane Jacobs' and Oscar Newman's contributions to crime prevention through environmental design, through to the latest steps towards crime science, as proposed by leading academics in the field, such as Ken Pease and Gloria Laycock. Finally, some common criticisms of situational crime prevention are rebutted. This chapter outlines the theoretical assumptions which are threaded through the interventions and evaluations included within the thesis.

Chapter Four describes the methodology used for the systematic review. This chapter follows the format of a protocol from the Campbell Collaboration, the main body involved in the development of systematic reviews in the field of crime and justice. This includes: the objectives of the review; criteria for inclusion and exclusion of studies; a description of the search strategy; and a description of the methods used in the primary research. Details about how studies were coded and the statistical methods incorporated into the review form the remainder of this chapter. This methodology lays out all the steps which were used to conduct the review presented in Chapter Five.

Chapter Five presents the findings of the systematic review of repeat victimisation prevention programmes. The findings are split into four categories, correlating with the crime types within which eligible studies were identified: domestic burglary; commercial burglary; sexual victimisation; and domestic violence. Thirty-one eligible studies are discussed in total. This chapter concludes with a brief examination of what works, what doesn't work, and what's promising in the prevention of repeat victimisation, finding that the evidence shows this to be a successful mechanism of reducing crime.

Chapter Six describes the methodology used for the competing approach of meta-evaluation, that of the realist synthesis. Following the process described by Pawson et al. (2004) this chapter lays out the steps involved in conducting the realist synthesis presented in Chapter Seven. Broadly, these steps are categorised as: defining the scope of the review; searching for relevant evidence; appraising the quality of the evidence; extracting the data; synthesising the evidence; and drawing conclusions. A flow chart providing an overview of the iterative process of conducting a realist synthesis is shown in Figure 6.1 of this chapter.

Chapter Seven presents the findings from the realist synthesis. The chapter addresses a number of hypotheses developed initially from identifying and articulating the programme theories, as well as from the consultative process carried out as part of the realist synthesis. These hypotheses are discussed for the relevant crime types, and supporting and contradictory evidence presented. Common implementation issues are identified, and particular focus is on problems with staffing, data, and flexibility. A final conclusion draws out what can be confidently said about the differences between successful and unsuccessful programmes designed to prevent repeat victimisation.

Chapter Eight is a comparative analysis of systematic reviews and realist syntheses, using the preceding four chapters to inform and support the analysis. The chapter examines the core principles underlying each methodology, compares the techniques used, and discusses the implications of the analysis for future meta-evaluations. Throughout, the contribution each methodology made to the meta-evaluation of the programmes designed to prevent repeat

victimisation is considered. This chapter argues that the two approaches are complementary and co-evolving. A 'third way' of the creation of a 'realist systematic review' is suggested.

Chapter Nine presents the conclusions and recommendations from the thesis as a whole. These relate to both the lessons learnt about preventing repeat victimisation, and to the use of systematic reviews and realist syntheses as meta-evaluative techniques. The contributions of the two methodologies are summarised, and future directions for repeat victimisation research suggested.

Chapter Two: The Evaluation Debate

It is better to debate a question without settling it than to settle a question without debating it.

- Joseph Joubert

BACKGROUND

Evaluation is important for crime prevention. It is only through evaluation that the success of interventions can be known. Evaluation informs policy and practice, their continued improvement and increased efficiency. It is perhaps surprising therefore that evaluation is not routinely employed in crime prevention, and that there is no standardised methodology - or at least no general consensus on such - for that which is undertaken. All too often evaluation is perceived as being unnecessary, where practitioners and policy makers 'know' something to be true, and therefore do not see the need for a formal assessment to be carried out. For example in a study examining the effectiveness of reducing the mortality of premature babies by giving them oxygen enriched air, nurses on one maternity ward ignored the allocation to treatment and control groups because they believed the treatment worked, and provided the oxygen to all babies in their care. Once this was discovered, and the trial conducted correctly, it was determined that not only did the extra oxygen not help improve mortality, but it was linked with increased blindness of infants (Sherman, 1992). This outlines that not only are evaluations necessary, for sometimes results are counterintuitive, but also is an example of how evaluations can be very difficult to carry out in practice.

Rosenthal (1991) suggests that whereas within the physical sciences evidence cumulates over time, that this is not standard practice within social sciences, where there is instead a tendency to start afresh each time a solution to a problem is sought. By having an understanding of what has been attempted in the past, new initiatives have a greater chance for success. As such, effective evaluation arguably lays the foundation for improvements to social interventions.

The focus of this thesis is on evaluation within crime prevention. This is sometimes a contentious issue, and there is debate between academics about what evidence should be used to inform evaluation and subsequently policy. There are two broad schools of thought which

will be examined herein. Firstly, the experimentalist approach brings the notion of ‘pure’ scientific principles into the social sciences. The argument for this is that high levels of control and internal validity ensure that any change in crime rates can be causally linked to a crime prevention programme (see later in this chapter for a discussion of causation and correlation). Experimentalists therefore seek the highest level of evidence possible in order to irrefutably answer questions about ‘what works’ in crime prevention. In contrast, the realist approach dismisses the ‘medical’ model of evaluation, and deems social interventions as being inherently unsuited to the rigid evaluation that traditional science demands. Instead, realists generally favour smaller scale, cheaper, faster evaluations which are tailored to suit the immediate needs of policy makers and practitioners at a local level. Context is very important to realists, and large scale evaluations are perceived as having low external validity and therefore lacking relevance to the ‘real’ world. This characterisation masks many subtleties and complexities but is an attempt to capture the essence of what has become an increasingly important debate.

The common theme joining both experimentalists and realists is one which runs throughout this thesis: that of a focus on practical, policy-influencing evaluation. Neither experimentalists nor realists conduct evaluative research for its own sake. Rather, they have the broader aim of changing for the better existing practices, and spreading the take up of effective social programmes. The evaluation debate in this chapter does not therefore centre on *why* evaluation should be carried out, but on *how* to best evaluate crime prevention programmes in order to ensure that positive, timely progress is made.

This chapter begins this methodological focus by discussing the concept of evidence, and what makes for valid findings in which academics, practitioners and policy makers can have confidence. This includes a discussion on the importance of determining causation rather than correlation, and how three broad approaches to evaluation (experimental, quasi-experimental, and non-experimental) address this, and other, issues within evaluation. The chapter then goes on to discuss how the evidence which emerges from evaluations can be assimilated. Again, experimentalists and realists debate the benefits of their respective approaches: systematic reviews and realist syntheses. An overview of each approach is presented. These two approaches to meta-evaluation are scrutinised later in this thesis to determine the contributions each makes to evaluative research.

EVIDENCE

The evidence which emerges from evaluation research can be criticised where the quality of the evaluation itself is questionable. This in turn can affect how likely the findings are to be taken up by policy makers and practitioners. Two common issues which affect how much impact an evaluation of a crime prevention intervention may have in practice are: firstly, whether it can establish a causal link between the programme and any change in crime; and secondly, whether the measures are valid and can be extrapolated to the broader population. The quality of evaluation research can be assessed by a number of means. One such measure is the Maryland Scale of Scientific Methods, a hierarchy of evidence which has been successfully used in a number of Campbell Collaboration meta-evaluations (see for example Bennett et al., 2008; McDougall et al., 2008). This section examines these issues of quality,

causation, and validity, and the different approaches to evaluating crime prevention interventions.

CORRELATION AND CAUSATION

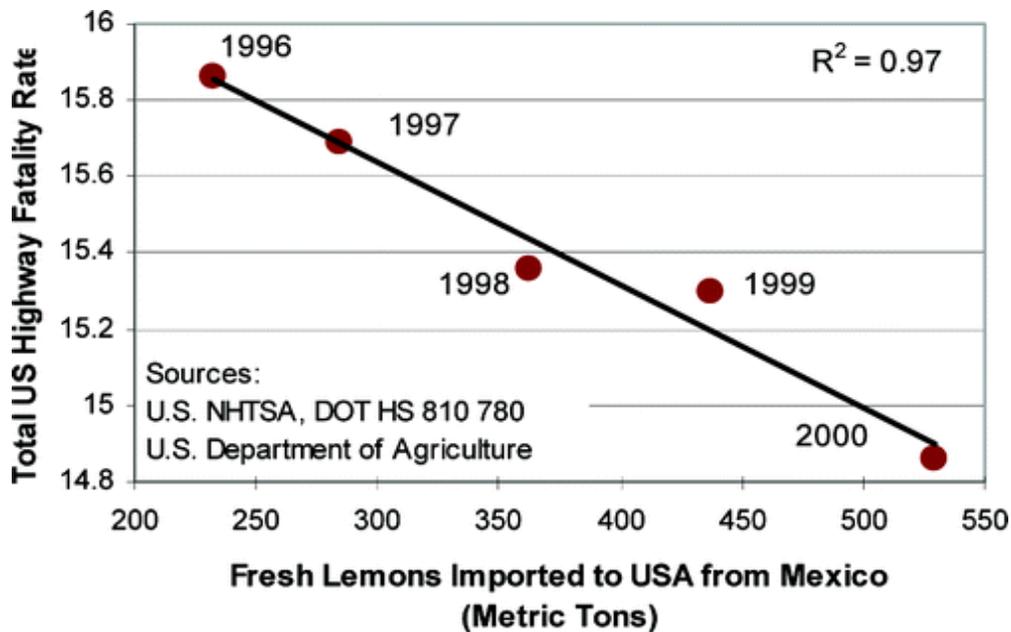


Chart 2.1: An example of a correlational relationship (Johnson, 2008)

The need to differentiate between correlation and causation is essential. Correlation indicates a relationship between two features, whereas causation means that one brings about the other. Cook (2004) discusses the factors which identify causation: that the assumed cause and effect co-vary, and that the cause preceded the effect. In addition, and that which Cook (2004:88) believes to be the most difficult point to establish, is that “no plausible alternatives exist to the hypothesis that deliberately varying the cause makes the effect respond”. Causation is notoriously difficult to establish, and within social research it is almost impossible to control for external influences which can interfere with the causation chain. Mark and Henry (2006: 327) note that “the logic of causal inference does not discriminate between qualitative and quantitative data” and are emphatic about the possibility of using non-statistical routes to establish a causal link. However, they also recognise that experimental and quasi-experimental methods are more often used by “evaluators intending to influence policy-making or generate knowledge” (ibid.).

INTERNAL VERSUS EXTERNAL VALIDITY

Internal and external validity are terms which refer to any threats to producing generalisable causal inferences (Cook, 2004). Internal validity “relates to the association of the measure with its supposed effects” (Tilley, 2009: 162). External validity is concerned with “whether the results of a study can be generalized beyond the specific research context in which it was

conducted” (Bryman, 2004: 539). Common threats to both internal and external validity are listed in Tables 2.1 and 2.2 below, taken from Tilley (2009: 164-5).

Table 2.1: Threats to internal validity (Tilley, 2009)

Threat to internal validity	Explanation
History	Something happens to create change that would have happened anyway without any intervention
Maturation	Treatment subjects mature in the change direction anyway, regardless of the intervention
Testing	The measurement creates the change not the intervention itself
Instrumentation	The measurement methods change and create the impression of real change while there is none
Statistical regression	Treatment targets begin at an extreme position and tend naturally to regress towards the mean without any need for intervention
Seasonality	Changes may be part of a regular set of rhythms unrelated to the measures put in place
Selection	Those selected for treatment are atypical and especially susceptible to influence
Mortality	Drop outs may be different from those staying the course, and these latter may change anyway
Interactions with selection	Selection biases may interact with other threats to internal validity, for example selection-maturation
Ambiguity about direction of causality	Apparent effects may be associated with treatments but it may be the effect causing the treatment
Diffusion or imitation of treatments	Those not treated or those areas not treated (for comparison purposes) may adopt the intervention measure themselves
Compensatory equalisation of treatments	Those not treated (and used for comparison purposes) may be given additional services to compensate for missing out on the treatment given to the target group
Compensatory rivalry by respondents receiving less desirable treatments	Those not treated (and used for comparison purposes) may work especially hard to equal or outperform the treatment group or area
Resentful demoralisation of respondents receiving less desirable treatments	Those not receiving treatments (and used for comparison purposes) may under-perform because they feel neglected and resentful

Table 2.2: Threats to external validity (Tilley, 2009)

Threat to external validity	Explanation
Place attributes	Places are never exactly the same, and the details may be important to the effects brought about
Victim attributes	Patterns of victim attributes will vary from one site to another, and the details may be important to the effects brought about
Offender / likely offender attributes	Patterns of offender/likely offender attributes will vary from one site to another, and the details may be important to the effects brought about
Intervener attributes	Who is involved in delivering the intervention, in terms of leader, front line worker, or agency will vary from site to site, and the details may be important to the effects brought about
Community / family / peer group attributes	The patterns of social relationships in which offenders and victims are embedded will vary from site to site, and the details may be important to the effects brought about
Intervention attributes	What is done can never be duplicated exactly, and the details may be important to the effects brought about
Non-crime options	Other non-crime behaviours available to those who would otherwise commit an offence will vary from site to site, and the details may be important to the effects brought about
Crime options	Different crime possibilities available to those who would otherwise commit some particular type of offence will vary from site to site, and the details may be important to the effects brought about
Dosage	Intensity of intervention in relation to target people, places or crime problems varies from site to site, and the details may be important to the effects brought about

Internal validity is prioritised by Campbell and Stanley’s seminal 1963 text, although they state that “the selection of designs strong in both types of validity is obviously our ideal” (p5). Researchers have continued to place an emphasis on internal validity (Steckler and McLeroy, 2008). Clarke (2004: 60) criticises this emphasis on internal validity to the detriment of other aspects of research:

In the typical research project, more weight is given to its internal validity and methodological rigour than to its creativity and insightfulness. Valued least of all is its relevance to solving real-life problems.

Chen and Rossi (1987) countered the view expressed by (amongst others) Donald Campbell and Lee Cronbach that a trade-off between external and internal validity is necessary. They refuted the need for sacrificing one aspect of validity to ensure the integrity of another, stating that “In many situations both internal and external validity (and other types of validity) can and should be dealt with simultaneously” (Chen and Rossi, 1987: 97).

MARYLAND SCIENTIFIC METHODS SCALE

The quality of evidence is not a static concept, and “can differ across various stakeholder groups” (Cooksy and Caracelli, 2005). One way in which the quality of evidence can be classified is according to the Maryland Scientific Methods Scale (SMS). This scale was developed by Sherman et al. (1998), building on earlier work (e.g. Gibbs, 1989 and Brounstein et al., 1997) which had attempted to classify evidence quality on numerous points. These earlier scales of evidence quality had been criticised for being complicated to understand and difficult to use. The SMS simplified everything down to five levels, each having a simple description, and is reproduced below, from least to most internal validity (reproduced from Sherman et al., 1998).

Level 1: Correlation between a crime prevention program and a measure of crime or crime risk factors at a single point in time.

Level 2: Temporal sequence between the program and the crime or risk outcome clearly observed, or the presence of a comparison group without demonstrated comparability to the treatment group.

Level 3: A comparison between two or more comparable units of analysis, one with and one without the program.

Level 4: Comparison between multiple units with and without the program, controlling for other factors, or using comparison units that evidence only minor differences.

Level 5: Random assignment and analysis of comparable units to program and comparison groups.

Evaluations at Level Five are often referred to as the ‘gold standard’ (see for example Chanhathasilpa et al., 2000; Polizzi et al., 1999) and anything below Level Three is rejected, as this is the “minimum interpretable design” which “rules out many threats to internal validity” (Farrington et al., 2002). Weisburd et al. (2001: 61) assessed the effect that the research design has on study outcomes and concluded that “the stronger the method in terms of internal validity as measured by the SMS, the less likely is a study to conclude that the intervention or treatment worked.” They argued that these findings “point to the possibility of an overall positive bias in nonrandomized criminal justice studies” (p.65), a conclusion which Eck (2003) strongly disputes. Eck contends that the way in which this conclusion was reached is a variant of the Greek liar’s paradox (“All Cretans are liars” said the Cretan), as the Weisburd et al. study did not itself have a high enough internal validity to support its own conclusion. Summarising, Eck (2003: 113) states that “a level 1 study shows that all studies

below level 5 overestimate their results”. Hope (2005) argues that the SMS is not a useful tool to assess crime prevention interventions because its use is likely to result in a Type II error, where the null hypothesis is incorrectly not rejected and the conclusion falsely states that there is no programme effect. Farrington et al. (2002) acknowledge problems with the scale, but suggest that if a more defensible system was introduced, it would be less practical and meaningful than the current SMS. They propose that a separate scale for area-based interventions may be suitable, as many good quality area-based evaluations only have measures of level 3 on the Maryland Scale. Table 2.3 summarises the different levels of evidence quality according to the SMS.

Table 2.3: Key features of experiments, quasi-experiments and non-experiments

‘TRUE’ EXPERIMENT	QUASI-EXPERIMENT	NON-EXPERIMENT
Maryland Scale level 5	Maryland Scale level 3 – 4	Maryland Scale level 1-2
Controls for everything except one thing to be changed to determine causation	Accepts lack of control in natural environment, often use statistical techniques to compensate	Focus on correlation rather than causation
Generally, 50 units or more in each group	Can be one to one group comparison	No control group
High internal validity	Medium internal validity	Low internal validity
Considered ‘gold standard’ for Campbell Collaboration systematic reviews	Acceptable for Campbell Collaboration systematic reviews	Cook and Campbell (1979) consider these designs to be “unusable”. Realists, for example Pawson, would disagree.

Although other evidence hierarchies are available and in use in other fields, they tend to follow similar rank ordering (Stevens and Abrams, 2001) with randomised controlled trials at the top. The SMS has the advantage in this context that it was designed specifically to assess the quality of crime prevention interventions.

EXPERIMENTAL DESIGNS: THE RANDOMISED CONTROLLED TRIAL

Randomised controlled trials (RCTs) are traditionally associated with medical research. However, in recent years RCTs have been increasingly used to determine what works in social interventions, including crime prevention research. Examples include research into preventing repeat sexual victimisation (Gidycz et al. 2001), the effects of police patrols on the level of crime in hot spots (Sherman and Weisburd, 1995), and research into whether reinforced glasses reduce the severity of injuries in bar fights (Warburton and Shepherd, 2000).

RCTs are designed to maximise internal validity. Participants are assigned randomly to either a treatment or a control condition. Provided there are sufficient numbers of participants, the random allocation ensures that groups can be considered to be equivalent. Selection bias is therefore removed. Randomised controlled trials rely on the manipulation of only one variable, and the equivalence of groups ensures that this variable can be held responsible for

any changes in the treatment group. The process of double blinding in RCTs, where neither participant nor researcher is aware of whether they are in the treatment or control group, is often used in medical research, but is far more difficult to replicate in social research. Many crime prevention interventions require the knowing participation of the treatment group (for example target hardening of houses to prevent burglary), it is impossible to hide (for example increased street lighting), or it relies on the mechanism of deterrence to reduce crime (for example increased police patrols).

Randomised controlled trials are often lauded as the ultimate assessment of the success of an intervention (Clarke, 2006). Donald Campbell, despite being involved in the early development of the quasi-experiment as an alternative to be used in situations where RCTs were unsuitable, believed that other methods were “subordinate to experimentally based knowledge” (Shadish and Luellen, 2004: 81).

A number of academics dispute this ‘gold standard’ status of the randomised controlled trial, contending that they are not suitable for many crime-related interventions. Tilley (2009) acknowledges that randomised controlled trials are well suited to examining whether a particular intervention (or class of interventions) works, but states that they are unsuited to the inherent complexities of assessing context-mechanism-outcome configurations in a population. Pawson and Tilley (1994: 292) state that the experimental approach “is a lousy means of expressing the nature of causality and change going on within social programmes”. Randomised controlled trials are also notoriously expensive and time intensive, particularly true for area based interventions, where the number of areas required to attain internal validity can be prohibitive (Welsh and Hoshi, 2002). In practice these features make RCTs inherently poorly suited to small scale enquiries and exploratory research. Laycock (2001) emphasises the need for timely, relevant research, and stresses the fast paced nature of changes can mean that there is a rapidly moving target to be evaluated, which perhaps suggests that alternatives to the RCT should be considered. Clarke and Cornish (1972) discuss particularly difficult problems they faced whilst conducting a randomised controlled trial in a borstal setting, which led both to the project being prematurely closed, and to the authors making a conclusion that there may be a more appropriate alternative to RCTs for institutional settings. RCTs are criticised for their rigidity in the context of social interventions, where the complex and tailored nature “makes experimental evaluation inappropriate” (Clarke, 2006: 567). There is also a risk of contamination within RCTs in that the population not in the treatment group could pick up elements of the treatment for use in their community.

RCTs also present important ethical considerations about whether it is correct to withhold crime prevention measures from a group of people. However, it has been suggested that this criticism of RCTs is largely unfounded, as unforeseen negative consequences can potentially result from crime prevention interventions, as well as positive outcomes (Tilley, 2009). Indeed, Weisburd (2003) argues strongly for a moral imperative to conduct RCTs. This is mirrored in Sherman (1992: 70) who described the impact of an ethical objection to a randomised trial of providing free milk at school (where teachers moved the more needy children into the treatment group) as acting “to undermine the case for a national policy for providing free milk at school”.

Within medicine, the use RCTs raises concern in some quarters – particularly where interventions are aimed at a population, rather than having an individual focus (Sanson-Fisher et al., 2007). This has been written about extensively in the field of community based preventive medicine (see for example Rootman et al., 2001; Jansen et al., 2010) a field which has parallels with community based crime prevention programs. Indeed, there are moves away from randomised controlled trials within complex health programmes for populations (see for example Heller and Page, 2002; Kirkwood et al., 1997; Benson and Hartz, 2000). The argument that RCTs be held as the gold standard for crime prevention research because it is lauded in medical interventions therefore perhaps seems a little outdated.

There is also a concern that many RCTs have low external validity (Rothwell, 2005). RCTs often rely on volunteers, which do not naturally match other populations. Therefore, even where a causal link is found, it does not necessarily mean that the outcome is replicable elsewhere (Henry, 2009). One famous example of this is with the Minneapolis domestic violence experiment (see Sherman, 1992). Here, spousal arrest in domestic violence cases worked to reduce victimisation. However, when this tactic was replicated elsewhere, the outcome was not. Realists argue that it is variation in the context (including the time, the place, and the people involved) which produces such discrepancies and that intervention tactics should therefore be tailored to be locally appropriate (see for example Tilley, 1993's examination of three putative replications of the Kirkholt burglary prevention project).

Caution is therefore needed when using RCTs to evaluate crime prevention initiatives. The experimental notions are in themselves sound, but the practical application of even the best designed RCTs in social contexts can be hampered by, amongst other things: well meaning practitioners; the inherent complexities of many interventions; and external validity issues.

QUASI-EXPERIMENTAL DESIGNS

Quasi-experiments differ from randomised controlled trials because of the lack of the randomisation element of the programme. This enables a quasi experiment to be used for research in natural settings where randomised controlled trials would be difficult to implement. It can be argued that quasi-experimentation is a more flexible alternative to a pure experimentalist approach. Quasi-experiments can be conducted on a smaller scale, a shorter timescale, and for less investment. Quasi-experimentation can therefore be carried out to target specific needs at a given point in time. Donald Campbell initially coined the term quasi experiments, but later Cook took on the mantle and is credited with much of the development of quasi-experimentation in social research. There is opposition to the use of quasi experiments to assess criminological and social interventions. Campbell and Stanley (1963:35) note that “experimental psychologists may look with considerable suspicion on any effort to sanction studies having less than full experimental control”. This stems from the lower internal validity, and therefore greater difficulty of inferring causation which is inherent in any design other than the RCT. However, proof of causation is not impossible to garner from quasi-experimental methods. One way in which causation may be determined is by the replication of studies. A recurring positive outcome points to the success of the intervention, rather than unrelated factors relating to the differences between two groups. Alternatively, statistical analysis can control for other factors which may account for

changes in crime rate, as long as those other factors can be predicted. Trochim (2006) advocates “increased attention to social science theory”. An understanding of the underpinning theory enables the researcher to elicit possible mechanisms by which an intervention may work, allowing any causal effects to be more easily identified.

There are a number of types of quasi experimentation. By far the most common is that of the “pre-test post-test non equivalent comparison group design” (Mark and Henry, 2006: 323). This closely mimics the randomised controlled trial, but the groups are not formed by random assignment, instead being matched on any number of characteristics. Other common approaches to the quasi experiment are the regression-discontinuity design and the interrupted time series design.

The regression-discontinuity (RD) design is used for groups which differ, so for example to prevent commercial crime, participant businesses could be split into two groups: those experiencing high levels of crime and those experiencing low levels of crime. The treatment is then given to the high crime rate businesses only, and the known selection criteria, in this case crime rates, are then used to control for selection bias. This enables an intervention to be provided to those who are in most need, which sidesteps a major ethical objection often launched at social experiments, whilst still enabling success or failure to be measured. Trochim (2006) asserts that “inferences which are drawn from a well-implemented RD design are comparable in internal validity to conclusions from randomized experiments”.

The interrupted time series design is a more complex design, which collects data at several points in time (Mark and Henry, 2006). This design presents the advantage of separating out the “real intervention effects from other long-term trends in a time series” (Glass, 1997). A comparison group is often used, which when equivalent, makes the design seem “altogether internally valid” (Campbell and Stanley, 1963: 44).

Quasi-experiments therefore present an acceptable alternative to RCTs in situations where conducting an RCT is not possible. Quasi-experimental designs also have many advantages over RCTs in social settings. Depending on the design implemented, these advantages may include: greater flexibility; more acceptable ethics (as in some designs treatment can be given to those groups most in need); lower cost; and less time intense. There are numerous designs which can be called upon to best suit the needs of the intervention under evaluation. Although the most common have been addressed above, other, less common, quasi-experimental designs include the following: “Proxy Pretest Design, Double Pretest Design, Nonequivalent Dependent Variables Design, Pattern Matching Design, and the Regression Point Displacement design” (Trochim, 2006). Importantly, although the quasi-experiment is frequently perceived as being inferior to the RCT design, Concato et al. (2000) conclude that the findings from each approach do not tend to differ:

The results of well-designed observational studies (with either a cohort or a case-control design) do not systematically overestimate the magnitude of the effects of treatment as compared with those in randomized, controlled trials on the same topic.

NON-EXPERIMENTAL DESIGNS

The term non-experimental designs is here used to describe those studies which do not have a measure for causation: those which look at correlational measures only. Non-experiments are typically any other designs not mentioned in the sections above. They are commonly associated with qualitative research, and can include for example case studies (Mark and Henry, 2006). Some simple quasi-experimental designs may also fit into this category, for example where there is a one-group pre-test post-test design. These designs are commonly used by practitioners such as the police, where there are either feasibility issues with conducting a more complex study, or where the value of a negative finding, being able to rule out an initiative as ineffective, is not appreciated (Knutsson, 2009).

Extreme care must be taken when using non-experimental evidence, as both internal and external validity are low. However, this is not to suggest that there is no value to this sort of study. They have a great deal of use as preliminary enquiries, for example, and the low-cost typically associated with this type of study makes it more feasible in some areas. Evaluations do not always have an aim of assessing outcome, and where aims include for example tracking the implementation of a particular measure, there is no reason why non-experimental measures should be dismissed. In addition, coupling non-experimental methods with theory tracking can produce an understanding of causal mechanisms by which an intervention may work. Realist evaluation is one such theory-based method of assessing program effectiveness, which “does not, cannot, and steadfastly refuses to promise simple effect-size measurements” (Tilley, 2009: 171). Such approaches to evaluation do not provide all of the information demanded by policy makers, although they do allow for theory refinement which is important for the development of future programmes. Although non-experimental designs in isolation may have limited usage, taken in conjunction with other evidence, they provide a greater body of knowledge to draw upon for assessment. Qualitative studies in particular may provide a greater depth of understanding of an evaluative issue. Eck and Madensen (2009) propose one complementary approach, a procedure they term SCEMA (Signature Characterization, Expectation, Measurement, and Analysis). This evaluation design eliminates many threats to internal validity by examining the changes in “data patterns that describe how crime is associated with various features of the [crime] opportunity structure” (Eck and Madensen, 2009: 59) and can therefore strengthen an otherwise weak design.

One promising non-experimental approach to crime prevention evaluations, that of computer simulation models, is discussed by Johnson (2009). The potential use of simulation models for evaluation purposes could serve a number of purposes. Provided the simulation was reasonably representative, artificial estimates of the counterfactual, geographic displacement and diffusion of benefits, and control of other area changes (for example new housing developments in an area) could all be developed without the need for a traditional experimental approach. Johnson (2009: 211) stresses the need for caution in this emergent approach, as all such models inevitably rely on assumptions, but concludes that “simulation methods offer researchers exciting new tools for research”.

MULTI-LAYERED APPROACHES TO EVALUATION

There are a number of academics who are firm advocates of one or other of the approaches discussed above. However, many academics do not share this outlook, and instead advocate a varied approach to evaluation. Rossi and Freeman (1985) discuss the importance of tailoring evaluations to fit the situation. Mark et al. (2000) concur, and use the phrase ‘common sense realist philosophy’ to describe their method as being dependant on the purpose to be served by the evaluation.

The underlying assumption taken of the evaluation styles discussed in this chapter has been that researchers wish to change policy and practice. This is certainly true of those working within crime prevention generally, and the focus of this thesis pivots around the notion of improvements to policy and practice. Weiss (1976) accounts for the current political landscape of decision making as having an impact on how evidence is taken up. Weiss (1976) also coined the phrase ‘decision accretion’ to describe the process by which new evidence only incrementally accumulates into the decision makers’ realm, and that evaluation can influence decisions only gradually over time.

CONSOLIDATING THE EVIDENCE

Taken alone, a single piece of evidence about an intervention has inherently limited application to other settings. The peculiarities of a context in which an intervention has been applied can have unforeseeable consequences for its efficacy. Therefore, in order to gauge whether an intervention has potential for wider application, the available evidence needs to be assimilated to enable the findings to be transferred to a broader range of situations.

The way in which this evidence should be gathered and assessed for inclusion in a meta-evaluation is a matter of some debate. Broadly speaking, there is a split between experimentalists and realists. Experimentalists favour the outcome-focused, largely quantitative systematic review, whilst realists search for greater context and mechanism-focused information in the form of a realist synthesis. The two approaches are discussed below. Later in the thesis each approach in turn is used to examine evidence on the success of repeat victimisation prevention interventions, and a third way, combining the two approaches, is suggested.

SYSTEMATIC REVIEWS

Systematic reviews are widely used state-of-the-art assessments of whether a crime prevention intervention or programme works. They are rigorous evaluations produced under strict guidelines, where transparency and replication are key qualities. In the UK, crime prevention systematic reviews are largely carried out on behalf of the Campbell Collaboration, an organisation set up with the aim of providing high quality evidence of the effectiveness of crime related interventions, with a focus on the effect size of interventions. A statistical meta-analysis often, but by no means always, forms a part of a systematic review. Petrosino et al. (2000) use the example of ‘Scared Straight’ programmes, where youngsters are shown what life in prison is like as a deterrent, to illustrate why experimental approaches to evaluating crime reduction programmes are so important. Although Scared Straight programmes were much lauded, with anecdotal evidence suggesting them to be successful, a

systematic review of the evidence (including randomised controlled trials) suggests that these programmes actually do more harm than good. Without this experimental approach to the synthesis of evidence, they argue, these harmful programmes would continue to the detriment of those involved.

Systematic reviews are traditionally associated with medical research as a meta-evaluation tool for randomised controlled trials. Shortly after the Cochrane Collaboration (C1) was established in 1993 to provide a central focus for such reviews, there was recognition that similar reviews would be useful for providing evidence of the effectiveness of social interventions. Thus, in 1999, the first meeting of what was to become the Campbell Collaboration (C2) took place. These two organisations have many shared aims, chief amongst which is to prepare, maintain and disseminate systematic reviews in each of their fields.

A systematic review collates and summarises all the available, good quality, evidence on whether or not an intervention has been successful. Not all evidence that is available is included in the final review. Each report and evaluation is assessed to ensure that there is a minimum standard of methodological quality, often using the Maryland Scale as described earlier in this chapter.

It is considered acceptable to include non randomised studies in a systematic review where: interventions either cannot be, or are unlikely to be included in a randomised controlled trial; where effects cannot be adequately studied in a randomised controlled trial (for example with rare outcomes); or where the aim is to assess the weaknesses of existing studies and examine the case for conducting a randomised controlled trial (Higgins and Green, 2008). All studies must be assessed for methodological quality, with weak designs rejected from the review - although what constitutes a weak design varies according to the body co-ordinating the review (Hansen and Rieper, 2009). The judgement on whether a non- randomised study meets the criteria for inclusion will depend on the specifics of the review being carried out. For crime prevention intervention reviews co-ordinated by the Campbell Collaboration, the reviewers are expected to justify the reasons for the minimum design features of quality (Farrington and Petrosino, 2001). Some reviews include all studies that meet a minimum of a basic pre- post- comparison group design, whilst others exclude studies on grounds including for example the timings of the pre-post measurements (for example where the post measurements take place whilst the intervention is still ongoing) or where there are questions about other methodological aspects to the evaluation which are not covered by this initial minimum criteria. The various criteria on which an evaluation was judged are outlined within the protocol and the main body of a systematic review.

Systematic reviews are planned in advance to minimise bias, and the plan written into a formally documented protocol. The methodology in Chapter Four in this thesis follows the protocol guidelines as laid out by the Campbell Collaboration. A protocol lays out the research question, search strategies, and methodology. Once the protocol has been accepted, and work on the systematic review has begun, deviations from the planned work are discouraged wherever possible. However, the need for flexibility is recognised and where modifications to the original plan are necessary, these should be “clearly documented and

justified” (Centre for Reviews and Dissemination, 2008: 4). Such changes should only result from the reviewers developing a greater understanding of the review question, and should not be influenced by findings of individual studies identified for inclusion (Centre for Reviews and Dissemination, 2008). The protocol stage sees the establishment of distinct inclusion and exclusion criteria of studies. These criteria can include many characteristics of studies, including methodological quality and relevance to the research question (which itself can vary in specificity). The search strategy must be included in the protocol, including what sources will be examined for relevant evaluations (Campbell Collaboration, 2001). Sources can include electronic databases, hand searches of journals, institutional repositories and searches through the bibliographies of identified studies. The use of grey literature, for example from government sources, and of unpublished work, is strongly supported (Higgins and Green, 2009) and the protocol lays out how such data will be identified. This is particularly important due to the common problem of publication bias, where positive results are more likely to be published than neutral or negative findings, which may result in an overestimation of the intervention effect (Centre for Reviews and Dissemination, 2008). Additionally, the focus of a systematic review is not normally restricted by country or language, although resources may sometimes dictate the ability of reviewers to tackle the latter of these. Systematic reviews are normally conducted by a team of at least two reviewers which ensures that “measures to minimize bias and error can be implemented at all stages of the review” (Centre for Reviews and Dissemination, 2008: 4). The inclusion of the search terms used is important, as this feature allows scholars to replicate the searches of the review in the future (Campbell Collaboration, 2001). One of the decisions which must be made by the reviewer is the years which the search strategy covers (Campbell Collaboration, 2001). In some cases a restriction on date would be a useful approach, for example where technological improvements have rendered older studies irrelevant due to outdated crime prevention methods. An example of where date restrictions on data may be acceptable is that of assessing the effectiveness of CCTV, where the quality of the footage gained has much improved in recent years. Here, the decision on the age of the research studies included may be dependent on the theory surrounding the mechanism by which the crime prevention mechanism may be thought to work – whether it is the fear of getting caught in the act, or the risk of being identified at a later stage that may deter potential offenders for example (with the latter being far more dependent on image quality than the former). There is therefore an argument for doing some preliminary reading to establish a theoretical base prior to commencement of the review, although reviewers may already be in a position to make this judgement call if they have previously conducted research in similar areas.

The number of studies included in a systematic review can vary widely. For example, Bennett et al. (2009) identified thirty eligible studies, whereas Davis et al. (2008) included just ten. If a search returns no suitable studies, this is still an important finding. The absence of existing research in an area can suggest directions for new research questions. Likewise, should the identified studies not meet the methodological standard required for inclusion in a systematic review, this too is a significant discovery, and weaknesses identified in those studies may enable improvements to be suggested for future research.

The evaluations identified must be systematically coded, with the same type of information drawn out of each. One way of doing this is by making use of a coding manual. This is a

document drawn up by the reviewer to answer the specific questions needed, as well as drawing out background on the intervention. A good example of this can be found in the appendix of Lipsey and Wilson (2001). The data drawn out using this coding manual is then used as the basis for summarising the studies identified, and for conducting a meta-analysis where appropriate. Statistical meta-analyses are often, but not always, included in a systematic review. A meta-analysis in this context is the statistical combination of data in order to provide an overall summary of the results of relevant studies. It usually examines the effect size(s) of each study included, and then can combine the weighted effect sizes to show the magnitude and direction of the success of an intervention type (Lipsey and Wilson, 2001). There are a number of different effect sizes that can be calculated, each dependent on the type of data available. These include, for example, the odds-ratio, risk ratio, or Cohen's d. Different effect sizes cannot be combined into one mean effect size, and so a meta-analysis can only be carried out where the findings contained within each report are similar enough in style as to be meaningfully combined (Lipsey and Wilson, 2001). In other words they must:

- a) be conceptually comparable, that is, deal with the same constructs and relationships and
- b) be configured in similar statistical forms

Lipsey and Wilson, 2001: 2

The absence of a meta-analysis does not negate the usefulness of the systematic review, but the conclusions take a more qualitative form rather than the quantitative figure produced from a meta-analysis. Tables can be used to summarise key data in an easy-to-use format, both where a meta-analysis is conducted and where a more narrative approach is taken. Whether or not a meta-analysis is included as part of a systematic review, recalculations of results from original studies may be carried out to determine for example change relative to a comparison area (see for example Bennett et al. 2009).

Table 2.4: Campbell Collaboration Crime and Justice systematic reviews available (as of Spring 2010)

Lead Author	Year	Title
Piquero	2010	Self-control interventions for children under age 10 for improving self-control and delinquency and problem behaviors
Petrosino	2010	Formal System Processing of Juveniles: Effects on Delinquency
Farrington	2009	School-Based Programs to Reduce Bullying and Victimization
Murray	2009	Effects of parental imprisonment on child antisocial behaviour and mental health: a systematic review
Killias	2009	Effects of drug substitution programs on offending among drug-addicts
Mishna	2009	Prevention and intervention of cyber abuse targeting children and adolescents: A systematic review to evaluate current approaches

Bennett	2009	The effectiveness of neighborhood watch
Welsh	2008	Effects of closed circuit television surveillance on crime
Tolan	2008	Mentoring interventions to affect juvenile delinquency and associated problems
Davis	2008	Effects of second responder programs on repeat incidents of family abuse
Weisburd	2008	The effects of problem-oriented policing on crime and disorder
Farrington	2008	Effects of improved street lighting on crime
Feder	2008	Court-mandated interventions for individuals convicted of domestic violence
Piquero	2008	Effects of early family/parent training programs on antisocial behavior and delinquency
McDougall	2008	Benefit-cost analyses of sentencing
Garrido	2007	Serious (violent and chronic) juvenile offenders: A systematic review of treatment effectiveness in secure corrections
Lipsey	2007	Effects of cognitive-behavioral programs for criminal offenders
Mazerolle	2007	Street-level drug law enforcement: A meta-analytic review
Braga	2007	The effects of hot spots policing on crime
Petrosino	2007	Scared Straight and other juvenile awareness programs for preventing juvenile delinquency
Killias	2006	The effects of custodial vs. non-custodial sentences on re-offending: A systematic review of the state of knowledge
Mitchell	2006	The effectiveness of incarceration-based drug treatment on criminal behavior
Visher	2006	Systematic review of non-custodial employment programs: Impact on recidivism rates of ex-offenders
Lum	2006	The effectiveness of counter-terrorism strategies
Wilson	2005	Effects of correctional boot camps on offending

Systematic reviews should be disseminated as widely as possible: The Centre for Reviews and Dissemination (2009: 14) recommends widespread dissemination, as “dissemination of findings is an integral part of the review process and fundamental to ensuring that the essential messages from the review reach the appropriate audiences”. A review is normally targeted at an end-user group, within crime prevention typically this may be policy makers and practitioners, and therefore dissemination should be carefully considered to allow publications and reports to specifically target these audiences (Centre for Reviews and Dissemination, 2009). To this end, the Campbell Collaboration makes all the reviews it has commissioned freely available on their website. The details of reviews that are in the development stage are also included. At the time of writing, there were 25 reviews available within the Crime and Justice strand of the Campbell Collaboration website (see Table 2.4). The aim of a systematic review is to identify, appraise and synthesise research-based evidence, to make an assessment of whether the intervention works, and to present this in an

accessible format (Higgins and Green, 2009). This involves an enormous condensation of the information available. This is one area where systematic reviews come under fire from critics of this approach, as it is seen as an oversimplification of an enormously complex area. For example, Kingdon (2010: 26) concludes that “over-simplification and exclusion of non-conforming studies can readily demonstrate limited or absence of effect”. One concern about the exclusion of evidence which does not meet rigid quality criteria is that of the ‘Martinson problem’: the danger of erroneously declaring that ‘nothing works’ (Pawson and Tilley, 1997). Another criticism of systematic reviews is the possibility of a lack of attention to the context and mechanisms involved in any social intervention (Pawson and Tilley, 1997). For a more in-depth discussion of these criticisms, see Chapter Eight, where a comparative analysis of both realist synthesis and systematic reviews is carried out.

REALIST SYNTHESIS

An alternative approach towards interpreting the evidence base in order to inform policy is that of realist synthesis, an approach developed in large part by Ray Pawson (see for example Pawson and Tilley, 1997; Pawson et al., 2004; and Pawson, 2006). This methodology takes a more holistic style than the systematic review, with the focus being on the key principles of context – mechanisms – outcomes (CMO). Context refers to the setting in which a social program is introduced; mechanisms the way in which aspects of the program are thought to work; and outcomes the success or failure of the program. “The importance of mechanism is often overlooked in criminological evaluations” (Eck, 2005: 708) and realist synthesis addresses this often missing element of evaluation. Realist synthesis looks in more detail at why programs work or not, rather than focusing on whether or not they work, as systematic reviews do. The aim here, therefore, is programme theory refinement. In practice, this means realist synthesis can inform decisions on another level, as past experiences (for example surrounding implementation failures) can be utilised to assess whether a program is suited for the current environment. The intense focus on theory means that care must be taken to ensure that the end product is useful for practitioners who will not necessarily be experts in the theoretical underpinnings of their area, but will have an interest in the applicability of findings. The realist approach to evaluation can be used formatively and summatively, but the focus is on retrospective synthesis of data.

Realist synthesis was developed from a philosophical background, as a methodological orientation towards evaluative enquiry (Pawson et al. 2004). Realist synthesis aims to be “real, realist, and realistic” (Pawson and Tilley, 1997: xiv), by which is meant that:

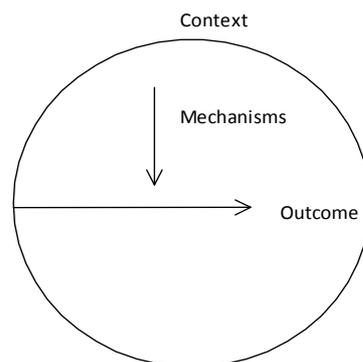
- a) the approach deals with real-world examples, rather than those supposed ideals intended when social programs are initiated;
- b) both scientific evaluation and a more central role for theory are necessary for progression;
- c) the research is applied to specific projects in specific contexts – that it informs policy makers and practitioners rather than providing a one-size-fits-all answer.

The notion of realist synthesis is to get to the crux of “what works for whom in what circumstances, and how?” (Tilley, 2009:171). This sets a different agenda to the ‘what

works?’ of systematic reviews. Pawson often uses the physical sciences example of gunpowder to illustrate the need for an understanding of context and mechanisms, rather than solely outcomes (see Pawson and Tilley, 1997; Pawson et al., 2004; Pawson, 2006). Where the gunpowder is damp, or there is no spark, the gunpowder will not ignite. The parallel with crime prevention is that the context of the programme can determine whether there is an effect. It is only through developing an understanding of these context level issues, and the mechanism by which an effect occurs, that the same effect can be replicated elsewhere – whether this is the ignition of gunpowder or the successful implementation of a crime prevention programme. If these contexts are ignored, the reasons why gunpowder fails to ignite when an experiment is replicated will not be identified – and likewise with crime prevention initiatives. This illustrates the realist view that replication does not go far enough – without an understanding of the underlying mechanisms, and the contexts which give these mechanisms the greatest chance of success, replication does not explain where and why to use particular programs – or explosives. Figure 2.1 is a visual representation of this ‘spark + gunpowder = explosion’ analogy, using a simple context-mechanism-outcome configuration.

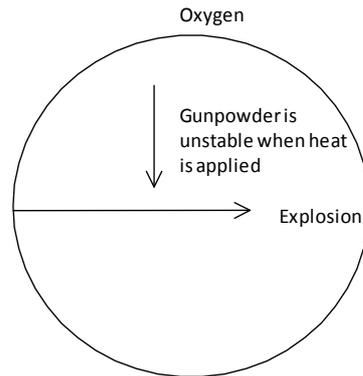
Figure 2.1: The importance of context and mechanisms: the gunpowder analogy

The context and mechanisms are at least as important as the outcome.

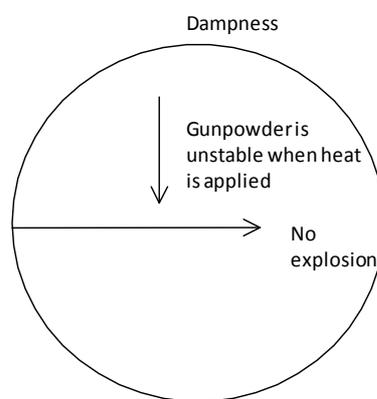


Finite resources means a focus on likely mechanisms, identified through theory, rather than any possible mechanism. Here, the chemical composition of gunpowder and its likely reaction to heat is understood.

Success depends on the context...



...and theories can be refined by failures, as well as successes



There are seven principles of realist reviews, according to Pawson et al. (2004: 4-10). These are:

1. Reviews should identify, track and evaluate the underlying theories of interventions – regardless of their explicit or implicit nature.
2. The explanation for the success or failure of interventions will often be found, at least in part, within the interactions, reasoning, and choices of those involved at all levels of the intervention.
3. Each stage of the implementation chain should be examined for weaknesses, strengths and points of contention. Intermediate outputs required for an overall success should be identified.
4. Implementation of interventions is not necessarily linear – the impact different parties have on this process should be examined.
5. Various contexts and settings can result in the ‘same’ intervention having different outcomes. The reviewer should therefore contextualise differences surrounding the individuals involved, interpersonal relationships, the institutional setting and the wider infra-structural and welfare setting.

6. Interventions evolve and mutate as they are being implemented into local circumstances, particularly due to knowledge exchange. This process should be assessed by the reviewer, and the impact on outcomes evaluated.
7. Reviews should examine both anticipated and unanticipated effects of innovation in a social programme – these could cause interventions to become self defeating or self fulfilling.

The methodology of a realist synthesis differs in some significant aspects from that of a systematic review. Whereas in a systematic review, the review question and process is laid out in advance in a protocol, in a realist synthesis, the clarification of the review question is a more iterative process. The refinement of the purpose and key theories underpinning the review can continue, sometimes to about the half way point (and sometimes beyond), and can be informed by the literature found during the search process. The literature searches rely as much on snowballing as on more traditional keyword searches. The aim is not necessarily to find every piece of relevant evidence, but rather to stop searching when no new evidence or knowledge is being added to the review – when theoretical saturation is reached (Pawson et al., 2004). In practice the two endpoints could be very similar as it is difficult to determine when theoretical saturation is reached without identifying the relevant literature.

The use of evaluation hierarchies such as the Maryland Scale of Scientific Methods is rejected by the realist approach for the purpose of assessing whether studies should be excluded from the review (Pawson, 2006). Rather, the realist synthesis looks for evidence of fitness for purpose – whether a study addresses the theory under test, and whether it consists of “a methodologically credible contribution to the test of a particular intervention theory” (Pawson et al., 2004: 22). This is an iterative process, with analysis of studies informing the need for caution about certain pieces of evidence.

The data extraction process in a realist synthesis may take the form of a more detailed version of the coding manuals used in a systematic review. However, Pawson remains rather sceptical about the utility of this model and instead suggests that note-taking, annotation, and “inevitable piles of paper on the floor” (Pawson et al., 2004: 24) form a large part of this stage of the review process. Significantly, this stage therefore becomes harder to document than a systematic review, with a complex “archaeology of decision making” (Pawson et al., 2004:24). This means that a realist synthesis could be at risk of being unable to match the transparency which is so lauded within the systematic review approach.

Realist synthesis does not aim to cover all possible aspects of an intervention (Tilley, 2009). Instead, a particular slant can be adopted. So, for example, the synthesis may examine the same theory in comparative settings; compare official expectations with the actual practice; adjudicate between rival programme theories; or question programme theory integrity. In all cases the focus is on analysing the underlying threads of theory which connect programmes and their successes, rather than merely examining individual studies. Weiss (2004) suggests that theory-based evaluations have advantages over other evaluations, with the ability to determine not just if, but also how and why an intervention works. This is essential to being able to translate successes to other environments.

The ultimate aim of a realist synthesis is that the findings are taken on board by practitioners and policymakers. To this end, similarly to systematic reviews, widespread dissemination needs to occur, but in addition to this it is a methodological requirement that consultation with end users takes place as part of the review process (Pawson et al., 2004). The findings from a realist synthesis are presented quite differently from those of a systematic review. Rather than presenting an answer to ‘what works?’ conclusions and recommendations in a realist synthesis take the form of advice, for example:

‘remember A’, ‘beware of B’, ‘take care of C’, ‘D can result in both E and F’, ‘Gs and Hs are likely to interpret I quite differently’, ‘if you try J make sure that K, L and M have also been considered’, ‘N’s effect tends to be short lived’, ‘O really has quite different components – P, Q and R’, and ‘S works perfectly well in T but poorly for U. The review will, inevitably, also reflect that ‘little is known about V, W, X, Y and Z’.

(Pawson et al., 2004: 27)

Realist reviewers must be aware that the end product needs to retain ease of use for practitioners, and as such avoid unnecessarily complex language, concepts and length of the recommendations. Realist synthesis is not designed to provide a final policy solution, but rather a theory on what works for whom, how and in what circumstances. This theory is open to further modification and refinement as new evidence emerges, reflecting the strong influence of philosopher Karl Popper and evolutionary epistemology on this methodological approach. Many academics have argued for a greater role of theory in evaluation (e.g. Chen and Rossi, 1989; Weiss, 1997) and the realist approach provides this central positioning of theory within evaluation.

THE DEBATE

These two approaches to meta-evaluation are sometimes pitted against each other by key proponents. Each is perceived as having a distinctly different ethos and set of aims (see for example Pawson and Tilley 1994; Bennett 1996; Pawson and Tilley 1996). Systematic reviews align themselves with experimentalist approaches to evaluation, wherein randomised controlled trials (RCTs) are lauded as the gold standard. Realist synthesis is imbedded in the scientific realist approach wherein ‘pragmatic methods’ of evaluation which question the utility of randomized designs are similarly lauded (Guerette, 2009:30). There has been acceptance of some elements of each approach by the other, but they appear to continue in opposition. For example, Farrington (1998) goes as far as to agree ‘that it is desirable to establish what works, for whom, in what circumstance and, hence, that it is desirable to study mechanisms and contexts’, but in 2003 expanded on this to state that his ‘conclusion on Pawson and Tilley’s challenge is that it does not require any changes in the Campbell tradition’. That is a strong statement with which scientific realists disagree. Pawson and Tilley (1998) acknowledge that there are circumstances where ‘experimental principles can be followed usefully in evaluation research’. However they have ‘expressed reservations about the over-usage of [experimentalist approaches to] evaluation’. Whilst discussing the evaluation of ‘Communities that Care’, Pawson and Tilley (1998) go further by stating that

the experimentalist design ‘would not and could not produce valid or useful findings’. In this way they suggest that realist synthesis can provide answers where systematic reviews cannot.

Despite the seeming disparate nature of the two meta-evaluative methodologies as they exist today, they share the underpinning philosophy of evolutionary epistemology. Both Donald Campbell, held up by experimentalists and systematic reviewers as an influential figure; and Karl Popper, likewise lauded and respected by realists, wrote extensively on the premise of evolutionary epistemology. This theory of knowledge has parallels with, and draws upon, the Darwinian theory of natural selection (Popper, 1984). The elimination of errors in theories is compared with the elimination of errors in attempts at adaptation. The premise of trial and elimination of error is the same in both natural selection and theory building. Popper summarised this evolution of theories using the following:

Figure 2.2: The Evolution of Theories (Popper, 1984: 396)

$$P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$$

A problem (P_1) is attempted to be solved by tentative theories (TT) which are then processed and error elimination (EE) occurs. Those errors which are eliminated give rise to new problems (P_2).

Evolutionary epistemology seems to tie the two methodologies together on a fundamental level, as each inherently aims to gradually develop and refine theories of how to solve problems. It is therefore the premise of this thesis that the two methodologies have inherent similarities and can find common ground on which to work together and merge the best aspects of each approach in order to develop a new ‘realist systematic review’.

To test this hypothesis, the present thesis examines the claims of both systematic reviews and realist syntheses, and the extent to which the two methodologies share common ground, by using each to review the same body of evidence. This leads to an assessment of whether the two can or ought to be integrated or whether they are actually incompatible. The overarching aim therefore is to determine whether the policy maker’s toolbox requires one or both approaches or an integrated version of the two (with ‘none of these’ and other approaches making up the remaining possible permutations). In Section II, both a systematic review and a realist synthesis are carried out in turn to meta-evaluate the success of programmes designed to prevent repeat victimisation (Chapter Three provides the background to the practical techniques used for such crime prevention). Section II therefore provides the basis for an assessment of the advantages of combining the two approaches, which is discussed in depth in Chapter Eight.

Chapter Three:
The Evolution of Situational Crime Prevention and the Prevention of Repeat
Victimisation

He who does not prevent a crime when he can, encourages it.

- Seneca

INTRODUCTION

This thesis seeks to provide evidence-based insight into the relative contributions of realist and experimentalist approaches to evaluation. To do this, they must be examined in detail. The second major strand of this thesis is therefore situational crime prevention and repeat victimisation which form the platform for that examination. A thorough understanding of these literatures, and the nuances of both theory and practice, is a precursor to an in-depth understanding of the evaluation techniques. Hence the present chapter details the evolution of situational crime prevention and locates the study and prevention of repeat victimisation in that context – though more details on repeat victimisation are included in later chapters.

Situational crime prevention is an opportunity based approach to crime. The focus is on ‘near’ causes of crime rather than ‘distant’ causes (Clarke, 2004). In that broad context, preventing repeat victimisation provides a way of ‘drip feeding’ crime prevention to those areas most in need (Pease, 1991, 1998). As victimisation occurs, the crime prevention initiative can be implemented to those individuals or locations, thus enabling the resources to be distributed gradually, targeted to those at greater risk. Rather than examining dispositional motivations for criminality, as much traditional criminology does, the central focus of situational crime prevention is the immediate causes of crime and how crime occurs (Clarke, 2004). Cornish and Clarke (1986) posit that the decision making process of entering a criminal career is seen as being different to the decision making process of committing an individual offence, and it is the latter component that situational crime prevention attempts to disrupt (it may also thereby, via a positive circle, disrupt the former). It is argued that by focusing on opportunities for crime, and thus making it harder for potential offenders to find a prospect for offending that is perceived as low risk, the numbers of crimes committed will naturally fall: “If there were no opportunities there would be no crimes” (Laycock, 2003:5).

The term ‘situational’ crime prevention was coined by Clarke in 1980, who presented the approach as an alternative to the ‘dispositional’ approach previously favoured by criminologists. Although this was when the term first came into usage, the background to the theory extends much further back in time. It is generally agreed (see Clarke, 1997; Clarke and Cornish, 1983) that the emergence of situational crime prevention as we know it stems from the 1960s and 1970s, although aspects of the approach have been around much longer. Arguably the driving force behind the development of situational crime prevention in the UK was that of the Home Office Research Unit (Clarke, 1997) although there was a coincidental and independent focus on opportunities for crime on both sides of the Atlantic at this time.

From this point onwards, there was an explosion of interest in situational crime prevention from governments, researchers, and practitioners (Clarke, 1997).

This chapter traces the expansion of situational crime prevention since the 1960s and provides a detailed examination of the main developments within the theoretical background which underpins the thesis as a whole. Key theories discussed include the crime pattern theory, the rational choice perspective, and the routine activity theory. Important discoveries including those surrounding patterns of victimisation, displacement of crime and what makes a product a desirable target are covered in this chapter. A selection of the most influential studies is examined, including the Kirkholt Burglary Prevention Project, and the British Gas Suicide Story. The improvement of crime prevention tools is also considered, with analysis of crime prevention through environmental design, problem oriented policing, the 25 techniques of situational crime prevention, and crime hot spots all being included in the forthcoming text. The chapter concludes with an examination of the key criticisms of situational crime prevention.

This chapter lays the theoretical groundwork for understanding the interventions examined in the main body of this thesis. The techniques, theories, and developments examined herein all contribute to the body of knowledge on which successful crime prevention initiatives are formed. The incremental way in which aspects of situational crime prevention approach have developed is reminiscent of the philosophy of evolutionary epistemology discussed in the previous chapter. Particular attention here should be drawn to the discussion of repeat victimisation and its use to identify future victims – this forms a key element of the interventions assessed in later chapters.

Table 3.1: An Overview of the Evolution of Situational Crime Prevention – Main Developments from the 1960s to the Present

Year(s)	Development(s)
60s-70s	Home Office research unit stimulated development of SCP - identified opportunity reduction as a topic worthy of further research.
1961	Jane Jacobs discussed the influence of architecture on crime
1969	Zimbardo's experiment with releaser signals on abandoned cars , precursor to Broken Windows
1969	The term CPTED coined by Jeffereys
1972	Oscar Newman 'Defensible Space' provided pragmatic ways in which architecture can influence crime, and empirical evidence
1975	Brantinghams' 'Journey to Crime'
1976	Repetto was the first to recognise that displacement is not inevitable
1976	Ziegenhagen study of 'recidivist victim'
1976	Mayhew et al. wrote Crime as Opportunity
1978	Hindelang et al. developed lifestyle/exposure model of victimisation – that the risk of victimisation is linked to lifestyle
1979	Goldstein first developed Problem Oriented Policing.
1979	Cohen and Felson developed Routine Activity Theory, similar conclusions to Hindelang et al.

1979	The notion of VIVA describing characteristics of hot products (Cohen and Felson)
1982	Wilson and Kelling, Broken Windows Theory
1982	The Brantinghams noted the concentration of crime in certain places
1985-6	Clarke and Cornish developed the rational choice perspective.
1986	Felson added the idea of a handler to routine activity theory
1986	Cornish and Clarke edit landmark volume 'The Reasoning Criminal'
1988	Clarke and Mayhew published the British Gas Suicide Story which provided evidence that displacement did not occur
1988-1990	The Kirkholt Burglary Prevention Project demonstrated that repeat victimisation could be successfully used to tackle burglary
1989	Sherman et al. identified hot spots and quantified what earlier studies had suggested
1990	Barr and Pease discussed the notion of malign and benign displacement, and add 6th category of displacement - perpetrator
1992	Publication of the first Home Office car theft index (Houghton)
1992	First Home Office circular on preventing repeat victimisation
1992	Clarke published 12 techniques of situational crime prevention
1993	First volume of <i>Crime Prevention Studies</i> published (25 volumes at time of writing)
1994	Clarke and Weisburd publish on the diffusion of crime control benefits
1997	Clarke and Homel expanded their theory to the 16 techniques of situational crime prevention
1999	CRAVED was developed as an expansion of VIVA by Clarke
2001	Opening of the Jill Dando Institute of Crime Science
2001	Wortley publishes 'situational precipitators' work
2002	Smith et al. introduced the idea of anticipatory benefits
2003	25 techniques of situational crime prevention was published by Cornish and Clarke in response to Wortley's 2001 critique
2004	Bowers et al. publish 'prospective hot-spotting', combining repeat victimisation and hot spot work as a predictive tool for SCP
2010	Addition of the notion of supercontrollers to routine activity theory

Note to table: The table seeks to give an overview of landmark events and contributions, and hence a flavour of the growth of SCP. It thereby necessarily entails some subjectivity and it cannot hope to be exhaustive with regard to academic contributions which grew exponentially from the late 1980s and early 1990s.

SITUATIONAL CRIME PREVENTION: MAIN DEVELOPMENTS

CRIME PREVENTION THROUGH ENVIRONMENTAL DESIGN (CPTED)

Crime Prevention Through Environmental Design (CPTED) concentrates on how the relationship between people and their environment can affect crime. Concepts traditionally included in this approach are those of territorial reinforcement, natural surveillance, and natural access control (Cozens, 2008). These use physical attributes of an area to (respectively): promote a sense of ownership; increase observation of potential offenders; and

to informally prevent access to potential targets. More recently CPTED has been expanded to include target hardening, space management, and activity support (Cozens, 2008). Target hardening involves the provision and upgrading of physical security measures. Space management refers to the upkeep of the physical environment, in order to promote perceptions of ownership and protection. Activity support provides specific areas where legitimate activities are encouraged, in order to increase safety.

The term ‘crime prevention through environmental design’ was originally coined by Jeffery in 1969, although his work was preceded by that of a number of academics working in a similar area, (see for example Wood, 1961; Jacobs, 1961; Angel, 1968). Perhaps the most influential work on early CPTED was that of Oscar Newman’s ‘Defensible Space’ in 1973. Newman identified problems with high crime in specific types of built environment, and built on Jane Jacobs’ ‘The Death and Life of Great American Cities’ by providing not only empirical evidence to support the argument that architecture could influence crime, but also pragmatic suggestions to resolve this situation. Newman’s work was popularised in the UK by Coleman (1985) who described 16 features of design which created disadvantages for the residents of an area. Arguably taking a more proactive stance in 1991, Poyner and Webb identified 12 requirements for crime free housing. Second generation CPTED was first discussed by Saville and Cleveland in 1997, although earlier references by other names also exist (e.g. Taylor and Harrell, 1996). This incorporates not just physical, but also social and economic changes within an area to reduce crime. Application of CPTED has had mixed results in practice (Parnaby, 2007) although it is possible that poor implementation, or putting CPTED into unsuitable contexts could be partly at fault. Cozens (2002) suggested that there are some areas which are incapable of being defended, a concept he terms ‘indefensible space’. While there is an extensive literature on CPTED and its variants, among the more significant research is that of Armitage (1999) and Armitage and Monchuck (2009). These studies of the effectiveness of Secured-By-Design in public housing (both as new build and retro-fits, with a ten-year follow-up study) found that design and security features of households and the residential environment can dramatically affect the overall crime profile of an area.

DISPLACEMENT THEORY

Displacement theory was prompted by the assumption that crime can never be prevented but instead is merely shifted, which was one of the earliest and most persistent criticisms of SCP. The idea of crime displacement has permeated the situational crime prevention movement for decades. A large number of studies in the 1970s and 1980s examined the displacement phenomenon as part of broader evaluations in response to earlier claims that crime prevention efforts were thwarted by displacement. For example, Chaiken et al. (1974) examined assertions that robberies from the New York subway were displaced to buses when police patrols were increased on trains and in stations during the peak offending hours of 8pm to 4am. They suggested that partial displacement of robberies occurred toward the buses, and when an exact fare system was introduced some years later (thus decreasing the amount of money available to the driver), a partial displacement back to the subway again occurred. Due to the long term nature of the patterns they identified, Chaiken et al. stated that “the short-term effect may not be a good measure of its overall value” (ibid. p30) and that “the crimes

may seem to disappear, but it appears more reasonable to believe that at least a portion of them are eventually displaced to unidentified targets” (ibid. p31). As an early example of evidence for displacement, note that even here there is no suggestion by the researchers that *total* crime displacement occurred. The somewhat curious notion of displacement to ‘unidentified targets’ would, in light of more recent evidence, now be more likely to be interpreted as an overall reduction in crime.

Repetto (1976) was amongst the first to attempt to categorise different types of displacement (see also Chaiken et al. 1974), although these groupings were later criticised for being too rigid (Hesseling, 1994). The five types of displacement Repetto identified were: temporal, spatial, tactical, target, and crime type. A description of each is below. It is important to note that these categories are not mutually exclusive, as for example an offender could be deflected to another place, at another time, using a different method.

Temporal: Where a crime shifts from one time to another, for example to avoid specific police or security patrols.

Spatial: Perhaps the most commonly considered type of displacement, spatial refers to the movement of crime from one area to another in response to specific crime prevention measures implemented. This is also referred to as geographic displacement.

Tactical: Offenders may use a different method to commit a crime, whilst still realising the same goals. A shift from smashing a window to gain access to a house to picking a lock when double glazing is installed is an example of tactical displacement. Also referred to as MO displacement.

Target: Offenders may shift their focus from one target to another. For example, in the 1960s it was reported that increased security on the New York subway caused a rise in the number of attacks on buses instead (Perlmutter, 1965)

Functional: This refers to offenders being deflected from one crime type to another. This type of displacement is also referred to as ‘crime type displacement’

A sixth category, *perpetrator*, was added by Barr and Pease in 1990. Perpetrator displacement refers to a criminal opportunity that is so tempting that if one offender does not take it up, another may. This is important, however, insofar as it gets to the heart of the issue: It is opportunities rather than dispositions that play the critical role in determining the overall crime rate.

That displacement was not an inevitable outcome of crime prevention initiatives was first written about by Repetto (1976). His findings were largely based on interviews with offenders rather than large-scale data, and despite a number of studies backing up Repetto’s conclusions, quantitative evidence was not generally acknowledged by mainstream academia until Clarke and Mayhew published the British Gas Suicide Story in 1988. This pivotal piece of research examined the suicide rate in Britain. In 1963, gas was used as the technique for killing oneself in more than 40% of suicide cases. Carbon monoxide was reduced in the gas

supply in Britain during a search for a cheaper form of gas, thus rendering it relatively harmless. Displacement to another suicide method was expected. However, Clarke and Mayhew instead found that the suicide rate fell dramatically, from 5713 deaths in 1963, to 3693 deaths in 1975. Similarly, suicide by gas fell from 2499 in 1960 to just 23 in 1975, matching a respective fall in the concentration of carbon monoxide in the gas supply from a little over 11% to less than 1%. This study demonstrated that even with such a deeply motivated issue as suicide, the expected displacement did not occur.

Key developments have included Barr and Pease's (1990) conceptual exploration of malign and benign displacement. Benign displacement, they contend, occurs where the end result can still be perceived as a positive one. They cite, for example, the displacement of car theft to older cars following the introduction of car steering locks on newer models. Even with the unlikely scenario of total displacement in terms of the number of crimes, the cost of older cars is less, and the cost of repairs is self limiting (due to the lower value at which they will be written off), and so they argue that this 'worst case pessimist' scenario is still a benefit to society as a whole due to the lower financial costs. Barr and Pease also discuss the related positive benefits from gun control, stating that even if all deliberate gun killings displaced to a different weapon, that accidental gun related deaths would still be reduced: "It is difficult to contend that if you do not have a gun to play with, you might kill yourself with a pool cue" (Barr and Pease, 1990: 287). In addition, their paper contends that victimisation can, at the very least, be spread more fairly across society. Malign displacement, in contrast, is an unfavourable outcome. This can occur, at least in theory, where for example burglars are displaced into committing robbery instead. However, Barr and Pease found no hard evidence for malign displacement in any studies they were aware of, although some anecdotal evidence existed.

In keeping with the review by Eck (1993) which examined over 30 studies, in 1994 Hesseling conducted a review of 55 empirical studies for evidence of displacement. In 22 of these studies, there was no evidence that displacement had occurred. In the remaining 33, there was evidence for some displacement taking place, but none of the studies he identified showed evidence for the worst case scenario of total displacement, giving credence to Barr and Pease's (1990) derogatory comments about "extreme-case pessimists". The more recent reviews, published during the preparation of this thesis, are those of Guerette (2009) and Guerette and Bowers (2009). They expanded the analysis and examined 102 studies for displacement and diffusion of benefits effects. Of the 574 observations included in these studies, 26% showed evidence for some displacement, while 27% showed evidence for a positive diffusion of benefits. They concluded that "crime displacement seems to be the exception rather than the rule, and it is sometimes more likely that diffusion of crime-control benefit will occur" (Guerette and Bowers, 2009: 1357).

THEORY OF THE DIFFUSION OF CRIME CONTROL BENEFITS

Although many scholars had acknowledged that benefits of crime prevention efforts could spread (e.g. Chaiken et al. 1974, Scherdin et al. 1986, Miethe 1991) the term 'diffusion of benefits' was first used by Clarke and Weisburd in 1994. Diffusion of benefits refers to an unintended reduction in crime caused by a crime prevention initiative – for example,

reduction may occur in nearby areas, at a different time to that targeted, or different crime types may be reduced. Clarke and Weisburd (1994: 169) define this as:

the spread of the beneficial influence of an intervention beyond the places which are directly targeted, the individuals who are the subject of control, the crimes which are the focus of intervention or the time periods in which an intervention is brought.

In particular, this phenomenon occurs where criminals cannot be sure of the extent of the increased risks, for example where publicity is used appropriately. Publicity can deter criminals by several mechanisms, including informing them of the increased risks they face, and create uncertainty as to the exact times, locations and tactics that the crime prevention initiative is in place – therefore increasing the breadth of the effect of the initiative (Bowers and Johnson, 2005). Publicity is more effective where it is targeted geographically and to a specific crime, as this increases the relevance of the information to the audience (Johnson and Bowers, 2003a).

Anticipatory benefits can also occur, where the crime reduction occurs before the start of the programme. Smith et al. (2002: 79) suggest that this can occur for a number of reasons, but focus on specific publicity and disinformation effects “whereby covert measures are presumed to exist as a result of publicity or hearsay”. This anticipatory effect has been observed in a number of interventions, for example in the Reducing Burglary Initiative (Johnson and Bowers, 2003b).

The evidence presented above suggests that displacement can be avoided, and by no means is it always undesirable. Where unwanted displacement does occur, there is normally a net gain, an overall reduction in crime. Moreover, not only is displacement not the problem once thought, but it is now known that crime prevention efforts can produce benefits that extend beyond their intended sphere of influence. These are significant advances in criminological knowledge. As such, it might be expected that it is now common practice in evaluations to attempt to identify any displacement or diffusion resulting from an intervention. This may be somewhat true for much academic research, but appears less true for that generated by practitioners, about which it was recently concluded that “the majority of problem-oriented policing projects fail to investigate displacement” (Guerette, 2009).

REPEAT VICTIMISATION

The Home Office defines repeat victimisation as taking place “...when the same person or place suffers from more than one incident over a specified period of time” (Bridgeman and Sampson, 1994: 2). It is now widely accepted that past victimisation is one of the best predictors of future victimisation (Sagovsky and Johnson, 2007). The concept of repeat victimisation is therefore one of the most important tools we have in our situational crime prevention arsenal – the ability to predict where a crime is likely to happen, and therefore where the most effective pinch point is for (usually limited) crime prevention resources.

One of the earliest studies of repeat victimisation was by Johnson et al. (1973). They examined hospital records for gunshot and stab wounds, and found that some patients “were

admitted not once, but several times for treatment resulting from an act of violence” (Johnson et al. 1973: abstract). Ziegenhagen (1976) followed this up with a survey of 268 victims of violent crime in New York City, and discovered that 30% reported being victims previously within the last 5 years. Ziegenhagen then continued on to identify specific characteristics particular to recidivist victims (as opposed to the one-time victims also surveyed) and concluded that they can be defined as a distinct group with specific needs, though acknowledged that his results were not generalisable due to the small sample. Larger scale studies of repeat victims then followed. Sparks et al. (1977, 1981) conducted a survey in London, and Hindelang et al. (1978) used American crime survey data to find that victimisation was indeed uneven across the population. Gottfredson (1984) showed that similar patterns were also found in Britain by identifying the high rates of repeat victimisations in the British Crime Survey, a now long-running victim survey which complements police recorded data.

In the late 1980s, repeat victimisation was used as a way of targeting crime prevention by the Kirkholt burglary prevention project which influenced many future projects, including Biting Back and the HomeSafe programme. These projects are explored in more detail later but the pivotal and catalytic role of Kirkholt means it warrants discussion here. Kirkholt was a very successful burglary prevention project on a public housing estate in the UK. Households that had been burgled were offered security upgrades; coin payment meters (which were often the target of break-ins) were removed; and immediate neighbours of victims were encouraged to form a Cocoon Watch to increase guardianship. Despite critics suggesting that general estate improvements may have instead been responsible for the dramatic fall in crime during the project (Safe Neighbourhoods Unit, 1993), later analysis definitively linked the falls with the interventions put into place by the project itself (Farrington, 1992). The Kirkholt study is widely cited in crime prevention literature, and is assessed in Chapters Five and Seven of this thesis.

Many further developments in repeat victimisation research have occurred since the early 1990s. Polvi et al. identified that not only does repeat victimisation occur, but that the peak risk of the offence recurring is within a close time of the original offence – usually about a week (Polvi et al., 1990). Evidence about the extent of repeat victimisation was pulled together, including new evidence from the British Crime Survey, by Farrell and Pease in 1993. This demonstrated conclusively that repeat victimisation is not restricted to burglary victims, but rather is a phenomenon that extends across the breadth of crime types. In 2004 this was extended by Moitra and Konda to show that repeat victimisation also occurs in cybercrime (Farrell and Pease, 2008)

Two different explanations for the occurrence of repeat victimisation have been hypothesised: event dependence, and state heterogeneity. The event dependence model suggests that one victimisation ‘boosts’ the chance of a further victimisation, for example where a burglar returns to a house to retrieve valuable items they did not manage to take during the first burglary. In contrast, the state heterogeneity model suggests that characteristics of likely victims are ‘flagged’ by their repeated victimisation, for example repeated burglary may point to poor security and attractive belongings. These theories are also known as the boost and flag models respectively. In 1995 Launtsen and Davis Quinet

used a victimisation survey to show that both the boost and flags explanations for repeat victimisation were contributory factors. Further studies have been conducted which have both confirmed and refined this finding (Tseloni and Pease, 2003).

By the mid 1990s, the role of repeat victimisation in crime prediction and prevention had become firmly established as an important tool in the UK, and was emerging elsewhere. From the mid to late 1990s, work began to roll out repeat victimisation as a key performance indicator for the police (Laycock 2001:71-2).

A number of variations on repeat victimisation have been identified in recent years. The hot dot is a simple spatial repeat, more details on which are provided in the crime pattern theory section of this chapter. Pease (1998) coined the term *virtual* repeat victimisation. This refers to targets selected because they are perceived to be similar or identical to other targets where the offender had success - for example the same make and model of car. Townsley (2000) discussed the concept of 'near repeats', where the victim is not the same, but there is a combination of spatial and tactical repeat victimisation, for example when the neighbour of a burglary victim is also burgled. This risk can be calculated using techniques developed in the field of epidemiology, as the risk of victimisation can be treated as communicable (Bowers and Johnson, 2004). The concept of the offender as forager can be employed to explain the phenomenon of near repeats. Linked to the boost explanation of repeat victimisation, this posits that offenders, like nature's foragers, aim "to maximise resources acquired, whilst minimising search time and the risk of exposure" (Johnson, 2010: 14). Targets close to each other are likely to share characteristics which become familiar to the offender: "homes proximate to each other are likely to share architectural features, levels of natural surveillance and occupant affluence and routine activities" (Johnson, 2010: 15) and therefore are attractive to offenders as they can be more confident about the potential risks and rewards.

There have been difficulties in gauging the true extent of repeat victimisation. Measurements can be affected by the 'time window effect' (Farrell et al., 2002). This refers to the greater likelihood of the mis-measurement of a criminal event as a single incident (rather than a repeat) when the time period examined is small. A solution used by Tilley (1993, see also Weisel et al. 1999) was to have a rolling window, so that the same period of time (ideally 12 months to account for seasonal variations) is measured after each seemingly new victimisation. A recent development in the measurement of repeat victimisation came from Farrell and Pease in 2007. They identified that due to a cap of 5 victimisations recorded for each victim, that chronically victimised targets (with more than 5 victimisations each in the 12 months surveyed) were being undercounted. They concluded that this has resulted in around 40% of personal crimes being excluded from estimates, and around 20% of household crimes (Farrell and Pease, 2007). Their work on the British Crime Survey largely mirrored findings for the National Crime Victimization Survey by Planty and Strom (2007). This suggests even greater chronic victimisation than has previously been acknowledged, which has implementation implications for the future focus of crime prevention efforts.

Repeat victimisation is a critical component of this thesis. Identifying where victimisation can occur is an important step on the road to crime prevention. Although repeat victimisation can theoretically be applied to identify higher risks for people, places and businesses for any

crime type, the bulk of the prevention research exists in domestic burglary interventions. The success of using repeat victimisation as a means to target crime prevention resources varies considerably, and this thesis examines this varying success and the reasons behind this, with an eye towards the use for policymakers and practitioners.

CRIME OPPORTUNITY THEORY

The idea that criminal activity is linked to opportunity was first introduced into mainstream criminological thinking in 1976 with the publication of Mayhew et al.'s seminal paper 'Crime as Opportunity'. This paper laid out how opportunities (and therefore crime) could increase with an abundance of targets, for example when there was an increase in cars on the road, or more recently the growth of the mobile phone industry; be more likely when there was poor physical security; and finally examined how opportunities could decrease when surveillance was increased.

Crime opportunity theory has been further refined over the years, culminating in the 10 principles of crime opportunity theory as laid out by Felson and Clarke (1998, see below). These ten principles are deceptively simple, and very neatly summarise a vast swathe of experience and evidence from across the situational crime prevention field.

1. Opportunities play a role in causing all crime
2. Crime opportunities are highly specific
3. Crime opportunities are concentrated in time and space
4. Crime opportunities depend on everyday movements
5. One crime produces opportunities for another
6. Some products offer more tempting crime opportunities
7. Social and technological changes produce new crime opportunities
8. Opportunities for crime can be reduced
9. Reducing opportunities does not usually displace crime
10. Focused opportunity reduction can produce wider declines in crime

PROBLEM ORIENTED POLICING

Problem oriented policing was devised by Herman Goldstein in 1979, responsible for many important developments within the approach as well as its initiation (see for example Tilley, 2008; Scott et al., 2008). Problem oriented policing revolves around the examination of underlying problems, rather than relying on a reactive style of policing (Knutsson, 2010). The

approach does not just restrict itself to criminal problems, but tackles other social problems, utilising partnership work to address issues outside the remit of the police (Tilley, 2008).

The problem oriented approach makes use of a variety of situational crime prevention measures, including the problem analysis triangle, the 25 techniques of situational crime prevention, and crime prevention through environmental design (Scott et al., 2008). Further details on each of these can be found in the relevant sections of this chapter. Significantly, in the mid 1980s, the process of SARA was developed in Newport News, Virginia (Eck and Spelman, 1987). This is the pivotal idea within problem oriented policing, and lays out the steps by which problems should be tackled. SARA stands for: scanning; analysis; response; and assessment; and a summary of the key elements within each is presented below (adapted from Center for Problem Oriented Policing, 2010).

Scanning: Includes identification of the problem and its consequences, examining them closely and prioritising them, and establishing broad goals.

Analysis: Develops an understanding of the problem by collecting data, examining the problem in depth, narrowing the focus and looking at the success of current responses.

Response: Development of new interventions, and identification of those used elsewhere, with the aim of developing and implementing a response plan with specific objectives.

Assessment: Examination of the implementation, and collection of pre-post data, followed by determining whether the objectives were met and potential improvements. This should be an ongoing process.

More recently, the Center for Problem Oriented Policing developed a website (www.popcenter.org) which provides free access to the latest relevant research, case studies, and the theoretical background which underpins various aspects of the approach. Also available on the website are problem specific guides, a frequently updated set of publications aimed at police to guide them through how to approach different crime types and social problems they may face. This ensures that this approach to policing is widely accessible and thus able to be implemented across not just the United States, but also worldwide. Problem oriented policing is currently in widespread use and development in Canada, United Kingdom, United States of America, Scandinavia, Australia and New Zealand (Center for Problem Oriented Policing, 2009).

ROUTINE ACTIVITY THEORY

In its most basic form, routine activity theory states that a crime occurs when there is the convergence of three elements: lack of a capable guardian, a motivated offender, and a suitable target. This theory was first developed by Cohen and Felson in 1979 who examined the changes in behaviour after the war which resulted in an increased contact with offenders, and more opportunities for crime. For example, at this time women first began to routinely go out to work, with their pathways to work providing more opportunities to bump into potential

offenders, and thus more chance of an offence taking place. This was reflected in the increased crime rate of the time.

The lifestyle model of victimisation was developed at approximately the same time as routine activity theory, by Hindelang, Garofalo and Gottfredson. Though developed independently, their empirical data led them to draw similar conclusions as Cohen and Felson: that the risk of personal victimisation was dependent on lifestyle (routine daily activities) due to the exposure and associations which that lifestyle resulted in (Garofalo, 1986). As Hindelang et al. (1978: 251) put it:

Because different lifestyles imply different probabilities that individuals will be in different places, at particular times, under particular circumstances, interacting with particular kinds of persons, lifestyle affects the probability of victimization.

In their original formulations, lifestyle theory was proposed to explain variation in victimization rates across people and places at any point in time. In contrast, routine activity theory was proposed to explain crime rate trends, that is, variation in crime over time. The theories are compatible and certainly overlapping, differing in the emphasis of particular variables. However, it is perhaps routine activity theory's identification of the overarching mechanism by which crime occurs – the interaction of suitable offenders and targets and levels of guardianship – that appear to have made it the more widely applied and extended of the two, with the concept of 'routines' often incorporating lifestyles.

Further additions to routine activity theory were made by Felson in 2002. He refers to the supplementary elements of a crime, being props, camouflage and audience. Alongside the absence of a capable guardian, a suitable target, and a likely offender, these are the ingredients for "an ideal crime setting" (Felson, 2002). Props can include weapons or tools that are accessible to the offender; camouflage refers to anything that can help hide the offender; and an audience is sometimes desired either so they can be impressed or intimidated.



Figure 3.1: The Problem Analysis Triangle Clarke and Eck, 2005

The Problem Analysis Triangle is a visual representation of routine activity theory. It incorporates the later additions by Felson (1986) of the 'handler' and Eck (1994, cited in Felson, 1995) of the 'place manager'. The inner triangle is representative of the traditional convergence of the three elements needed to result in a crime. The outer triangle represents those elements which can prevent a crime. The handler acts on the offender, and can be for example a parent, probation officer, or peer – anyone who may prevent the offender from deciding to commit an offence. The guardian does not have to be a person, but may include for example CCTV or a dog, although of course guardianship also extends to other people, such as friends and neighbours. The capable guardian is anyone or anything that offers the target or victim protection from crime, or who can mobilise more formal guardianship. Hence in many instances nowadays, a guardian may be anyone with a mobile phone – phones having increased the capability of potential guardians. It is possible for a guardian to be incapable and thus not offer protection to the target, for example if the offender knows that an area is overlooked by grainy unmonitored CCTV footage, there is not an effective deterrent in place. Note also that the target may be an item, rather than an individual. Finally, there are place managers which provide protection to a place, for example bar staff or security patrols. The problem analysis triangle therefore is designed to demonstrate the practical application of routine activity theory by illustrating on a basic level where the crime opportunity can be disrupted. Its simplicity belies the complexities involved both within the underlying theory, and within the practical applications.

Sampson et al. (2010) have further developed the problem analysis triangle to incorporate the notion of super controllers. Super controllers are “the people, organizations and institutions that create the incentives for controllers to prevent or facilitate crime” (Sampson et al, 2010: 40). Examples of super controllers include families, peer groups, media, courts and government bodies.

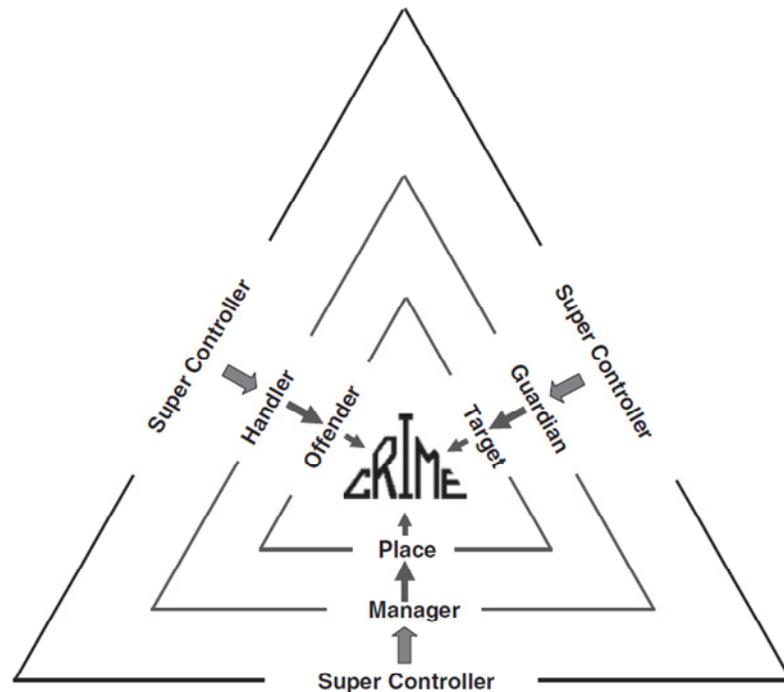


Figure 3.2: Super controllers and crime (Sampson, Eck and Dunham, 2010)

HOT PRODUCTS

Hot products are those targets which prove to be particularly attractive to criminals. By understanding which characteristics create a hot product, it is possible not only to explain patterns in crime, but also to provide a focus for crime prevention efforts, and even opportunities to design out crime before it occurs.

Cohen and Felson (1979) came up with the acronym VIVA to describe the characteristics of hot products: value, inertia, visibility and accessibility. According to Clarke (1999:32), VIVA was designed to be applicable to human targets of crime, not just inanimate objects, and this collection of characteristics “was clearly not meant to be definitive”. Today, VIVA has largely been superseded by Clarke’s (1999) acronym CRAVED, which stands for the characteristics: concealable, removable, available, valuable, enjoyable, and disposable. Unlike VIVA, CRAVED is designed to be specifically applicable to theft, rather than any target of predatory crime. Clarke (1999) suggests that displacement is unlikely to occur to a significant extent when focusing crime prevention efforts on hot products – they are ‘hot’ for a reason, and other targets are by definition less desirable.

CRAVED can be used as a reference point by practitioners of ‘designing out crime’. Ekblom (2008) discusses four broad ways in which hot products can be secured against crime. Firstly, “designing inherently secure products”, including for example password protection, camouflage of goods, physical resistance to damage (e.g. laminated glass), or security features which make documents difficult to copy. Secondly “securing the immediate situation in which products are at risk”, for example safes, controlling access to rooms, or providing

reminders or influences to make the product secure (e.g. beep to remind a driver to take the car keys from the ignition). Thirdly, “adding on security products” such as steering wheel locks, or cables to secure a laptop to a desk can be used to prevent theft of hot products. Finally, products can be secured “by remote interventions”, for example registering property or limiting knowledge of where products can be found.

BROKEN WINDOWS

The Broken Windows theory (Wilson and Kelling, 1982) is a broad theory encapsulated in the metonym of a broken window and the idea that if one window is broken in a neighbourhood, and is not fixed, it acts as a signal that no-one cares, and therefore other windows will soon be broken. Wilson and Kelling suggest that when low level anti social behaviour is ignored in a neighbourhood, it can leave the door open for more serious criminal acts to creep in, due to a breakdown in community controls. Therefore, by clamping down on low level offences, it should follow that there is never the opportunity for higher order offences to become established in an area.

The Broken Windows theory (as it relates to crime prevention – Wilson and Kelling state that the phenomenon had been recognised by psychologists much earlier) was inspired by a 1969 experiment by Zimbardo. He deliberately placed two cars with “releaser signals” (i.e. open hoods and no licence plates) in areas with different reputations. In the New York case, the car began to be stripped within 10 minutes of it being left: “In less than three days what remained was a battered, useless hulk of metal, the result of 23 incidents of destructive contact” (Zimbardo, 1969 [1973]: 223). However, in Palo Alto, California, there was no such occurrence, even when it was moved onto a university campus. Zimbardo then recruited two of his students to begin the destruction of the car (as a more extreme releaser signal). Before long, other students had joined in, with many more cheering them on. Zimbardo (1969[1973]: 224) concluded that to initiate such destructive vandalism there needed to be either “acquired feelings of anonymity [and] minimal releaser cues”, as in New York, or “more extreme releaser cues...and physical anonymity” (ibid.) as in California. In other words, though few will initiate such behaviour, given the right signals many people will join in with the action: even those for whom wanton vandalism is not the norm.

Zero tolerance policing in New York was heavily influenced by the Broken Windows theory (Mawby, 2008). Initially implemented on the subway, the election of Mayor Guiliani in 1993 saw this approach becoming prevalent across New York. Crime rates in New York fell dramatically, with proponents of the broken windows theory rapidly claiming success (Kelling and Sousa, 2001). However, the fall in crime was not unique to New York, and was seen in many other cities of the United States of America in the 1990s, both those with and without zero tolerance approaches to policing (Levitt, 2004). A recalculation of the New York crime drop by Corman and Mocan (2005) suggested that the broken windows approach to policing accounted for some, but by no means all of the fall in crime. Other countries have also experienced recent decreases in crime rates, and criminologists are yet to come up with a convincing explanation, although current research on the crime drop shows significant promise (see for example Farrell et al., 2008).

One opponent to the Broken Windows theory is Harcourt, who in recent years has published a number of texts demonstrating the evidence against the effectiveness of following the Broken Windows theory (e.g. Harcourt, 2001; Harcourt and Ludwig, 2005; Harcourt and Ludwig, 2007). Harcourt and Ludwig (2007) cite what they term as Newton's Law of Crime – and what is commonly known as regression to the mean – that what goes up must come down. In other words, those areas that had an abnormally high crime rate within New York had further to fall, so that although broken windows policing was implemented most heavily in those areas, these were the areas that would naturally have seen a fall in crime (towards the mean) over a cycle of a number of years in any case. The jury therefore remains out on the efficacy of broken windows in practice.

CRIME PATTERN THEORY

Patterns in crime have long been acknowledged. However, the importance of these patterns was dismissed by early criminologists, who were more interested in the motivation of criminals rather than distribution of crime events (Brantingham and Brantingham, 1981). Crime Pattern Theory was developed by Brantingham and Brantingham to redress this balance. The theory describes how crime is clustered in dynamic patterns rather than occurring randomly or uniformly, and can be understood on a detailed or a general level (Brantingham and Brantingham, 2008). Much of the theory centres on the routine activities of offenders and victims which create opportunities for crime. Most people, the theory contends, have a set of primary 'nodes' (for example home, work, shopping and entertainment) and tend to take the same routes between them – these are the 'activity spaces', or paths. "Crimes are likely to cluster around these activity spaces" (Brantingham and Brantingham, 2008). There are also 'edges', which are at the boundaries of where people have their normal activities – some crimes are more likely to occur here. Crime Pattern Theory therefore pays a lot of attention to "the geographical distribution of crime and the daily rhythm of activity" (Felson and Clarke, 1998) to identify hot spots for crime, and generate crime maps for potential criminal activity.

Hot spots were first identified by Sherman et al. (1989). This pivotal study backed up the routine activity theory with spatial data for the first time, and showed that 50% of calls to police in Minneapolis were generated in 3% of the places (Sherman et al. 1989, Sherman, 1995), the so-called crime hot spot, where there is a disproportionate risk of criminal victimisation. Since then, hot spots have become an important tool in locating crime prevention initiatives. Crime mapping allows crime patterns, such as hot spots, to be visually represented in user friendly packages – although police may have a rough knowledge of high crime areas, the use of crime maps allows pinpoint accuracy (Anselin et al., 2008). A number of statistical techniques are used to analyse crime patterns to assess the significance of crime clusters.

RATIONAL CHOICE PERSPECTIVE

The rational choice perspective (also known as rational choice theory) was brought together coherently for the first time by Derek Cornish and Ron Clarke in the 1980s, although some elements had existed prior to this. According to Cornish and Clarke (2008) the core concepts

of rational choice theory are: that criminal behaviour is purposive and rational; criminal decision making is crime-specific; criminal choices fall into two broad categories – involvement and event decisions; there are separate stages of involvement in crime; and that events unfold in a sequence of stages and decisions.

Rational choice theory draws on Simon's concept of bounded rationality, which takes into account cognitive limitations (see for example Simon, 1957; Simon, 1991). In other words, rational choice theory does not assume that criminals have perfect rationality – nor in all cases do they make a conscious choice to commit a crime, but a series of decisions often made impulsively or quickly, and often without all the salient facts. It is important to note that rational choice theory does not reject the relevance of background factors to criminal involvement. Rather it sees the process of becoming involved with crime in general as being completely separate from each criminal event decision process (Cornish and Clarke, 1986). Hirschi (1986) sees rational choice theory as dealing solely with the criminal event, and therefore completely compatible with his control theory, which deals with the criminal motivation for crime. Cornish and Clarke (1986) agree that the two theories are compatible, although they stress that rational choice can account for criminal motivation too.

Although often criticised for being suited only to economic crimes (Hayward, 2008), rational choice theory actually applies across a much broader range of crime types, with work done in such diverse areas as opioid addiction (Bennett, 1986), joyriding, graffiti and child molestation (for a discussion see Farrell, 2010).

Recent discussion has included that of situational precipitators, which are those “cues which prompt criminal behaviour” (Wortley, 2001:65). Wortley (2001) incorporated these into a critique of the 16 techniques of situational crime prevention, prompting the redevelopment into the current 25 techniques (see below). Situational precipitators are seen by Wortley as complementary to rational choice theory. Wortley (2008:51) suggests that “immediate environments might precipitate criminal responses” through: prompts; pressures; permissions; and provocations. In this model, prompts are situational factors that can trigger a criminal response, provide signals of or models for appropriate behaviour, or expectancies about criminal activity (see for example Broken Windows Theory, above). Situational pressures include those of conformity, obedience towards authority figures, or feelings of anonymity. Permissions occur where the situation distorts “moral reasoning processes” (Wortley 2008:55), such as where rules are not clear, where there is shared blame, or where the offender is ignorant of the harm caused. Finally in this model, provocations are stressful situations which can provoke an anti-social response. According to Wortley, therefore, situational precipitators form the stage before rational choice theory comes into play, and act to explain why normally law abiding people may sometimes commit a crime.

CAR THEFT INDEX

The car theft index was developed within the Home Office as an attempt to encourage the motor industry to design more secure vehicles (Laycock, 2004). The index took the form of a league table, first published in 1992, which ranked the cars in terms of the percentage of thefts of each model (see Houghton, 1992). The publicity involved in being ranked

unfavourably had the potential to impede future car sales, so the leverage provided by the car theft index encouraged manufacturers to improve their security features (Laycock, 2003). The car theft index is now an annual publication. Replication of this index within the mobile phone market has been attempted. Theft data for mobile phones was available but the manufacturers would not release sensitive data on the number of sales to the researchers (Mailley et al., 2008). To overcome this, the second version of the phone theft index used an alternate indicator of the prevalence of phone models in the population (Farrell and Mailley, 2007). The car theft index is of particular importance because it recognises that individuals can only take *responsibility* for their own risk of crime when provided with the necessary tools by those agencies that have the *competency* to do so (Laycock, 2003).

THE 12/16/25 TECHNIQUES OF SITUATIONAL CRIME PREVENTION

The 25 techniques of situational crime prevention refers to the current form of a grid of crime prevention measures which has been evolving from the early 1990s onwards. The table provides an easy reference to a number of ways in which crime can be prevented within five categories: increase the effort, increase the risks, reduce the rewards, reduce provocations, and remove excuses. Initially there were 12 techniques of situational crime prevention in the grid (Clarke, 1993) then it was extended to 16 by Clarke and Homel (1997). The current 25 techniques were developed by Cornish and Clarke (2003) in response to a critique by Wortley (2001). Although a generic table, with a small number of examples provided, the 25 techniques can, within reason, be adapted to any situation, with any crime type. For example, Newman and Clarke (2004) adapted the (then 16) techniques to reducing opportunities for e-commerce crime. The 25 techniques of situational crime prevention have been reproduced here for ease of reference, as in the main body of the thesis it provides the conceptual backdrop for the discussion of the preventive mechanisms utilised in many of the evaluations of efforts to prevent repeat victimisation.

Figure 3.3: The 25 Techniques of Situational Crime Prevention (Cornish and Clarke, 2003 [2010])

TWENTY FIVE TECHNIQUES OF SITUATIONAL PREVENTION				
Increase the Effort	Increase the Risks	Reduce the Rewards	Reduce Provocations	Remove Excuses
1. Target harden <ul style="list-style-type: none"> ▪ Steering column locks and immobilisers ▪ Anti-robbery screens ▪ Tamper-proof packaging 	6. Extend guardianship <ul style="list-style-type: none"> ▪ Take routine precautions: go out in group at night, leave signs of occupancy, carry phone ▪ “Cocoon” neighborhood watch 	11. Conceal targets <ul style="list-style-type: none"> ▪ Off-street parking ▪ Gender-neutral phone directories ▪ Unmarked bullion trucks 	16. Reduce frustrations and stress <ul style="list-style-type: none"> ▪ Efficient queues and polite service ▪ Expanded seating ▪ Soothing music/muted lights 	21. Set rules <ul style="list-style-type: none"> ▪ Rental agreements ▪ Harassment codes ▪ Hotel registration
2. Control access to facilities <ul style="list-style-type: none"> ▪ Entry phones ▪ Electronic card access ▪ Baggage screening 	7. Assist natural surveillance <ul style="list-style-type: none"> ▪ Improved street lighting ▪ Defensible space design ▪ Support whistleblowers 	12. Remove targets <ul style="list-style-type: none"> ▪ Removable car radio ▪ Women’s refuges ▪ Pre-paid cards for pay phones 	17. Avoid disputes <ul style="list-style-type: none"> ▪ Separate enclosures for rival soccer fans ▪ Reduce crowding in pubs ▪ Fixed cab fares 	22. Post instructions <ul style="list-style-type: none"> ▪ “No Parking” ▪ “Private Property” ▪ “Extinguish camp fires”
3. Screen exits <ul style="list-style-type: none"> • Ticket needed for exit ▪ Export documents ▪ Electronic merchandise tags 	8. Reduce anonymity <ul style="list-style-type: none"> ▪ Taxi driver IDs ▪ “How’s my driving?” decals ▪ School uniforms 	13. Identify property <ul style="list-style-type: none"> ▪ Property marking ▪ Vehicle licensing and parts marking ▪ Cattle branding 	18. Reduce emotional arousal <ul style="list-style-type: none"> ▪ Controls on violent pornography ▪ Enforce good behavior on soccer field ▪ Prohibit racial slurs 	23. Alert conscience <ul style="list-style-type: none"> ▪ Roadside speed display boards ▪ Signatures for customs declarations ▪ “Shoplifting is stealing”
4. Deflect offenders <ul style="list-style-type: none"> ▪ Street closures ▪ Separate bathrooms for women ▪ Disperse pubs 	9. Utilize place managers <ul style="list-style-type: none"> ▪ CCTV for double-deck buses ▪ Two clerks for convenience stores ▪ Reward vigilance 	14. Disrupt markets <ul style="list-style-type: none"> ▪ Monitor pawn shops ▪ Controls on classified ads. ▪ License street vendors 	19. Neutralize peer pressure <ul style="list-style-type: none"> ▪ “Idiots drink and drive” ▪ “It’s OK to say No” ▪ Disperse troublemakers at school 	24. Assist compliance <ul style="list-style-type: none"> ▪ Easy library checkout ▪ Public lavatories ▪ Litter bins
5. Control tools/ weapons <ul style="list-style-type: none"> ▪ “Smart” guns ▪ Disabling stolen cell phones ▪ Restrict spray paint sales to juveniles 	10. Strengthen formal surveillance <ul style="list-style-type: none"> ▪ Red light cameras ▪ Burglar alarms ▪ Security guards 	15. Deny benefits <ul style="list-style-type: none"> ▪ Ink merchandise tags ▪ Graffiti cleaning ▪ Speed humps 	20. Discourage imitation <ul style="list-style-type: none"> ▪ Rapid repair of vandalism ▪ V-chips in TVs ▪ Censor details of modus operandi 	25. Control drugs and alcohol <ul style="list-style-type: none"> ▪ Breathalyzers in pubs ▪ Server intervention ▪ Alcohol-free events

CRIME SCIENCE: THE NEXT STEP?

Seen by many prominent academics as the future of criminology (e.g. Laycock, 2003; Clarke, 2004; Pease, 2008; and the collection in Smith and Tilley, 2005), crime science is a relatively recent development. In 2001, the Jill Dando Institute opened at University College London, and is the first Institute in the world to be dedicated to preventing crime. Crime science takes situational crime prevention to the next level. Like situational crime prevention, crime science has an emphasis on the 'applied' understanding of crime, rather than the 'pure' understanding of criminals (Clarke, 2004). However, crime science strives for an even greater use of scientific techniques and methodologies, and is a problem-led approach drawing on multi-disciplinary expertise. In this way, crime science steps outside the realms of social sciences by embracing links with biologists, geographers, town planners, economists, chemists and engineers – to name but a few. Pease (2008) suggested traditional discipline boundaries may hamper progress in crime prevention. By taking a multidisciplinary approach and using the criminologist as a conduit for specialist skills and knowledge from across the entire academic community, crime science avoids this limitation.

CRITICISMS OF SITUATIONAL CRIME PREVENTION

It is perhaps inevitable that any growing area of academia will have its critics, and situational crime prevention is no exception. Some of the criticisms have been addressed above, where they are specific to a niche area of situational crime prevention. There are enough criticisms of the whole approach that a separate section is warranted in order that these can be discussed, and rebutted where appropriate.

SITUATIONAL CRIME PREVENTION IGNORES THE ROOT CAUSES OF CRIME

Hayward (2007: 234) characterised the use of rational choice theory in crime prevention as being reduced to an equation, where "the human purposes and existential meanings of crime are thus literally banned". However, situational crime prevention actually sees crime as being the outcome of the combination of disposition and of opportunity. Clarke (2005:42) proposes that opportunity also "plays a vital role in eliciting criminal behaviour". The main focus of situational crime prevention is on opportunity rather than root causes, which can prevent crimes in the immediate term. Methods can be tested and rolled out in the short term, whereas because of the necessarily longitudinal nature of examining ways to address the root causes of crime, the relevance of the research can be lost by the time the results are established, and it is difficult to separate out other factors. Clarke (2004) illustrates this with the example of studies of delinquents 20 years ago – the research provides interesting insights from an historical basis, but came too late to be of use to the participants, and the world has moved on so quickly that the findings bear little relevance to modern-day delinquents. More generally, SCP views opportunities as the key and most proximal root causes of crime, and there is now ample evidence that variation in crimes across time and place are caused primarily by variations in opportunity. In addition, SCP does not preclude efforts to tackle other social problems but recognises that crime prevention is generally not the main goal of many such efforts.

WHEN CRIME IS PREVENTED IN ONE PLACE, IT MOVES TO ANOTHER

The displacement argument is examined in depth earlier in this chapter. However, it is worth reiterating here that displacement has never been shown to be total, and is not inevitable. The idea of displacement pervades the early literature, for example Hakim and Rengert (1981:17) state of displacement “the community is protecting itself partially at the expense of neighboring communities” and that (p12) “for the professional criminal however, crime is merely displaced”. Eck (1993: 1) introduces the concerns of the police that crime prevention efforts “may simply mow crime around without reducing offenses”. Much work has been done in this area over the last two or three decades, and our understanding of displacement has increased accordingly. Although it can be impossible to prove displacement has not occurred due to its nature (Barr and Pease, 1990), where there have been measurable failures, this has usually been due to implementation mistakes (Clarke 2005).

SITUATIONAL CRIME PREVENTION BLAMES VICTIMS

Much is made of the idea that placing blame onto victims is unacceptable (see for example Karmen, 2009). Critics of situational crime prevention may turn to the concept of blame being placed on victims of rape, for example because of their alcohol or drug intake, their clothing, or behaviour. Here it is important to distinguish between blame, and managing risk. The offender in such a case is to blame, however, in other areas of life, being given advice to minimise risks is the norm, particularly with health issues. Thus, we have women being advised to increase their folic acid uptake whilst trying to start a family; smokers being advised that their health (and indeed life) is at risk if they continue their habit; and obese people who develop Type 2 diabetes being given dietary and exercise plans by their doctor. These seem relatively accepted by society, yet similar risk reduction strategies are perceived differently when coming from crime prevention practitioners. Despite this, many simple techniques are incorporated into everyday life without claims of blame being bandied about: locking car doors; installing anti-virus protection; avoiding walking alone in certain areas at night; and keeping passwords secret, to name but a few. These routine precautions (Felson and Clarke, 1995) appear sufficiently embedded in everyday life that they are easily overlooked, or perhaps wilfully ignored, by those making knee-jerk accusations of victim-blame against situational crime prevention efforts.

SITUATIONAL CRIME PREVENTION RESULTS IN AN INFRINGEMENT OF FREEDOM

This criticism is a generalisation which is simply inapplicable to much, perhaps most, SCP. Honess and Charman (1992) examine this argument in the context of CCTV, whilst Homel (1996: 2) characterised the orientation of SCP as being viewed as “wedded to a conservative crime control agenda emphasizing surveillance of marginalised groups and social exclusion”. In other instances the opposite occurs and situational prevention enhances individual freedom and liberty: modern vehicle security, for example, can use proximity devices which require little effort by the user but which change the default from insecure to secure. Such measures increase peace of mind (a psychological benefit), reduce accidental insecurity (that of leaving the car unlocked), reduce the time and effort of locking-up, and also reduce the temptation of easy opportunity to likely offenders in the vicinity. The simplistic notions of infringement of freedom tend to be limited to a few high-profile crime prevention tactics such as CCTV which are frequently criticised by proponents of SCP. As Jeremy Bentham put it “It is the

greatest good to the greatest number of people which is the measure of right and wrong". There exists a need to weigh up the costs and benefits of any particular approach to crime prevention on a case by case basis. For example, few people would expect airport security to be non-existent, as this could result in a return to the frequent hijackings of the mid 20th century, or an increase in terrorist attacks (Clarke, 2005). This is a limitation of freedom that most accept. Of course, in cases where there is no or little effectiveness, the infringement of freedoms may be too high a price to pay but this is entirely compatible with the SCP approach which incorporates evaluation of good and bad practice

SITUATIONAL CRIME PREVENTION NEGLECTS INEQUALITIES AND IS SOCIALLY DIVISIVE

This argument generally seems to refer to the target hardening element of situational crime prevention, where (it is argued) the wealthier members of society can afford upgraded security to the detriment of the poorer members (see O'Malley, 1994). Firstly, this erroneously relies on the premise of total displacement, which seems particularly unlikely given the inherent differences between the target types – for example poorer people are less attractive targets for acquisitive crimes due to fewer possessions. Secondly, target hardening is just one of many tactics that can be utilised – not all of which require money. Finally, it can be argued that situational crime prevention is a less socially divisive means of reducing crime than other approaches, because the criteria by which a person or place is singled out for attention is that of their victimisation history, rather than any socio-economic factors favoured by other approaches. More generally, it is possible that situational prevention provides the breathing space from crime in which measures to alleviate other social problems might begin to take root where otherwise they could not.

SITUATIONAL CRIME PREVENTION CANNOT BE APPLIED TO EXPRESSIVE CRIMES, ONLY TO ACQUISITIVE CRIMES.

This perspective assumes that an expressive crime holds no measurable reward for the offender as the crime is committed on impulse (see Hayward, 2007). The concept of 'bounded rationality' also applies here, which was discussed earlier in this chapter. Expressive crimes do indeed hold a reward, albeit non-monetary. Graffiti artists experience a psychological reward – a thrill from seeing their work in public (Farrell, 2010) and even use their work to exert control (Hayward, 2002) and to undermine authority (Ferrell, 1995); child molesters have sexual gratification and the knowledge they have control over their victim (Bern, 2009); and the name itself suggests that joyriders, experience 'joy' at their crimes. Thus, rapid cleaning of graffiti reduces the thrill of seeing ones 'tag' publicly displayed and arguably reduces their ability to exert control, making it less likely for the crime to be perceived as worthwhile. Preventing access to children by known sexual offenders via measures such as background checks (though of course in practice this is extremely difficult, and does not account for previously undetected offenders) , can prevent a child from becoming a victim of abuse. Moreover, Wortley and Smallbone (2006) found that most child sex offenders had offended due to the situational precipitators, that is, the opportunity and context, rather than an inherently strong motivation. Improving security on cars makes it harder for a joyrider to find a suitable (i.e. easily taken) target, and thus reduce the likelihood of a car being stolen, despite the possibility that some 'desire' is still present (though the almost complete elimination of joyriding by car security in recent years in some countries suggests such desire is often far from intransigent). Overall, however, and regardless of motivation, a removed opportunity prevents that crime.

CONCLUSION

There has been massive progress within situational crime prevention since its inception three decades ago. Developments both in theoretical and practical aspects have increased understanding of ways in which criminal activity can be disrupted. The absorption of some situational practices into everyday activities, such as the adoption of repeat victimisation as a police performance indicator, and the development of Secured By Design practices on some housing estates, shows that there have been significant successes within situational crime prevention. However, despite many research projects illustrating the usefulness of this approach to crime, there is much that remains unknown, and some critics remain vocal. In order to progress situational crime prevention further, rigorous assessment and evaluation of methods and outcomes is essential. To this end, the next section examines two methods of meta-evaluation: systematic reviews and realist synthesis. These are each used in turn to evaluate the impact of repeat victimisation prevention programmes, beginning with the systematic review, the methodology of which is presented in the next chapter.

SECTION II:

FOCUS ON REPEAT VICTIMISATION

To know what everybody else knows is to know nothing

- *Remy de Gourmont*

Chapter Four: Systematic Review Methodology

Truth is ever to be found in simplicity, and not in the multiplicity and confusion of things.
- Isaac Newton

BACKGROUND

Using repeat victimisation as a strategy to direct crime prevention resources is a method that has become increasingly widely utilised across police forces in England and Wales (Laycock, 2001) particularly for domestic burglary. Reducing repeat victimisation has been argued to reduce overall levels of the targeted crime type, or at the very minimum 'spread the misery' by more evenly distributing victimisation amongst the population and thus decreasing the burden on the most chronically victimised (Barr and Pease, 1990; Pease, 1992). Perhaps the most famous example of where this strategy has been successfully used is the Kirkholt Burglary Prevention Project, which successfully focused situational crime prevention efforts on reducing repeat domestic burglary as part of a package of measures, and is detailed in Chapter Three. The Kirkholt study was begun two and a half decades ago at the time of writing. Since then further work on preventing repeat victimisation has been carried out. This more recent research has included a focus on other crime types, such as sexual victimisation, commercial burglary and domestic violence. However, there remains a lack of consistency in the extent to which these developments have been disseminated into wider practice (Laycock, 2001).

Debate continues about how to implement an intervention based on repeat victimisation prevention. Upon close examination of interventions which are based on early work such as Kirkholt or Biting Back, one sees that they are often in many ways very dissimilar (see for example Tilley's examination of putative replications, 1993). Schemes which purport to tackle repeat victimisation appear to vary in the way in which the target group is defined. Some interventions examined in Chapter Five (e.g. that in Lambeth) placed a high priority on 'vulnerable' members of society, where the empirical basis for the definition of vulnerable was not always readily available, or even acknowledged. One such 'vulnerable' group often targeted by interventions was that of the elderly. In the Merthyr Tydfil domestic burglary project also discussed in more detail later, volunteers identified this group by examining lists of those ineligible for jury duty. Whilst an innovative approach, there is little evidence that older people are at greater risk of crime (Mawby and Jones, 2006). Such potentially inconsistent definitions could divert resources away from repeat victims and thereby undermine the purported crime prevention strategy. Also under debate is whether a victim needs to have been victimised on multiple occasions before the intervention or whether the crime prevention strategy should be in place from the first victimisation. Once a victim has been targeted once, it is known that they are more likely to be a victim again, and that their risk is greater within a short time from the original victimisation (Polvi et al., 1990). This would suggest that all one-time victims would benefit from involvement in an intervention. However, several programmes included within this review (e.g. the Cambridge domestic burglary project and the Liverpool commercial burglary project) focused their attentions on those victims experiencing two or more crimes. Reasons for this were not always explicit in

the reports, although it can be hypothesised that financial constraints on the number of individual interventions available is one possible explanation. While this is only a brief introduction to some of the issues, it is apparent that the question of ‘what works’ is therefore influenced not just by the techniques of crime prevention, but also more broadly by debates relating to definitions, scope, and implementation.

Existing opportunity theories suggest that crime can be interrupted by a series of situation-specific measures. Routine activity theory and rational choice theory suggest that criminal activities can be disrupted and therefore prevented at a specific point. These theories were presented and explained in Chapter Three. Various components of the twenty-five techniques of situational crime prevention can be utilised within interventions designed to reduce repeat victimisation. However use of the twenty five techniques was not a prerequisite for inclusion in this review. Event dependence or state heterogeneity theories of repeat victimisation (or boosts and flags as they are more colloquially referred to) suggest two key reasons for a victim being targeted more than once. Firstly, once victimised, the offender knows of further reasons to go back to the target or to tell other offenders of such reasons – thus ‘boosting’ their chances of being victimised. Secondly, the existing attractiveness of the target may be great, so that they are effectively are ‘flagged’ to potential offenders. In ‘boost’ situations, the offender may be deterred from returning if the situation has visibly altered since the original offence. For example, additional security on a home or commercial premises, or a police-linked personal alarm in the case of domestic violence or sexual victimisation could increase an offender’s perception of risk. Similar techniques can be used if the underlying reason for revictimisation is due to the ‘flags’ explanation. Here, longer term vulnerabilities can be addressed, and perhaps short term loans of equipment are less appropriate than longer term improvements to security. In either of these situations, taking preventative action to lessen the risk to the victim can be seen as an effective way of ‘drip-feeding’ crime prevention measures (Pease, 1998) because resources are allocated on a gradual basis to targets as they are victimised, proving a manageable schedule for crime prevention (rather than a ‘blanket’ effort where, for example, all households in an area are target hardened at the same time)

There are dissident voices to the approach of tackling repeat victimisation. Many of these are more appropriately viewed as general criticisms of situational crime prevention, and Clarke (2005) has recently rebutted seven oft voiced criticisms. Key among such criticisms is the idea of displacement, which was discussed in depth in Chapter Three. This criticism suggests that regardless of the prevention scheme in operation, criminal activity will always move (be ‘displaced’) to another location, crime type, modus operandi, target, or time and therefore crime levels will remain constant. As such, no overall gain would be expected. However, the Hesseling (1994) and Eck (1993) reviews gave empirical confirmation to the suggestions of Cornish and Clarke (1987) and Barr and Pease (1990) that this is unlikely, plus that if displacement was experienced it would be unlikely to be total and would leave a net gain of crime reduced. The extent to which displacement or diffusion of benefits occurred is commented on for each of the included studies in this review where the information was available.

Farrell and Pease (2006) conducted a narrative review on “a subset of the evaluations relating to the prevention of repeat victimization” summarising eleven residential burglary studies. Although the review aimed to follow the ethos of the Campbell Collaboration’s systematic review process, searches were limited to projects aiming to prevent repeat residential

burglary. They concluded that “there remains a paucity of evidence” but that successful interventions appeared to rely on:

- a strong preventive mechanism
- multiple tactics
- strong implementation
- a focus on high crime or burglary rate situations

The systematic review presented here returns to repeat victimisation prevention programmes but expands, updates and improves on the previous efforts of Farrell and Pease, with a more stringent and transparent set of inclusion criteria. Effect sizes and counterfactual calculations are included to assess the success of the programmes both individually and as a whole. This review moves beyond domestic burglary to examine other crime types, and the Tilley (1993) putative replications reviewed in the Farrell and Pease article are included here as identified, named sites due to the availability of greater information – with the exception of site R2 (Bradford, from Safer Cities – See Tilley and Webb, 1994) which was excluded from this review as the intervention was not aimed specifically at prior victims. With one exception, searches were not restricted by crime type, but eligible studies were identified for just four crime areas: domestic burglary, domestic violence, commercial burglary, and sexual victimisation. The exception is the exclusion here of evaluations of second responder programmes on family abuse, which were comprehensively covered by Davis et al. in their 2008 Campbell Collaboration review. That review concluded that although the follow ups to victims did not reduce violent incidents, they did encourage increased reporting of incidents (Davis et al., 2008). A key justification for their exclusion is that the focus was primarily on family violence than on *repeated* family violence per se. This is also the argument that excludes efforts to evaluate the effectiveness of arrest to prevent domestic violence. While they have been evaluated and included in reviews elsewhere (Sherman, 1992; Feder et al., 2008), the focus was primarily on the effectiveness of arrest as a tactic rather than being a focus primarily on repeat victimization.

OBJECTIVES OF THE REVIEW

Using crime prevention programmes which focused on repeat victimisation prevention as a mechanism to reduce crime, the present review aims to establish:

- the success of these interventions, predominantly using the outcome measures of change in incidence and change in prevalence, depending on which were available.
- the extent of any displacement which occurred as a result of the intervention (and diffusion of benefits where available)
- a summary of the evidence available to help practitioners understand whether or not to adopt the policy of focusing crime reduction specifically on preventing repeat victimisation

CRITERIA FOR INCLUSION AND EXCLUSION OF STUDIES

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 essential, though there were no significant restrictions on how that group was fashioned (pragmatic considerations meant that comparison groups comprising the rest of area were permitted – see Farrington and Welsh, 2006, whose examination of regression to the mean reveals such comparison areas to be generally valid).
3. A focus on repeat victimisation on an individual level rather than a hot spot/area basis had to form a significant part of the study.

Features incompatible with those listed above meant a study was excluded from the systematic review. In addition, a judgement call was made about the methodological quality of a study, with those found to be lacking excluded – these excluded studies are listed in Appendix A. Where the extent of data was insufficient, extensive attempts were made to locate another data source (for example a different report on the same study) before the decision to exclude the study was taken.

Decisions on relevance were made initially by titles, and then the list narrowed down by examining abstracts. Efforts were then made to access the full text of all potentially relevant reports, and the final decision on inclusion was based on a reading of the full text. The decision on relevance was made by this author and, as specified in the systematic review procedures, the reliability of the decisions was assessed by a separate consideration of the reports by Professor Farrell. Any disputes on inclusion were debated, with the final decision resting with this author. Some particularly close decisions were based on two falls or a submission. No kicking or gouging was permitted during collaborative decision-making.

SEARCH STRATEGY

Every effort was made to ensure that all relevant studies were identified for consideration for inclusion. The wide range of sources searched ensured that the risk of missing relevant studies was minimised, and that therefore potential for bias due to not including data from relevant interventions was also minimised. In addition to searches of electronic databases, the search made a specific effort to discover all potential sources of data, particularly grey literature. These attempts included: searches of government sources; snowballing from bibliographies of identified studies; searches of institutional repositories; and contact with authors. There was no restriction on searches by country of evaluation, and provided that titles and abstracts were available in English, full texts of all languages had the potential for inclusion. None of the identified studies required translating. Extensive efforts were made to retrieve all identified studies, and with the help of inter-library loans, wide-ranging resources from Professors Pease and Farrell, and the invaluable assistance received from the authors of identified studies, all bar three full texts were retrieved where identified as having potential relevance. These three were: a report on a Tasmanian domestic burglary intervention by Goodwin (a conference presentation was obtained but did not contain full details), a conference poster by Calhoun et al., and further details on ‘Safe as Houses’ by Fisher (where a short report only was identified and obtained, it is not known if a longer report exists). Any bias due to non-identification or retrieval of relevant information was kept to an absolute minimum.

Eight online databases were 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)

In addition, the websites of the following bodies were searched for relevant evaluations:

UK Home Office
Australian Attorney General's Office
EThOS (Electronic Theses Online Service)

Bibliographies hand searched were:

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**)

Following background research and preparations, plus iterations of preliminary searches to lay the groundwork, the final versions of the searches used herein were conducted during the first half of 2009. This means any subsequent studies are not included.

DESCRIPTION OF METHODS USED IN PRIMARY RESEARCH

The methods utilized in the primary research varied by crime type. The following section separates out the crime types investigated within the systematic review and describes the common methods. This includes: selection and allocation of participants (or areas) to treatment or control conditions; data sources; and outcome measures. Any additional methods or issues specific to a particular crime type or study are described at the appropriate point as the review progresses.

DOMESTIC BURGLARY

Selection and allocation of areas

In the majority of evaluations, the treatment area was chosen due to particular features, including a relatively high domestic burglary rate, and clear area boundaries (either geographic or administrative). This latter feature was important to enable data collection and analysis, and often resulted in areas being defined by police beats. In general, a comparison area or areas were chosen once the treatment area had been identified. This selection process varied in rigour across evaluation studies, and comparison areas ranged from the use of the remainder of the police area; through to using neighbouring areas; to non-contiguous comparison areas, carefully matched on factors such as socio-economic demographics.

Data sources

Although some of the studies included here used victim surveys, it was more common to rely on police data. In some cases the police data gathered was additional to that which would normally be collected, and researchers were frequently involved in cleaning the data before any suitable analysis could be undertaken. Generally, where victim surveys were used, response rates were too low to provide data of use for the purposes of this review.

Outcome measures

The evaluation studies varied as to whether they measured outcomes in terms of changes in prevalence, incidence, concentration, or a combination of these. To complicate matters further, comparison areas were not always examined in as much depth as the treatment areas. Thus, in several cases prevalence of victimisation was measured in the treatment area only. Therefore, as incidence was measured in both treatment and comparison areas in all of the domestic burglary studies included here, this is the main comparison outcome measure used. The obvious difficulty here is that it is the relationship between incidence and prevalence that provides the meter of change in repeat victimisation. Other important indicators of outcome have also been included where available, most notably that of any evidence of displacement. Once again, this outcome measure was not available for all studies.

COMMERCIAL BURGLARY

Selection and allocation of treatment groups

In two out of the three included studies, interventions were provided to a subset of surveyed businesses – those that were identified as high risk (due to previous victimisations). In the remaining study (of the Multnomah site) recently victimised properties were separated out at the analysis stage. Commercial sites were identified for the initial survey by either a stratified sample (Merseyside and Multnomah) or all businesses that agreed to participate (Leicester). Comparison groups varied in strength, from having equivalent burglary type and geographic distribution (Multnomah) through to the use of the remainder of the police subdivision (Leicester) to the arguably weaker design of using the businesses ineligible for the intervention (due to a low crime rate) as the comparison group (Merseyside). This last comparison is at risk of regression to the mean, where a high crime rate naturally falls, and a

low crime rate naturally rises to average out at the mean, so here any positive results needed to be treated with particular caution. That is, in the Merseyside evaluation - where the high-rate treatment and low-rate comparison group are drawn from the same geographical area - there can reasonably be said to be an *a priori* expectation that regression to the mean could play a more prominent role than that identified by Farrington and Welsh (2006) who compared high and low rates in distinct geographical areas.

Data sources

A combination of business surveys and data on reported crimes were used for all three studies. Commercial burglaries are often reported for insurance purposes (Pearson, 1980) and so reported crime data are considered to be reasonably reliable. Surveys were used in all cases, as either a primary or secondary source for crime data.

Outcome measures

A mixture of incidence, prevalence, and concentration of burglaries were recorded within the three studies. Incidence was the only measure common to all three. Displacement was only measured by the Merseyside study, where evidence for spatial, crime type and tactical displacement was examined.

SEXUAL VICTIMISATION

Selection and allocation of treatment groups

With one exception, participants were taken from undergraduate programs and randomly assigned into treatment and control groups. The exception took participants from counselling programmes in the community, but still made efforts to randomly assign participants, except where there was a need to specifically allocate to one group or another (for example where participants shared a house, contamination was likely if one but not the other was in the treatment group). Some groups included a mixture of victims and non-victims, but in all cases participants with a prior history of victimisation could be identified for separate analysis.

Data sources

History of prior victimisation was identified through a self-report questionnaire, except where participants had been referred to the programme directly from sexual victimisation counselling. A further questionnaire identified victimisation in the follow-up period, the length of which varied but was most commonly nine weeks. Care must therefore be taken that success in the post-victimisation period does not get artificially inflated – as the post-period is much shorter than the pre- period of measurement. Counterfactual calculations allow the success relative to the control group to be calculated, but there is still scope for fluctuations in this short time period to disrupt the level of confidence in the findings.

Outcome measures

The common outcome measure was that of prevalence measured in both treatment and comparison groups, and broken down to those that had or had not been prior victims. For the purposes of this review, the interest lies solely with those that had experienced a previous victimisation.

DOMESTIC VIOLENCE

Only one domestic violence study was identified which met the criteria for inclusion within the systematic review. Although others had interesting methodological takes, the outcome data was either not relevant to this review, or there was insufficient data. Therefore, the information below relates to this single study only.

Selection and allocation of treatment groups

The treatment was available in two areas; the comparison group was a compilation of other urban areas. Interventions were provided to individuals within these areas who had been victims of domestic violence. No justification for the choice of treatment areas was provided, although a geographical match with police districts was implied.

Data sources

The predominant source of data was that of a database of calls to the police. There was a database recording the types of intervention provided to victims (and offenders), however this was not linked with change in victimisation levels. Survey data was only available from before the study, not after; and there were insufficient interviews with victims to be able to provide useful data for this review from this source.

Outcome measures

Calls for service provided an implicit measure of incidence and concentration of domestic violence victimisations. Increases and decreases in both total and repeated calls for service therefore indicated the success or failure of the intervention.

CRITERIA FOR DETERMINATION OF INDEPENDENT FINDINGS

Duplication of findings could result in the effect of a particular intervention appearing to have a far greater (or lesser) impact than it actually did. Steps were taken to prevent this from occurring. Any anonymous studies were identified before inclusion and matched to named studies. This prevented the accidental duplication of the findings from entire evaluations. Several of the evaluations included data on multiple outcome measures, so that there was information on incidence, prevalence, and concentration. The sexual victimisation studies had different outcomes based on whether the measure was of rape or of other sexual victimisation experiences. In the relevant graphs presented herein, these different outcomes are presented separately but in the text it is made clear that these should not be double counted, and that only four studies were included in the odds ratio calculations. Transparency was an underlying theme, and so each type of outcome is presented separately in Tables 5.1 – 5.9.

Incidence was used as the main outcome measure for all crime types bar sexual victimisation, where prevalence was used due to the availability of data. This allowed comparisons to be drawn across different studies. Several reports presented findings from multiple studies. Each study was coded separately, even where it came under the same programme. For example, the DirectLine Homesafe programme (Webb, 1996) contained seven studies, each examined separately (and only four of which were finally included in the systematic review). This provided a more precise approach than treating the programme as a single multi-site study, as each site had its own distinct methodologies, approaches and problems. Some of the studies measured the same outcome at multiple points in time. In this review, just two points were taken: a 'before' and a 'during' or 'after' measurement. Although for comparison purposes, it would have been preferable to have the same time period recorded for each study (i.e. either *during* the intervention, or *after* the intervention), as a retrospective review there was no such luxury of choice, and restricting the studies on the basis of covering the same time period would have further cut the number of included studies. In practice, the data used was commonly the year before and the year after the start of the intervention.

DETAILS OF STUDY CODING CATEGORIES

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. An example of the coding manual used is included here as Appendix B. The data from studies were coded directly into a computer-based spreadsheet database. The use of both manuals and database combined to allow continued coding reliability. Monitoring of this reliability was done by recoding a sample group of studies at a later stage to check that the same coding outcome was recorded. Characteristics coded varied between crime types, a necessary adaptation to allow for the distinct differences in approaches to the different crime types. However, the coding was kept the same wherever possible.

Moderating variables were coded and included a description of the context of each intervention. For example the context variables coded for the domestic burglary studies included the size of the study area and the housing estate as a whole; the type of housing; and the type of occupancy. These contextual factors are all represented in Tables 5.1 to 5.9 in Chapter Five. Other factors that may have impacted on the findings from the study were also coded for, for example if there were any other interventions reported as occurring in the treatment or comparison area. Some of the coding categories were used as inclusion criteria, for example the research design descriptors and some sample characteristics. For the purposes of the review, there was minimum standard of a Level 3 on the Maryland Scale of Scientific Methods, although the criterion for a 'comparable' comparison group was flexed slightly to enable a useful number of studies to be included (and the specifics of each study are discussed later in this thesis).

STATISTICAL PROCEDURES AND CONVENTIONS

In order to standardise the findings from the studies so that they were comparable, recalculations had to be carried out. In the first instance this was done by working out the counterfactual. The counterfactual provides a baseline against which the outcome in the treatment group can be measured (Cummings, 2007). The number of crimes expected without an intervention is then compared to the actual number of crimes to determine whether the

outcome was favourable or not. The number of crimes expected is worked out using the formula from Johnson et al. (2004):

$$\text{Expected crimes} = \text{crime in treatment area before} \times \frac{\text{crime in comparison area after}}{\text{crime in comparison area before}}$$

The number of crimes prevented is then calculated by the following, also from Johnson et al. (2004):

$$\text{Burglaries prevented} = \text{Actual Crime in Action area after} - \text{Expected}$$

Evaluating the outcome in this way allows an implicit acceptance of whether the intervention was responsible for changes seen, rather than the changes being due to natural fluctuations in the crime rate.

This counterfactual analysis provided an excellent starting point. However, to enable the interventions to be directly comparable to each other, an effect size needed to be calculated. Effect sizes are a way of standardising and directly comparing effects across studies and outcomes (Gottfredson et al., 2002). For the purposes of this review, the effect size calculated was that of the odds ratio. 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 multitude of place-based crime prevention evaluations (Bowers et al., 2008) and Welsh and Farrington used it in their systematic review of the effectiveness of CCTV (Welsh and Farrington, 2002). Marchant (2005) criticised the use of the odds ratio to evaluate place-based crime prevention initiatives on the basis that “crime events are not independent” and the natural fluctuations within these is large enough to “mask” any effects caused by the crime prevention initiative. Whilst Marchant makes a reasonable point, Bowers et al. (2008)’s succinct justification for their continued use of the odds ratio in this context was that “frankly, satisfactory alternatives are not yet available”. The Bowers et al. conclusion remains true at the time of writing.

An odds ratio effect size can be calculated using the following:

Figure 4.1 Odds ratio calculations (from Lipsey and Wilson, 2001)

	Frequencies		Proportions	
	Status A	Not Status A	Status A	Not Status A
Group 1	<i>a</i>	<i>b</i>	$p_a = a/(a+b)$	$p_b = b/(a+b)$
Group 2	<i>c</i>	<i>d</i>	$p_c = c/(c+d)$	$p_d = d/(c+d)$

$$ES_{OR} = \frac{ad}{bc} = \frac{p_a p_d}{p_b p_c} = \frac{p_a \bar{p}_b}{p_c \bar{p}_d} = \frac{p_a(1-p_c)}{p_c(1-p_a)}$$

(If a, b, c, or d = 0, add .5 to all cells)

However, there are a number of software programmes which do this calculation automatically. The one used for the purposes of this research was online, from the Centre for Evidence Based Medicine, Toronto. Before committing to the use of this calculator, a number of known odds ratios were worked through using the software, to ensure accuracy.

Not all studies in the systematic review provided the complete data required for odds ratio calculations. Commonly, this problem arose where percentages, but not raw data, were provided for intervention and comparison groups. These studies were not excluded from the systematic review, as they met the inclusion criteria, but readers should be aware that this small number of evaluations was excluded from the odds ratio calculations. Affected studies are flagged as being excluded from the odds ratio in the text of the systematic review.

TREATMENT OF QUALITATIVE RESEARCH

There were no purely qualitative studies identified that were suitable for inclusion in this review, although several studies included qualitative elements. Although there were a number of qualitative studies identified in the field of repeat victimisation, the focus of these was not on a reduction of crime, but rather related areas, such as fear of crime which were not relevant to the purpose of this review. The one exception was Lloyd et al. (1994) who examined repeat domestic violence reduction, however this was excluded from this review on the grounds that there was no comparative work conducted with victims who had not received an intervention.

CONCLUDING NOTE

This methodology was used to conduct a full systematic review on the impact of repeat victimisation prevention programmes. The findings from this review are presented in the following chapter, and conclusions and recommendations which emerge from these findings are presented in Chapter Nine.

Science may be described as the art of systematic over-simplification — the art of discerning what we may with advantage omit.

- Karl Popper

RESEARCH QUESTION

Do interventions which target crime prevention measures at repeat victimisation work to reduce crime?

INTRODUCTION AND SUMMARY OF FINDINGS

Thirty one studies were eligible for inclusion in this systematic review, although not all of these provided sufficient information for inclusion in the odds-ratio calculations, hence the disparity between numbers included in the graphs and those included in the narrative review. Exclusion from the odds-ratio is noted in the relevant sections of the review below. The narrative review of included studies is presented below by crime type, and chronologically within crime types. A snapshot summary of outcomes is presented first in Table 5.1. This is supplemented by a series of summary tables showing evaluation design and implementation issues as Tables 5.2 – 5.9. The format of Table 5.1 draws on that of Bennett et al. (2009), and the remaining tables draw on those used in Farrell and Pease (2006) and existing systematic reviews such as those of Braga (2007) and McDougall et al. (2008).

New analyses and reinterpretations of the original data used in studies are included where appropriate. In particular, wherever possible the analysis includes a use of ‘counterfactual inference’ (Cummings, 2006:7) where changes in victimisation are compared to what might be expected based on the change in the non-intervention area. Effect sizes of each study have been calculated using the odds ratio, and findings are presented visually in Charts 5.1 – 5.3. Confidence intervals (at 95%) are included on these charts, to indicate the reliability of the effect size. With the exception of sexual victimisation, the charts use incidence as their measure, which is not the primary measure of repeat victimisation. This was a pragmatic decision, as the information on change in repeat victimisation was not provided for both treatment and control areas by the majority of evaluations included here. Due to the summative nature of what follows, readers are referred to the original sources for further information, and page numbers are provided where possible to facilitate that process.

Table 5.1: Summary of Outcomes for Evaluation Sites relating to Residential Burglary, Domestic Violence, and Commercial Crime

Evaluation	Year of main publication	Reduction in repeats? : %	Crime incidence change (treatment)	Crime incidence change (comparison)	Crime incidence change (treatment relative to comparison)	Outcome overall (incidence)¹
<i>Residential burglary</i>						
Kirkholt	1989	Yes : 100%	-71.7%	-24.1%	-62.8%	+
St Anns	1993	Not measured	+42.8%	+57.3%	-9.2%	+
The Meadows	1993	Yes : 40.4%	+1.2%	+112%	-57.5%	+
Eyres Monsell	1994	Yes: NA	+24%	+31%	-6%	+
New Parks	1994	Yes: 50%	-20%	-34%	+17.5%	- !
Huddersfield	1995	Equivocal	Not provided	Not provided	-30%	+
Blackburn	1996	Yes: 68.8%	-35%	+71%	-62%	+
Burnley	1996	Yes: 33.3%	-22.2%	+6.8%	-27.2%	+
Lambeth	1996	Yes ² : (95%)	-70%	+42%	-80%	+
Merthyr Tydfil	1996	Yes: 92%	-46%	-26%	-26%	+
Cambridge	1999	No.	-4%	-15.7%	+13.8%	-
Baltimore	1999	No.	-4.9%	+24.7%	-23.7%	+!!
Dallas	1999	No.	+9.4%	-5.7%	+16%	-
San Diego	1999	No.	-30.1%	-28.1%	-24.7%	+!!
Beenleigh	2001	Yes: >15%	-2.4%	-11.2%	+9.9%	-!
Ashfield	2001	Equivocal	-22.1%	-23.5%	+1.8%	-
Tea Tree Gully	2002	Equivocal	+31.3%	+22.2%	+7.5%	-

¹ ! or !! indicates where the reduction in repeats and incidence measures contradict

² The data available for repeat burglaries in Lambeth does not precisely correspond with the time period elsewhere in the evaluation. There is unlikely to be a causal link between the intervention and the outcome

Evaluation	Year of main publication	Reduction in repeats? : %	Crime incidence change (treatment)	Crime incidence change (comparison)	Crime incidence change (treatment relative to comparison)	Outcome overall (incidence)¹
Liverpool	2003	Yes: 70.5%	-32.6%	+10.9%	-39.2%	+
Orange	2003	Yes: 74%	-43%	-13%	-57%	+
Hartlepool	2005	Yes: 26.3%	-26.9%	-10.6%	-18.3%	+
Bentley	2005	Yes: 49%	-45.2%	-25.8%	-26.2%	+
Morley	2005	Yes: 58%	-24.2%	-25.8%	+2%	-!
<i>Domestic violence</i>						
NDV ³	2004	Yes	-12.3%	-5.3%	-8.2%	+
<i>Commercial Crime</i>						
Multnomah	1980	Yes: NA.	+3.9%	+22%	-14.9%	+
Leicester	1998	Yes	-41%	-32%	-19.7%	+
Merseyside	2001	Yes: 9.3%	-32.6%	+10.9%	-39.2%	+
<i>Sexual Victimization</i>			Note that the sexual victimization projects refer to crime prevalence not incidence in columns 4-7.			
Hanson	1993	NA	24.5%	29.8%	-17.8%	+
Breitenbecher	1998	NA	28%	28.6%	-2%	+
Marx	2001	NA	21%	32%	-36%	+
Gidycz	2001	NA	36.9%	32.9%	12.1%	-
Davis	2006	NA	26%	29%	-10.3%	+

³ Outcomes measured as domestic violence calls to the police

DOMESTIC BURGLARY

Twenty two eligible domestic burglary studies were identified for inclusion from the UK, Australia and USA. A further eight studies were excluded. These were predominantly from the same three countries, although one study from New Zealand was also identified. A list of the excluded studies can be found in Appendix A. Table 5.2 presents a summary of the key features of the programmes. Table 5.3 provides an at-a-glance summary of implementation issues and outcome measures for each study. Table 5.1 (above) shows an overview of the outcomes of the programmes according to the findings from the counterfactual calculations. Chart 5.1 provides a visual representation of odds-ratio calculations which were carried out to compare the effect sizes, based on incidence.

KIRKHOLT (UK)

The Kirkholt burglary prevention project (Forrester et al. 1988a; 1988b; 1990; Farrington 1991; Pease 1991; Farrington 1992) was the first to explicitly utilize repeat victimisation as the focus of a crime prevention strategy. The Kirkholt local authority housing estate consisted of 2,280 dwellings, and was bounded by roads on all four sides. Treatments were tailored specifically in response to the crime problems of the estate and included security upgrades at burgled homes with special attention to preventing repeat burglary by the same method of entry. Neighbours of victims were offered free security upgrades as an incentive to develop localized watch groups, each called a Cocoon Neighbourhood Watch. In burgled households, coin meters (boxes that held coins used to pay for electricity) were replaced since they were easy and frequent targets. In the second phase of the project, analysis of probation data identified debt as a motivator of burglary, and offenders were offered debt-management services. Implementation rates for prevention tactics were 68% for security upgrades (402 of 592 burgled households: Farrington 1992: 10-11) and close to one hundred percent for Cocoon Watch since by the end of the project close to the whole housing area was covered. The comparison group for the Kirkholt project comprised the remainder of the adjoining police subdivision – a larger area with some privately-owned properties and a lower burglary rate. There were three main outcome indicators: Burglary incidence fell 60% and repeat burglary to zero within six months of the start of the program (Forrester et al. 1990: 4); Burglary incidence fell 75% over three years (Forrester et al. 1990: 27); and burglaries declined at households where security was introduced but not at other households. The third indicator was developed by Farrington 1992 in an independent analysis which also excluded regression to the mean as a significant influence. There was no evidence of spatial displacement (Forrester et al. 1990: 29). The evaluators concluded that the project's key characteristic was preventing repeat residential burglary by all locally appropriate means, that is, tailoring multiple tactics to the local crime problem via a crime analysis approach.

ST ANN'S BURGLARY REDUCTION PROJECT (UK)

Gregson (1993; also Tilley, 1993; Tilley and Webb, 1994) evaluated efforts to reduce burglary in the St Ann's area of Nottingham. The area contained 8000 households and was "not a very high crime rate area" (Tilley, 1993: 6). Housing association and council tenants experienced a "particularly high" rate of reburglary of about 40% (Gregson, 1993: 20). Burgled properties of these tenants were target hardened, as were other 'vulnerable' publicly-owned households, while some privately-owned burgled homes were not. The comparison

group consisted of the remainder of the police subdivision. Burglaries in the treatment area increased from 694 in the year prior to the intervention to 991 during the year of implementation. The comparison group experienced a 57.2% increase from 978 to 1538 burglaries over the same period (Gregson, 1993: 49). If the treatment area had experienced the same magnitude of change, 1091 burglaries would be expected. Therefore, relative to the control group, the burglaries in the treatment area were 9.2% lower than expected. Displacement was not examined.

THE MEADOWS, NOTTINGHAM (UK)

Gregson and Hocking (1993; see also Tilley, 1993; Tilley and Webb, 1994) evaluated the prevention of burglary in The Meadows. This was an urban area consisting of a little under 4000 households. Security (to primary and secondary doors; ground floor windows and accessible upper floor windows) was upgraded by a Crime Prevention Carpenter specifically employed for the project. Burglary victims and those deemed at risk (elderly or disabled persons and single parents) were eligible for improvements. Security was upgraded swiftly, with 34% of households having their security improved on the same day as their victimisation. The report notes “The take up rate of the service amongst victims of burglary was 55%” (Gregson and Hocking, 1993: 22) though this may be partly because the letter to victims did not clarify that the service was free.

For present purposes 1990 was used as the pre-test and 1992 as post-test because only annual burglary data were available and the intervention ran for two years from April 1991. Annual burglary counts were derived for The Meadows (*ibid.* p.17) and for West Bridgford subdivision (*ibid.* p.16) in which it was located, with the subdivision minus the Meadows used as the comparison area.

Burglary remained stable in the treatment area, with 169 in 1990 and 171 in 1992. However, burglary increased dramatically in the remainder of the subdivision from 724 to 1722. Relative to the comparison area therefore, burglary in The Meadows treatment area was 135% lower than expected, that is, a relative decrease of 57.5%. Data on repeat burglaries was available for the 1991 part of the work and fell from 20.1% of burglaries in 1990 to 13.6% of burglaries in 1991 (*ibid.* p. 23), with a significant lengthening of the time to repeat burglaries (*ibid.* p. 24). Displacement was not measured.

THE EYRES MONSELL AND NEW PARKS CRIME REDUCTION PROJECTS (UK)

Matthews and Trickey (1994a, b) evaluated efforts to prevent burglary on the public housing estates of Eyres Monsell and New Parks. Both estates had comparatively low burglary rates at 4.4 and 4.8 burglaries per 100 households respectively (Matthews and Trickey, 1994 a, b: p. 2-3) which suggests that even the initial rates of repeats would not be high. Police recorded burglaries for the one year pre-implementation are compared here to the first 12 months of the 17 month intervention period. This was for two reasons: firstly, so that the pre- and post-periods would be of equivalent length; and secondly so that the evaluations were broadly comparable with other schemes included in this review.

Free door and window locks were provided on both estates, though there was a “relatively low” (Matthews and Trickey, 1994b: 20) take up rate, and many of the locks went to non-

victims. Our best estimate based on the evidence available is an implementation rate of between a quarter and a third for burglary victims. Property marking equipment was also distributed in Eyres Monsell (Matthews and Trickey, 1994a: 39). Neighbourhood Watch schemes were promoted on both estates, but with limited success on each, with “organisational problems” being cited on New Parks (Matthews and Trickey, 1994b: 57). Though one of the aims was to work with young people to reduce crime on each estate, “very little changed on New Parks...in relation to the improvement of facilities” (ibid. p.46) and Eyres Monsell set up a ‘school passport scheme’ that was found in practice to reward those children least likely to become involved in crime (Matthews and Trickey, 1994a: 41).

Eyres Monsell showed an increase of 24% in overall burglaries. Despite this there was found to be a decrease in repeat burglaries. Burglaries fell 6% relative to the comparison group, which was compiled from 3 “comparable estates” (ibid. p.2) for the purposes of this review.

New Parks showed a decrease of 20% in overall burglaries. Relative to the comparison area (of one comparable estate) there was an increase in burglaries of 17.5% due to a larger decrease in burglaries on the non-intervention estate. The number of repeat burglaries in New Parks fell by 50% (from 26 to 13).

Matthews and Trickey find that the reduction in repeat burglaries could not be attributed to the intervention for a number of reasons, including “delays in the fitting of locks”, evidence from interviewed burglars who said there was ‘no real deterrent’ and “not all houses who were eligible took advantage of the scheme or were informed about it” (ibid. p.26). There was “little evidence for displacement” within either estate. However in New Parks there was evidence of displacement to other estates.

FOUR HOMESAFE INITIATIVES (UK)

Janice Webb (1996) evaluated seven burglary reduction projects in Britain that are collectively termed Homesafe initiatives. Two of the evaluations (Manchester and Greenwich) are excluded here due to lack of a comparison area or pre-intervention data respectively. Plymouth is also excluded here, because although it set out to target repeat victims, the focus switched instead to tackling high crime areas. Each evaluation compares police recorded burglaries for one-year pre-implementation to the one year of implementation. The Homesafe initiatives were security upgrades – door and window locks and bolts plus door chains and viewers – intended for installation at the homes of burglary victims. Variations in implementation and other factors are discussed for each of the four evaluations below. In each project area, burglary victims and those defined as ‘vulnerable’ were eligible for security upgrades. The ‘vulnerable’ groups in each area varied, but tended to be the elderly, those living alone, and those in fear of crime. The report does not provide a rationale for why these groups were defined as being at greater risk of burglary. For each of the four evaluations, where a single comparison area was not identified, compilations of those provided were used as described in each. The ‘whole of area’ figures in each evaluation included the treatment area and therefore were not used here as part of the comparisons.

Blackburn

Two estates within Blackburn received the Homesafe intervention: Infirmery and Longshaw, comprising 2287 households in total. Security upgrades were provided to both burgled households and 'vulnerable' households, here being people experiencing attempted burglaries, those robbed of their keys, victims of domestic violence, single parents and the elderly living alone. 47% of burglary victims received a visit, and 84% of these had their security upgraded. Other activities in the area included some youth work, undercover police detection activities ("no exact details available", Webb, 1996: 50) and the improvement of sight lines overlooking bin sheds believed to be used as cover by burglars.

The comparison area used here was the neighbouring area of Mill Hill. Burglaries in the treatment area fell by 35%, whereas in the comparison area burglaries increased 71%. Hence burglary in the treatment area fell 62% relative to the level expected based on the comparison area. There was evidence that other crime types in the intervention area increased, possibly due to displacement.

Burnley

The implementation area within Burnley was that of Burnley Wood, an area of 2088 households with a relatively high incidence rate of 9.9 burglaries per 100 households in the year prior to the Homesafe intervention. Eligible groups for the intervention were: victims in the 2 months prior to the start of the intervention; all victims during the intervention year; and all repeat victims in the year before. In addition, a "very vulnerable" (Webb, 1996: 53) block of 14 houses deemed a hotspot also received security upgrades. 79% of victims in the intervention year received security upgrades, though this figure fell to 40% when looking at the previous year's victims. The report notes that "The very small number who were re-burgled after target-hardening and were subject to forced entry were offered enhanced security in the form of alarms or solid wood doors" (ibid. p.53). There was publicity throughout the scheme. Briefly mentioned were other measures which overlapped the Homesafe intervention, namely: a juvenile nuisance register, detached youth work, a youth rewards scheme, a graffiti removal scheme, some property marking, and advice packs from police and Police Specials "have been encouraged to patrol" (ibid. p. 53-4).

The comparison area used here is larger, an aggregate of the three adjoining police beats. The intervention area saw burglaries fall by 22.2%, whereas the comparison area experienced an increase of 6.8%. Hence burglaries in the Burnley Wood treatment area fell 27.2% relative to the expected change. Repeat burglaries fell from 9.6% to 8.7% of all burglaries in the treatment area but were not measured for the comparison area. Webb's evaluation speculates that part of the overall drop in burglaries could be "attributed to the intensive publicity having a deterrent effect on local opportunistic burglars." (ibid. p. 54). Displacement was examined. It was found that non-domestic burglary and theft from motor vehicles increased at the time of the Homesafe work, but other acquisitive crimes decreased.

Lambeth

The St Martin's housing estate in Lambeth, South London received the Homesafe intervention. The burglary incidence rate on this estate was 'medium' before the intervention

year at 7.9 burglaries per 100 households. Those eligible for the security upgrades were victims of burglary, and the vulnerable, which “included women living alone, single parents, the elderly, disabled, and victims of racial harassment and attack” (Webb, 1996: 64). In the August/September of the implementation year, the police ran Operation Bumblebee, which “resulted in some arrests” (ibid. p. 61) during the Homesafe implementation period.

Webb’s evaluation of the Lambeth Homesafe was deliberately not in-depth (see p.4 for discussion) because of the “severe implementation problems” due to “gross overcharging” and “poor workmanship” by the security contractors (ibid. p. 60). However, findings presented show that burglary in the treatment area fell 70% in real terms, whilst the aggregation of “two comparison areas” (ibid. p.62) saw an increase of 42% (though with small initial numbers). This suggests burglary fell 80% in the treatment area relative to the comparison area. The evaluation states that “burglary dropped around August/September 1995, the time when the police ran Operation Bumblebee” (ibid. p.62) and strongly implies that a large part of the drop is linked to Operation Bumblebee rather than the intervention, as there were such severe implementation issues with Homesafe in this area. As it is very unlikely that the intervention had any causal link with crime changes, Lambeth was not included in the odds-ratio calculations to determine overall effectiveness of repeat victimisation interventions in reducing crime.

Merthyr Tydfil

The Old Gurnos, a public housing estate consisting of 1614 households in the Welsh town of Merthyr Tydfil had a burglary incidence rate of 8.7 per 100 households in the year prior to the Homesafe intervention. The bulk of security measures (455 out of 516) went to ‘vulnerable’ people, namely the elderly, individuals living alone, disabled people, victims of other crimes, or those in fear of crime (Webb, 1996: 69). 51% of police recorded burglary victims during the intervention year received security upgrades, and only 31% of the prior year’s burglary victims received the intervention. Other initiatives in the area included a car-project “intended to reduce joyriding and theft from cars”, a Drug Aid Satellite base, an anti-burglary video, CCTV in car parks, furniture recycling “to support young homeless as tenants”, lighting around ‘vulnerable’ homes, “youth activities and play schemes and anti-bullying work in schools” (all listed but no further details, ibid. p. 70). Publicity was “high and continuous” (ibid. p. 69) including local media coverage and a van displaying the scheme name and sponsor. Each household in the Old Gurnos received a crime prevention advice pack.

The comparison area here is the “similar estate” (ibid. p.70) of Gellideg. Burglaries in the intervention area fell by 46%, and in the comparison area incidence fell by 26%. Hence, relative to the comparison area, burglaries in the Old Gurnos fell by 26%. The report notes “repeat victimisations fell from 25 (17.9%) in the year before Homesafe to only 2 (2.6%) in the Homesafe year, despite only just over half of the burglary victims taking up the target hardening service.” (ibid. p. 35). This outcome was not measured for the Gellideg estate. Examination of possible displacement and diffusion of benefits revealed that other acquisitive crime also fell on the Old Gurnos estate, except non-domestic burglary which increased by 9%.

HUDDERSFIELD (UK)

The Huddersfield 'Biting Back' project (Anderson, Chenery and Pease, 1995a, b; Chenery, Holt and Pease, 1997; Anderson and Pease, 1997) aimed to establish a precedent for routinely preventing repeat burglaries across a large area, and hence put this programme into place in the Huddersfield police subdivision, which had a population of 22,000. Arguably the key additional innovation of the project was the introduction of graded responses to repeat victimisation - more prevention resources were allocated to more frequently burgled households which remained more at risk (Chenery et al. 1997: 5). The three levels of response were: Bronze (the least resource-intensive), Silver, and Gold (the most resource-intensive for the highest-risk households). The comparison group was the remainder of the area covered by West Yorkshire police, an area larger than that receiving treatment. Victims reported greater satisfaction with the police and were more likely to report having received various types of crime prevention advice from the police. There was an increase in arrests based upon the use of temporary alarms, from 4% of installations to 14% of installations when they were allocated to burgled premises. The main crime rate outcome measure was the 30 percent reduction in burglary incidence relative to the force as a whole. The evaluation examined burglary patterns by known offenders before and after implementation and found no evidence of spatial displacement. Odds ratio calculations were not done for this evaluation due to the lack of raw data.

CAMBRIDGE (UK)

Bennett and Durie (1999) evaluated efforts to prevent residential burglary in two areas (Arbury, with 2665 households; and Castle, with 3024 households) and an overlapping burglary hot spot in Cambridge. Measures were aimed at improving victim security (various measures), increasing guardianship (surveillance measures), and offenders (after-school and youth schemes) (ibid. p. 19). Multiple comparison groups were similar areas, some with similar pre-treatment burglary rates, plus the city as a whole. Outcome measures of burglary incidence and repeat burglaries showed the small reduction in treatment areas were outweighed by larger reductions in the comparison areas. Any reductions could not be attributed to the treatment. Few victims were eligible for security or wanted advice, and of those who received 'treatment', few measures were implemented. Re-analysis of implementation data suggests that, of 171 burglary victims in treatment areas, 3.5% (n=6 victims) received free Keepsafe door locks, 9% (n=15 victims) received loan-alarms, and zero secure alley-gates were purchased. These may well have been the tactics with the strongest prevention mechanisms. Overall, victims declined or did not adopt measures even though project staff implemented them at fairly high rates among those eligible and willing. This re-analysis of implementation rates indicates implementation failure, perhaps more even than that identified in the original report where the evaluators concluded there was "the right medicine but in the wrong dosage" (ibid. p.41).

BALTIMORE, DALLAS AND SAN DIEGO (USA)

The three evaluation sites are shown separately in Tables 5.1 to 5.3 but grouped here for brevity. Weisel, Clarke and Stedman (1999; see also Stedman and Weisel 1999) evaluated police efforts to prevent repeat burglaries across the cities of Baltimore, Dallas, and San Diego. The report notes that "no monetary resources were provided to the cities for

developing or implementing responses.” (Weisel et al. 1999: 19). Police officers were given crime prevention training in each site but “police were not provided with any additional revenue for purchasing crime prevention or intervention tools” (ibid. p.19). The main responses focused on improving information gathered by police at burglary scenes rather than on implementing prevention. Advice leaflets and warning cards were given to victims but there was no provision of security or other measures. The evaluation determined there was implementation failure, noting:

The problem-solving efforts developed and implemented by police personnel in each city were relatively weak. The provision of target hardening or other crime prevention advice to the victim was a very hit-or-miss proposition – depending on the knowledge, interest, and motivation of the officer taking the report.

Weisel, Clarke and Stedman 1999: 113-114.

The result in relation to Dallas was that: “most of the victims in the experimental area received police advice... [but] victims in the comparison area were about as likely as victims in the experimental area to make any changes in behaviour” (ibid. p.97). These comments seemed to be applicable to each site. This is an important lesson regarding implementation, as it can be extrapolated that providing crime prevention information does not necessarily lead to a change in behaviour.

BEENLEIGH, QUEENSLAND (AUSTRALIA)

Budz et al. (2001) evaluated efforts to prevent repeat burglary in Beenleigh, a town of 41,000 people with a burglary rate above the regional average (ibid. p. 2). Three tiers of response were introduced: ‘Stopbreak’ was a package of crime prevention material provided to once-burgled households (623 provided); ‘Hot Dot’ was a response of higher-grade security provided to households burgled more than once (67 such responses provided); ‘Hot Spot’ was a response of a security survey and crime prevention advice offered to residents in high burglary rate areas (580 such responses provided). The evaluation design compared burglary for the year of the project to the preceding year for the treatment areas, neighbouring areas (to capture displacement), and a comparable non-neighbouring area with a similar burglary rate, socio-economic and demographic characteristics (ibid. p.12). Repeat burglaries fell by 15 percent in the treatment area and increased in the comparison areas, but there was no change in the proportion of repeat burglaries. There was no reduction in burglary incidence (burglaries fell 2 percent in the treatment area when one prolific offender was excluded but fell 13 percent in the comparison area).

ASHFIELD, NEW SOUTH WALES (AUSTRALIA)

Taplin and Flaherty (2001) evaluated efforts to prevent burglary in two areas of New South Wales, Australia. The evaluation of Ashfield is included here due to its focus on repeats. Burglary incidence rates in the intervention area were 2.06 per 100 population (not households), and in the comparison area the burglary incidence rate was 2.29 per 100

population⁴. The project targeted some non-residential as well as residential burglary. Burglary victims were eligible for security assessments and an advice package, which was implemented in 70% of victimisations. Immediate neighbours of victims were given a one-page information leaflet. Repeat victims were eligible for target hardening, but low levels meant that only 58 properties were identified as repeat victims and of these 27 received security upgrades (ibid. p.55). This means approximately 1.75% of burglary households and 46.5% of twice-burgled households received security upgrades.

There was more than one comparison area so that used here was Campsie as it had the closest matched pre-intervention burglary rate per capita. Burglary in the intervention site fell 22.1%. However, burglary in the comparison site fell 23.5%. Relative to the comparison area therefore, the observed number of burglaries in Ashfield was 1.8% higher than expected. This was not a completely unpredictable outcome since the main intervention that might be expected to have an effect was that of security upgrades, and a very small proportion of burglary victims received these. There was no displacement to other forms of property crime.

NORWOOD AND TEA TREE GULLY, ADELAIDE (AUSTRALIA)

The intervention was put into place in two (now defunct) metropolitan police subdivisions of Adelaide, Australia: Norwood and Tea Tree Gully with a total population of around 207,000 (Ball Public Relations and Walter, 2002). Five measures were included in the treatments provided to burgled households: a security audit; informal support; referral to other agencies; referral for property marking, and; links to neighbours. Implementation occurred at 31.7% of eligible properties (n = 994 of 3137 burgled properties) which “may be the result of police reluctance (during the first half of the project) to ask victims to participate or a victims’ willingness to ‘get involved’ even if the offer is put in the best possible light.” (ibid. p.9). However, only 61.2 percent of treatments resulted in victims following any security advice (833 interventions; ibid. p.9) - equivalent to a 19.6% implementation of any security. Smaller proportions of victims adopted specific measures: 7.4% (n=233) installed door locks, 8.4% (n = 263) installed window locks, 3.8% (n = 121) installed alarms, and 12.4% (n = 390) followed ‘some advice’ (p. 10). This is a re-analysis of the report data which suggests extremely low implementation rates for key prevention tactics. This strongly indicating implementation failure since a reduction in either repeats or overall burglaries would not be expected based on such low rates of improved security.

The Adelaide project evaluation design incorporated both similar neighbouring areas to assess displacement effects, and non-neighbouring comparable control areas to assess burglary reduction. The evaluation report concludes that the project reduced repeat burglaries relative to the comparison areas (though repeat burglaries remained stable in the treatment area but increased in the control area), while the treatment areas experienced a 31.3 percent increase in burglaries compared to a 16.7 percent increase in the comparison areas (Henderson 2002: 22). There was no evidence of spatial displacement.

⁴ Note that only the post-intervention rates are given in Table 3.1 on p25 of the original report whereas the pre-intervention rates from which the comparison area was identified were calculated for the present study.

LIVERPOOL (UK)

Bowers et al. (2003) evaluated a scheme designed to reduce burglary in an area of 3317 households in Liverpool. Within this target area were three sub areas where crime prevention initiatives were particularly focused, and Bowers et al. analyse these sub areas as a separate entity, which is the data used for the purposes of this review. The non-contiguous comparison area was matched from within the local authority area on socio-economic factors, and conformation to “police beat geography” (ibid. p.6).

There were four schemes in operation (details ibid. p. 2 - 4): target hardening was offered to victims and vulnerable residents (elderly, students, and low income residents); Smartwater property marking was offered to all residents; an alley-gating scheme was introduced; and an intensive supervision scheme for offenders was implemented. The supervision scheme was the only initiative to be introduced across the whole area, the others were in the three specifically targeted areas. Only ten alley-gating schemes (out of 69) were completed in time for inclusion within the evaluation period.

Burglaries within the specifically targeted areas (i.e. where the most crime prevention was implemented) fell by 39.2% relative to the comparison area (32.6% in absolute terms). Repeat burglaries fell from 13 to 5 in the specifically targeted areas, a fall of 61.6%. This compared to an increase of 30% in the comparison area (a change from 30 to 39 repeat burglaries). Hence, repeat burglaries fell by 70.5% relative to the comparison area (ibid. p.45). Theft from cars increased in the area during the intervention, suggesting that some crime type displacement may have occurred, though other crime types did not. Geographical displacement was examined across five buffer rings. Diffusion of benefits was observed in the first of these, then displacement was observed in the second ring before gradually decreasing outwards through the remaining buffer rings (ibid. p.39 – 40).

ORANGE (AUSTRALIA)

The Western Research Institute (2003) evaluated efforts to reduce repeat victimisation in Orange, New South Wales. No specific details on Orange were provided. The intervention was based on Huddersfield’s ‘Biting Back’ model of three tiers of intervention. Victims and non victims were offered a security audit, with suggested security improvements backed up by discount vouchers for security equipment. A second break and enter prompted free security upgrades for the household, as well as a temporary monitored alarm; whilst a third victimisation was responded to by further target hardening measures and increased formal surveillance.

Repeat burglary fell within the target area from 65 to 17, a decrease of nearly three quarters. Burglary overall also fell, by nearly a third since the start of Operation Never Again. The intervention began in 2001 and the data provided for comparison purposes is for a five year period from three years before this, as well as from the year after the intervention began. Here, the burglaries fell by 43% in the treatment area, 57% relative to the comparison area of the remainder of Central West. The authors suggest that “Operation Never Again has been effective in reducing the level of break and enter crime to the state average” (ibid. p.10). There was no evidence of displacement to theft from a person or theft of cars, but theft from cars increased significantly. Odds ratio calculations could not be carried out for this

evaluation as the report only included the percentage change for comparison areas rather than crime figures.

HARTLEPOOL (UK)

Sturgeon-Adams et al. (2005) evaluated a program designed to reduce burglary in 2 areas of South Hartlepool, Belle Vue Estate and Rift House East. In the 2 years prior to the start of the initiative, the average annual burglary incidence rate was 6.1 per 100 households. The program consisted of “seven separate interventions” (ibid. p.8). Target hardening and a security survey were offered to victims who had experienced 2 or more burglaries. Only 24 such cases were identified during the course of the project, of which 15 took up the intervention. Alleygating was provided at hot spot areas, and a number of plug in timers were made available. Initiatives working with young people and young offenders attempted to divert them from crime, although few young offenders attended, and it is not clear that the other young people were at risk of becoming involved with crime. Community initiatives included the setting up of 7 new Neighbourhood Watch schemes and a burglary prevention education campaign. Property marking had taken place within Belle Vue Estate prior to the start of this scheme with an implementation rate of 62%. Property marking within Rift House East was offered to all households during the project, with a 33.7% uptake.

The comparison area was situated to the north of the town centre and had a similar socio-economic demographic, although size and crime rate are not provided in the report. The change in the total number of burglaries was assessed relative to this comparison area, however the change in repeats were compared to the whole of Hartlepool Division 1. The project in Hartlepool also had a 600m buffer zone which contained a similar number of households to the treatment area. Burglary within the intervention area fell by 24.8% in the 2 years from the start of the intervention; in the comparison area burglary fell by 10.7%. Relative to the comparison area, burglary fell by 18.3%. Repeat burglaries in the treatment area fell by 26.3% from the two years before to the two years after the start of the intervention whereas in the whole of Hartlepool District 1, repeat burglaries fell by 15.2%. An addendum to the report examines the burglary levels for a further two years, and concludes that “Hartlepool SDP can show evidence of continuation of its initial success... [t]he immediate surroundings also have benefited” (ibid. p.48) – a diffusion of benefits to the buffer zone was recorded.

BENTLEY AND MORLEY, PERTH (AUSTRALIA)

Cummings (2005) evaluated Operation Burglary Countdown in Western Australia. In Metropolitan Perth as a whole, the annual burglary rate was 5 burglaries per 100 dwellings. The Perth suburbs of Bentley and Morley were selected due to their “high burglary rate” (ibid. p.2). However immediately preceding the start of the intervention, Morley was part of a “police blitz” (ibid. p.28) and the burglary rate reduced in the pre-intervention comparison months as a result. The 12 months before the intervention, and the 12 months of the program in Morley and Bentley were compared to the same time period in the larger Metropolitan Perth. This area was chosen to avoid diffusion and displacement effects, which were examined separately using surrounding estates (ibid. p.32).

Operation Burglary Countdown had a multi pronged approach to reducing burglary in the treatment areas. Burglary victims were offered security audits, but there was a low take up rate with only 17.7% having security audits which “went to completion” (ibid. p.35). Properties neighbouring the victims were given crime prevention leaflets, and 12,000 pamphlets were also distributed in community newspapers (ibid. p.43). Other initiatives (all listed ibid. p.14) included “Eyes on the Street” which used local council workers to identify and report suspicious activity; and a focus on “stolen goods disposal routes”. A property marking day was held, and police targeted known offenders. Additional community initiatives included “truancy action groups” and “diversionary recreational programs for youth at risk”.

Burglaries within Bentley fell by 45.2% in the program year, which was a reduction of 26.2% relative to the comparison area. Morley experienced a fall of 24.2% in the number of burglaries, but relative to Metropolitan Perth, this was an increase of 2%. Repeat burglaries were measured for the intervention areas, but not for the comparison area. In Bentley, repeat burglaries fell from 83 to 42, a reduction of 49%; and in Morley repeat burglaries fell by 59%, from 26 to 11. There was a suggestion that diffusion of benefits from the program may have occurred (ibid. p.32) in Bentley and surrounding areas, though a small amount of displacement may have occurred with Morley (ibid. p.36).

Table 5.2: Features of Treatment and Comparison Groups and Intervention Tactics by Domestic Burglary Evaluation (chronological)

Study (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
Kirkholt Burglary Prevention Project (Forrester et al. 1989, 1990; Farrington 1992)	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)
St Ann’s (Gregson, 1993)	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.
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.
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.
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.
Blackburn - HomeSafe (Webb,	Area of 2,287 households	Mill Hill “neighbouring Incident Location”	Target-hardening security measures (door and window locks, chains, bolts and viewers) offered

Study (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
1996)			free to victims and “vulnerable” (p.53). Crime prevention packs for residents. Publicity.
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.
Lambeth - HomeSafe (Webb, 1996)	Area of 1,240 households	Combination of 2 “comparison areas” (p.61).	Security offered to victims and “vulnerable” (p.60).
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)
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).
Cambridge (Bennett and Durie, 1999)	Castle - 2665 households; Arbury – 3024 households; One hot spot.	Similar non-adjacent local areas and hot spots plus some computer generated treatment 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).
Baltimore - Hot Dots in Hot Spots (Weisel	Three patrol sectors (p.25).	Patrol sectors matched on population, area, environment, housing stock, socio-economic	Warning cards of security advice to victims; Alert cards and warnings for neighbours;

Study (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
et al. 1999)		status (p.25).	Security checks; Free property registration; Police patrols (p.27). All free but no funding for actual security.
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.
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.
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).
Ashfield: Safer Towns and Cities (Taplin et al 2001)	Population of 74,604	Comparison area of Campsie, population of 90375.	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).
Norwood and Tea Tree Gully, Adelaide	Tee Tree Gully plus 3 police subdivisions:	Similar non-neighbouring comparison areas; similar neighbouring areas to measure	Security audit; informal support; referral to other agencies; referral for property marking, and;

Study (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
(Ball Public Relations and Walters, 2002; Henderson 2002)	total population 207,000 (p.7)	displacement.	links to neighbours.
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.
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.
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; treatment for offenders; community development; education and awareness campaign for local residents.
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;

Study (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
			encouraging local council workers to report suspicious behaviour; targeting truancy and providing recreational programs for at risk youths.

Table 5.3: Implementation and Outcome by Domestic Burglary Evaluation (chronological)

Study (Authors)	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Burglary?	Outcome Measure 2 - Reduction in overall burglary?	Displacement? / Other Issues Arising
Kirkholt Burglary Prevention Project (Forrester et al. 1989, 1990; Farrington 1992)	68% for security upgrading; close to 100% for Cocoon Watch.	Yes - Repeat burglary fell to zero within 6 months (recorded crime data). Pattern of burglary reduction linked temporally to security measures.	Burglary fell 60% within 6 months and 75% over 3 years. A reduction of 62.8% relative to the comparison area.	Displacement examined - none found.
St Ann's (Gregson, 1993)	Cocoon Watch achieved 25% coverage (p.7)	Not measured.	9.2% reduction relative to comparison area but increase in absolute terms	Displacement not measured / "The project was not cited in a very high crime area" (Tilley 1993, p.6)
The Meadows (Gregson and Hocking 1993)	55% of victims (187 households) received target hardening and 424 households overall.	Yes - (1) 40.4% reduction in proportion of repeat burglary, (2) Increased mean time to repeats	From 1990 to 1992 there was an increase of just 1% in absolute terms. A decrease of 57.5% in	Displacement not measured.

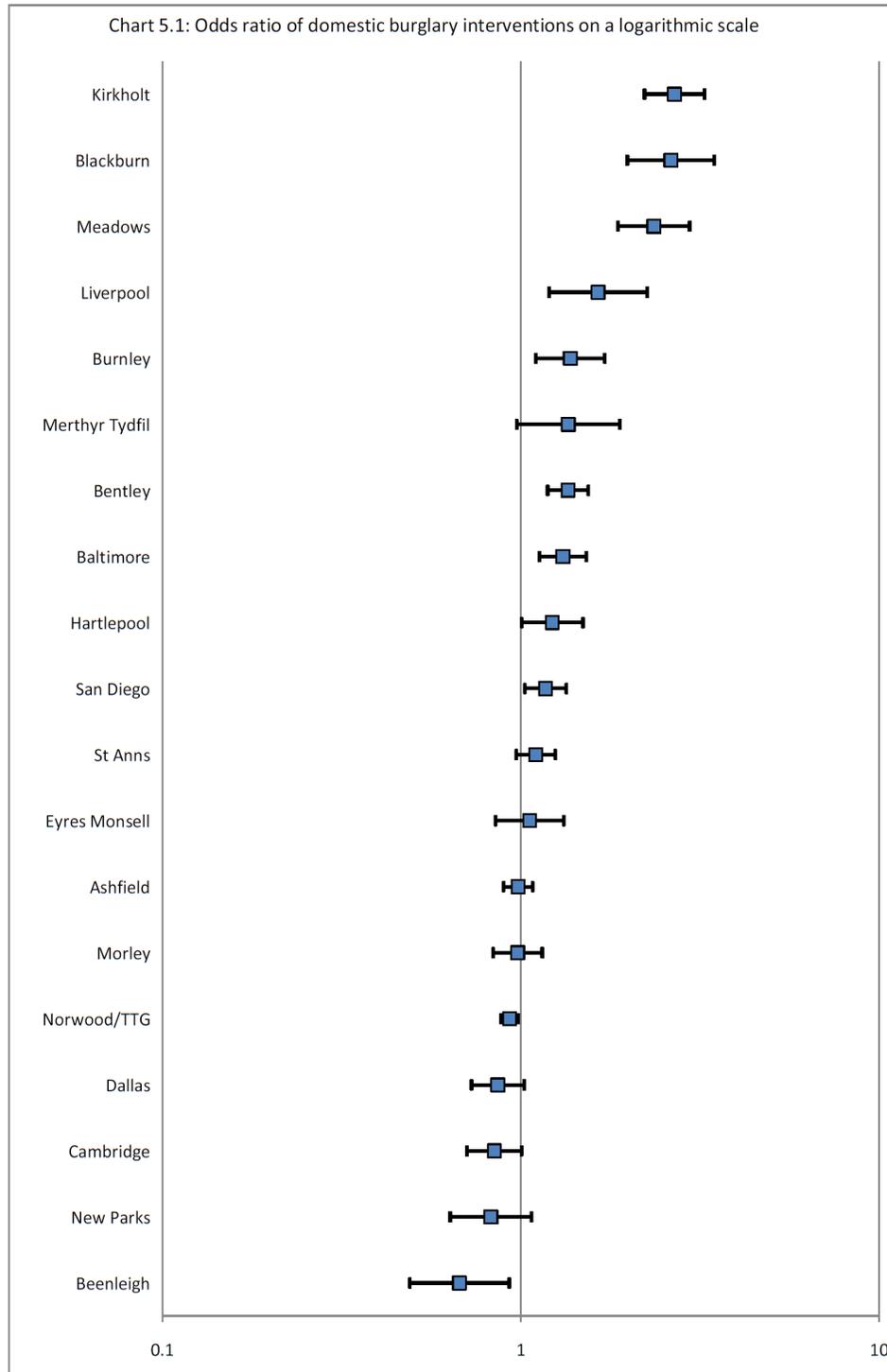
Study (Authors)	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Burglary?	Outcome Measure 2 - Reduction in overall burglary?	Displacement? / Other Issues Arising
		from 81 to 137 days, (3) Properties secured without prior burglary did not benefit	comparison to the rest of the subdivision.	
Eyres Monsell (Matthews and Trickey, 1994a)	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)	Yes - Prevalence up 23.4% but incidence only up 10.4% (p. 16).	5.5% decline in Eyres Monsell relative to comparison area. Increase in real terms.	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)
New Parks (Matthews and Trickey, 1994b)	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)	Yes - repeats halved from 26 in 1992 to 13 in 1993. (p.27).	17.5% increase in New Parks relative to comparison area. Decrease in real terms.	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)	47% of victimized households were ‘Homesafed’.	Yes - Repeats fell from 6.1% (n=16) to 2.9% (n=5) of total burglaries. (p.34).	Yes - Decrease in real terms. Decrease of 62% relative to neighbouring area.	“Increase in other types of property crime in the area”. (p.50)
Burnley - HomeSafe (Webb, 1996)	79% of current year victimised properties were ‘Homesafed’. (p.15). Other initiatives were	Yes - Repeats fell from 9.6% (n=18) to 8.7% (n=12). (p.35)	Yes - Decrease of 27% relative to comparison area. Decrease in absolute terms	Overall acquisitive crime decreased in Burnley Wood, though some types did increase

Study (Authors)	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Burglary?	Outcome Measure 2 - Reduction in overall burglary?	Displacement? / Other Issues Arising
	run in the area at the same time.		of 22%.	slightly (non-domestic burglary, theft from motor vehicles)
Lambeth - HomeSafe (Webb, 1996)	Unknown implementation rate: only 70 properties were 'Homesafed'. Operation Bumblebee also ran in this area during the summer months.	42% repeats in 1994, only 1 repeat in 1995. Note this does not exactly match the time period examined in the rest of the evaluation.	Yes - Decrease of 79.3% relative to comparison area. Decrease in absolute terms of 70%.	The scheme was halted twice due to "severe implementation problems" (p.60). Displacement not discussed.
Merthyr Tydfil - HomeSafe (Webb, 1996)	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.	Yes - Repeats fell from 17.9% (25 incidents) to 2.6% (2 incidents). (p.35) Not measured for comparison area.	Yes Decrease of 26.3% relative to comparison area. Decrease in absolute terms of 45.8%.	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)	Interviews with victims "suggest implementation a factor in any continuing repeats" (1997; p. 17)	Victims more satisfied; Increased arrests from alarms at victimized properties. Reduction over time in number of silver and gold responses suggests reduced repeat burglaries.	30 percent reduction in burglary incidence relative to comparison group.	Displacement examined - none found.
Cambridge (Bennett and Durie, 1999)	Very low for key tactics: 3.5% (6 of 171 victims) received free Keepsafe door locks;	No – similar or greater reductions in comparison areas.	Increase of 13.8% relative to comparison area, though a 4% reduction in absolute	"The right medicine but the wrong dosage" (p.41). Implementation failure.

Study (Authors)	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Burglary?	Outcome Measure 2 - Reduction in overall burglary?	Displacement? / Other Issues Arising
	“Some” victims acted on security advice; 9% of victims (n=15) received loan-alarms; 0% of visited victims required alley gates (p.36).		terms.	
Baltimore - Hot Dots in Hot Spots (Weisel et al. 1999)	Few process measures given: police ‘distributed’ cards and ‘alerted’ neighbours (p.27).	No.	A 23.7% reduction relative to comparison area, but probably spurious - no explanation for it	Weak treatment (advice) suggests theory failure produced implementation failure (no strong preventive tactics introduced).
Dallas - Hot Dots in Hot Spots Weisel et al. (1999)	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).	No: Victim survey (51% response rate) showed no change relative to comparison group.	Burglary increase of 16% relative to control (p.91)	Weak treatment (advice) suggests theory failure combined with implementation failure (no strong preventive tactics introduced).
San Diego -Hot Dots in Hot Spots Weisel et al. (1999)	Few process measures available; Changes in police personnel “challenge to implementation” (p.43); Police “skeptical” about repeat burglaries (p.43)	No.	A reduction of 24.7% relative to comparison (p.92), but probably spurious (no explanation for it).	Weak treatment (advice) suggests theory failure produced with implementation failure (no strong preventive tactics introduced).
Beenleigh - Lightning Strikes Twice (Budz et al. 2001)	Victims more likely than controls to use warning stickers (45% v 11%), property marking (42% v 12%), inventory lists (34% v 13%),	Yes – Repeat victims fell 16% and repeat incidents 15% in treatment area and increased in comparison areas.	No – burglaries increased by 9.9% relative to comparison group. Report suggests repeats may have displaced to other	Displacement measured - none found. / Evaluation difficult because: “It was difficult to distinguish possible project effects from... random or seasonal

Study (Authors)	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Burglary?	Outcome Measure 2 - Reduction in overall burglary?	Displacement? / Other Issues Arising
	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).		households within treatment group area but provides no evidence (p.21).	fluctuations.” (p. 14) and; Project was in an area with “low incidence of repeat victimization” (p. 14)
Ashfield: Safer Towns and Cities (Taplin et al. 2001)	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).	Although the number of repeatedly targeted residences, and the number of repeat incidents both fell, the proportion of repeats stayed the same. (p.28)	Burglaries fell in absolute terms by 22.1%. However, relative to the comparison area, burglaries showed an increase of 1.8%.	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 (Ball Public Relations and Walters, 2002; Henderson 2002)	Treatment (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.	Repeats reduced relative to control (but repeats stable in absolute terms)	No – burglary increased by 7.5% relative to comparison area.	No evidence of spatial displacement.
Liverpool (Bowers et al.,	Implementation rate not discussed.	Yes - Repeat burglaries fell by 61.6% in the	Yes - Burglaries fell in the targeted treatment area by	Report states that “theft from car significantly increased in the area.

Study (Authors)	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Burglary?	Outcome Measure 2 - Reduction in overall burglary?	Displacement? / Other Issues Arising
2003)		targeted treatment area, a 70.5% fall relative to the comparison area.	32.6% in absolute terms; 39.2% relative to the comparison area (p.45).	There was no significant switch to theft from a person, taking a vehicle without the owners' consent or theft of car" (vii).
Orange(Western Research Institute, 2003)	"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.	Yes – repeat burglaries fell by 74.2% in the treatment area.	Burglaries have fallen by 32% since the start of the intervention (p.9).	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)	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.	Yes - Repeat burglaries fell by 26.3% in the treatment area.	Burglaries fell in absolute terms by 24.8%. Relative to the comparison area burglaries fell by 18.3%.	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)	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.	Yes - prevalence of repeat burglaries fell by 49% (from 83 to 42) in Bentley and 58% (from 26 to 11) in Morley (p.36).	Burglaries fell in Bentley by 45.2% in absolute terms; 26.2% relative to the comparison area. In Morley, burglaries fell by 24.2% in absolute terms, but increased by 2% relative to the comparison area.	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).



COMMERCIAL BURGLARY

Three eligible commercial burglary studies were identified for inclusion from the UK and USA. Two further studies identified were excluded (see Appendix A). Table 5.4 presents a summary of the key features of the programmes. Table 5.5 provides an at-a-glance summary of implementation issues and outcome measures for each study. Table 5.1 shows an overview of the success of the programmes according to the counterfactual calculations. Finally, Chart 5.2 provides a visual representation of odds-ratio calculations which were carried out to compare the effect sizes, based on incidence.

MULTNOMAH (USA)

Pearson (1980) evaluated efforts to prevent non-residential burglary in Oregon County. Approximately half of the businesses (including for example churches and schools as well as commercial premises) in the intervention group, and all of the comparison group had been “burglarized at least once in the one or two month period prior to the survey” (ibid. p.xii). The target and comparison groups were “equivalent in terms of their geographic distribution and burglary type” (ibid. p.11). The intervention consisted of a security survey with a “thorough, internal and external assessment of physical security strengths and weaknesses” (ibid. p.2). A report was then mailed to participants giving details of the weaknesses and suggested improvements. Suggestions included for example locks, door improvements, key control and skylight security (a full list is available on page 25 of the report). 31.7% of the security suggestions were partially or fully complied with: the figure varied depending on the suggestion from a high of 60.4% compliance with money handling suggestions to a low of 0% compliance with suggestions relating to “iron work” (ibid. p.25).

The outcomes were separated into those for all the targeted businesses, and for those deemed “high risk” (ibid. p.15), defined as those experiencing 2 or more “pre-survey burglaries” (ibid. p.15). All targeted businesses experienced an increase of 3.9% in burglaries. However, in the comparison group, burglaries increased by 22%. Hence, relative to the comparison group, burglaries fell by 14.9% in the intervention group. High risk businesses experienced a 61% fall in burglaries in real terms: this was a fall of 18.5% relative to the comparison group. Displacement was not measured.

LEICESTER (UK)

Tilley and Hopkins (1998) evaluated efforts to prevent repeated commercial burglary against small businesses (those with less than 25 employees) in the West End and Belgrave areas of Leicester, discussed here as a single entity. Repeat victims had measures tailored to the business including: temporary silent alarms; covert CCTV; forensic traps designed to obtain footwear marks; and hidden movement detectors to trigger an audible alarm. Those businesses that had 10 or more incidents were defined as chronically victimized, and were given risk assessments. These businesses were given access to “a little money” to implement suggested measures. Businesses that were deemed the “most severely affected” by customer theft and violence were given an advice pack.

The treatment group was 1381 businesses, with 483 of these being successfully surveyed both before and after the intervention. Among the 42 chronically victimised businesses that were visited, 28 were “receptive” to advice on security (ibid. p.5), an implementation rate of 66.7%. However, there is no information on the implementation rate more generally.

The comparison area was larger, being the remainder of the police subdivisions in which the treatment areas were located. Non-domestic burglary fell in the intervention area by 41% and also fell 32% in the comparison area. Hence burglary in the treatment area fell 19.7% relative to what would be expected based on experience in the comparison area. Prevalence dropped in the intervention area only 36% (despite incidence dropping 41%). The evaluation reports this as a 7.7% reduction in crime concentration (average victimisations per target). This information is not available for the comparison area.

MERSEYSIDE (UK)

Bowers (2001) evaluated a scheme “aimed at reducing small business crime” (ibid. p.23) in Merseyside, with a particular focus on burglary. The intervention was available to 140 survey respondents (from 470 successfully surveyed from a stratified sample of 1000; total population of 2517 businesses with less than 25 employees) who were identified as being at high or medium risk. Particular weight when calculating risk was given to those businesses that had experienced repeated burglary victimisations. The intervention consisted of security advice including on: layout; ‘weak spots’ (for entry, or parts not under surveillance); good practice (e.g. keeping the minimum amount of money in the till; taking greater care with keys); and on target-hardening.

70% of the 470 businesses were surveyed both before and after the intervention including 105 of the 140 intervention businesses. 60% of these had introduced target hardening, including burglar alarms, CCTV, roller-shutters and occasionally detection devices. Seventeen businesses received up to £1500 as 50% funding for security measures.

The comparison group was businesses that were not eligible for the intervention, that is, those at low crime risk. However, despite the comparison between a high risk and low risk group, the evaluation used a simple pre- post- comparison- group design rather than a regression discontinuity analysis. This is therefore the weakest design included here, as it risks regression to the mean. Therefore, caution must be taken whilst interpreting the following findings. In the treatment group, the prevalence of burglary fell from 32.4% to 13.3% after the intervention. This was a 55% reduction relative to the control group where the prevalence of burglary had also fallen from 11.4% to 10.5%. While sample sizes were small, the prevalence of repeat burglary fell from 67.6% to 26.7% in the treatment group, a drop of 61% relative to the comparison group where the prevalence of repeats increased from 37.5% to 38.1% of those surveyed. Outcome data was collected on nine crime types overall (burglary; attempted burglary; criminal damage; shoplifting; employee theft; fraud and forgery; robbery; assault, and; theft from a customer) and there was some evidence that other crime types were reduced in the treatment compared to the comparison area. There was no evidence of spatial, tactical or crime-type displacement (ibid. p. 41).

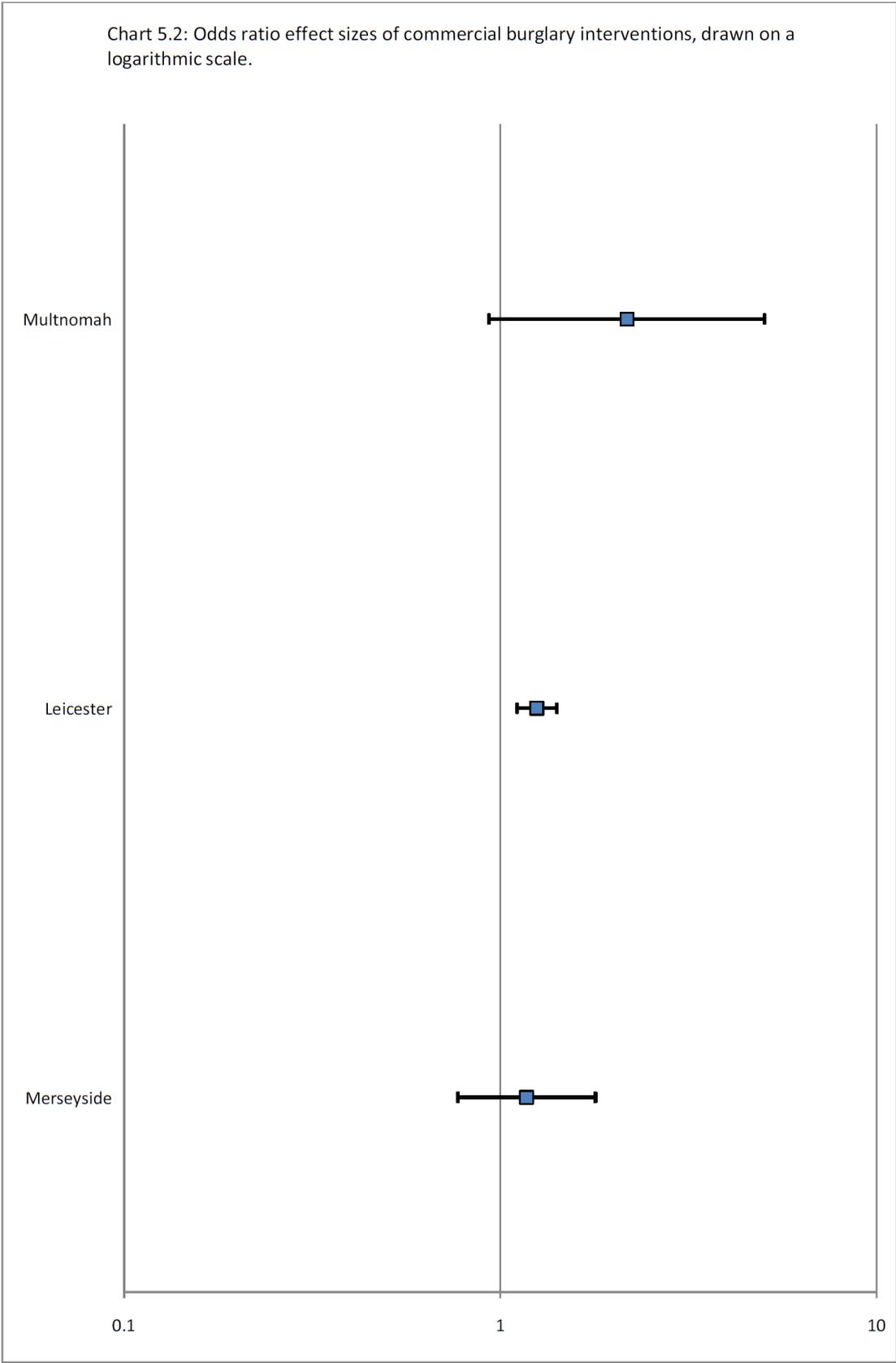


Table 5.4: Features of Treatment and Comparison Groups and Intervention Tactics by Evaluation (chronological)

Title (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
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)
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)
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.

Table 5.5: Measures of Implementation and Outcome by Evaluation (chronological)

Title	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Victimization?	Outcome Measure 2 - Reduction in overall crime? (source)	Displacement? / Other Issues Arising
Multnomah (Pearson, 1980)	31.7% compliance with security suggestions overall.	High risk businesses (those with 2 or more victimisations prior to the survey) saw a 61% fall in burglaries (from 82 to 32), this was a fall of 18.5% relative to the comparison group (where burglaries also fell, from 23 to 11).	The intervention group experienced an increase of 3.9% in burglaries, however relative to the comparison area there was a decrease of 14.9%.	Not measured
Leicester (Tilley and Hopkins 1998)	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).	Concentration of burglaries fell from 1.83 to 1.66 (p.24).	In the West End, non-domestic burglary saw a drop in incidence of 40% in absolute terms. No change relative to the rest of the subdivision. In Belgrave, non domestic burglaries fell 43% in absolute terms, a 30.4% drop in comparison to the rest of the sub division.	At the time of the report, non-domestic burglary was not separated out from commercial burglary in police figures (p.9)
Merseyside (Bowers 2001)	Target hardening implementation rate of 60% for eligible businesses.	There was a reduction of 61% relative to the comparison group.	The number of burglaries fell from 33 to 14 in the intervention group, a fall of 55% relative to the non-intervention group.	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

Title	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Victimisation?	Outcome Measure 2 - Reduction in overall crime? (source)	Displacement? / Other Issues Arising
				other crime types also experienced a reduction in prevalence.

SEXUAL VICTIMISATION

Five eligible sexual victimisation studies were identified for inclusion in this review, all from the USA. A further seven were excluded. The full text of one conference poster presentation, by Calhoun et al. (2001) could not be located. A list of the excluded studies can be found in Appendix A. Table 5.6 presents a summary of the key features of the programmes. Table 5.7 provides an at-a-glance summary of implementation issues and outcome measures for each study. Table 5.1 shows an overview of the success of the programmes according to the counterfactual calculations. Finally, Chart 5.3 provides a visual representation of odds-ratio calculations which were carried out to compare the effect sizes. Note that unlike the other crime type these odds ratios were necessarily based on prevalence, which was the measure commonly employed across the reports. The studies are here referred to by the name of the lead author to distinguish between them. This runs contrary to the other crime types, where studies are identified by location, and reflects the anonymity granted to the locations in many of these studies.

HANSON, 1993 (USA)

Hanson and Gidycz (1993) evaluated a program designed to reduce the risk of sexual assault with a group of college women, both victims and non victims. A victim was defined as a woman who had experienced a sexual assault at any point from age 14. The intervention included information on sexual assault; a 'Rape Myths and Facts' worksheet; a video demonstrating "events leading up to an acquaintance rape" (ibid. p.1047); and a discussion about protective measures.

Prevalence during the follow up period of 9 weeks for victims was slightly lower in the treatment group than the control. 24.5% of prior victims in the treatment group experienced a further form of sexual victimisation during the follow up, compared with 29.8% in the control group. The authors state that "this program was not effective in reducing the incidence of sexual assault among women with a history of victimisation" (ibid. p.1050). The authors were more optimistic about the findings with women without a history of victimisation, as prevalence of sexual assault was more than twice as high in the control group during the follow up period for these women, suggesting that "this program could be useful if administered early" (ibid. p.1050).

BREITENBECHER, 1998 (USA)

Breitenbecher and Gidycz (1998) evaluated a program designed to reduce the risk of multiple sexual victimisation of women, using a modified version of an intervention developed by Hanson and Gidycz (1993). The modification was that participants were informed that victims were at higher risk of re-victimisation, and given additional education with respect to the risks of revictimisation. There were nine weeks between the intervention and the post-test assessment. Participants were female undergraduate students, both victims and non-victims.

Amongst the women who had been previously victimized in their lifetime before the intervention, the prevalence rate of repeat victimisation in the nine weeks was 28.0% in the intervention group and 28.6% in the control group (Breitenbecher and Gidycz, 1998: 481), making the intervention group 2% less likely to be victimized relative to the expected rate

based on the control group. There were no differences in the rates of victimisation between intervention and control group for either victims or non-victims. With respect to the inability of the programme to prevent repeat victimisation, the authors conclude that “[t]he most straightforward interpretation of this finding is that the program was simply not powerful enough to break the cycle of victimization.” and that “[a] second possible interpretation is that the mechanisms of revictimisation addressed in this program (i.e. dissociative pathology, depression, anxiety, learned helplessness, dependency, and low self-esteem) are not, in fact, related to revictimization.” (ibid. p. 485).

MARX, 2001 (USA)

Marx et al. (2001) evaluated the impact of 2 two-hour risk reduction training sessions for victims of sexual victimisation. The training included: information about the extent and nature of sexual victimisation; a video and discussion of an events leading to an acquaintance rape and identification of risk-factors and protective factors relating to the scenario; how to avoid and react to risky situations. The sessions were conducted with groups of 5 to 10 previously victimized women, randomly assigned to the treatment or control group.

The pre-test measures covered victimisation since age 14, and the post-test a two-month follow-up. The two main outcome measures were of prevalence, one for sexual victimisation and one specifically focusing on rape. In the post-test timeframe, 21% of the intervention group and 32% of the control group were sexually victimized. This represents a 36% lower revictimisation rate, with lack of statistical significance due to the small samples involved. With regard to rape, 12% of the intervention group and 30% of the control group were raped in the two months measured in the post-test. That is, rape prevalence was 58% lower in the intervention group although the small sample sizes again mean the results were not statistically significant. The report concludes that “the intervention and control groups did not differ in their rates of victimization” (p. 28) although elsewhere it concludes the “program may be effective in reducing the incidence of sexual assault victimization” (ibid. p.25).

GIDYCZ, 2001 (USA)

Gidycz et al. (2001) evaluated an acquaintance rape prevention program. The program included men and women attending university, and consisted of educational material seeking to heighten awareness and reduce risk of offending among men, and to heighten awareness and reduce risk of being victimised among women. The method of allocation of participants is not stated. The post-test was conducted nine weeks after the intervention.

Outcomes were measured in terms of prevalence (number of victims rather than incidents) for any type of sexual victimization, and were slightly higher in the intervention group. Among those who had been victimized prior to the intervention, the prevalence of repeat victimization was 36.9% in the intervention group and 32.9% in the control group. Using rape (rather than any type of sexual victimization) the prevalence of rape amongst those previously victimized was 9.5% in the experimental group and 11.4% in the control group. There were no statistically significant differences between intervention and control group for any outcome measure.

DAVIES, 2006 (USA)

Davis et al. (2006 a, b) evaluate the effects of educational workshops on the risk of sexual assault. The workshops, conducted in New York and Seattle, consisted of 2 two-hour educational sessions taught to groups of 3 to 5 women. Eligible participants were women who had experienced sexual victimization at any point in their lives. The workshops were intended to “(a) increase knowledge of situations likely to lead to sexual assault, (b) teach communication skills and practical strategies for avoiding unwanted sexual contact, (c) teach recognition of risky interpersonal situations, and (d) teach assertiveness in social situations.” (Davis et al. 2006a: 20).

Sixty percent of the sample of women were recruited from rape/sexual assault programmes (crisis centres), with others via advertisements, shelters and referrals from other participants. Of 84 women, 46 were assigned to the treatment and 38 to the control group. Assignment was random but there were thirteen exceptions (ibid. p. 42).

The relevant outcome measure was a comparison of the proportion of women victimised in the six months before and after treatment. In the treatment group, victimization prevalence fell from 33% to 26% of women. In the control group, victimization prevalence fell from 32% to 29%. There was no statistically significant difference between treatment and control group. The actual numbers of women in the post-treatment samples, and the incidence of victimisation, are not given in the report. This evaluation is therefore not suitable for inclusion in the odds ratio calculations.

Table 5.6: Features of Treatment and Comparison Groups and Intervention Tactics by Evaluation (chronological)

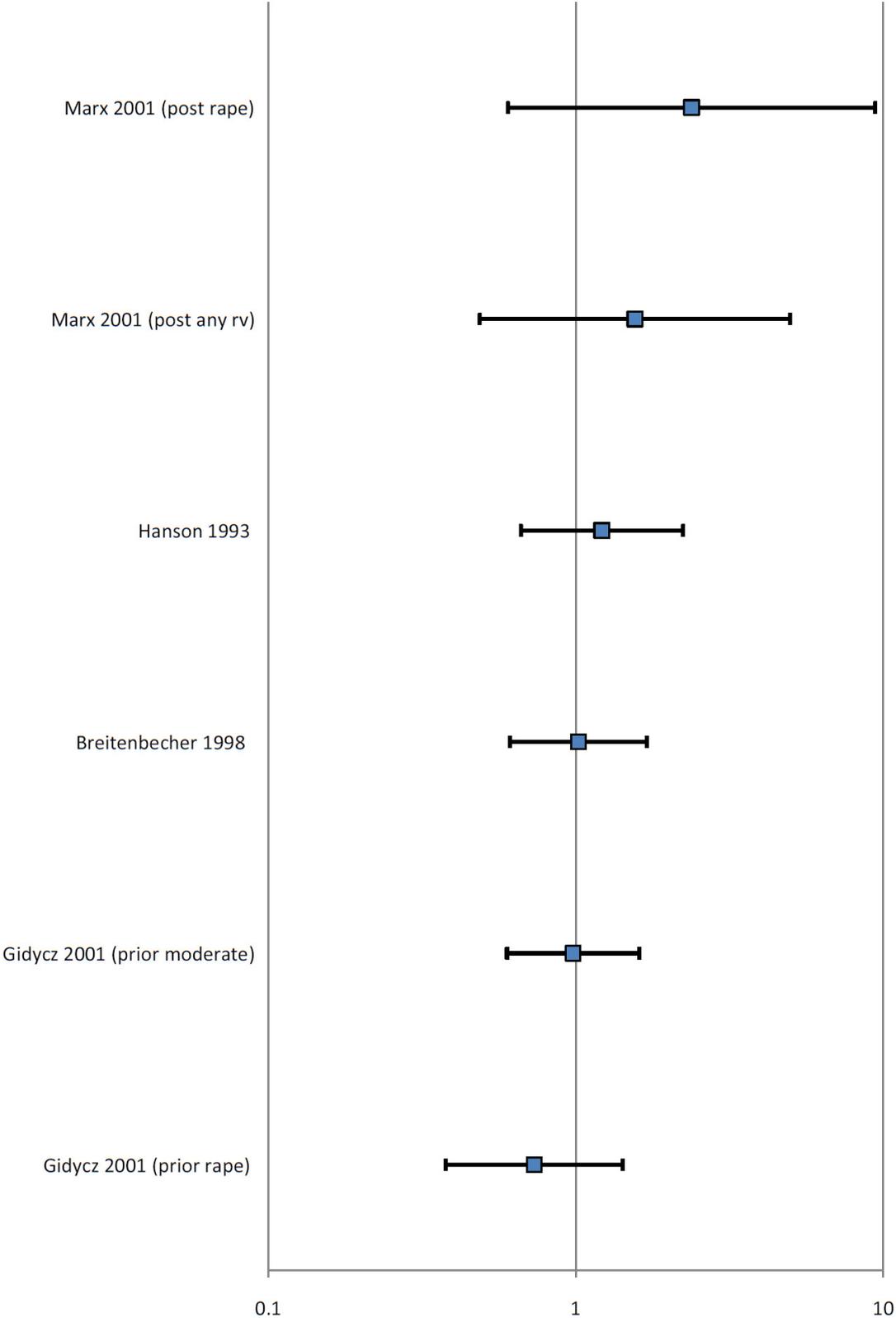
Title (Authors)	Treatment Group	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
Hanson, 1993	181 female undergraduate psychology students, of which 59 were prior victims of “moderate sexual victimization” and 43 were prior victims of “severe sexual victimization” (p. 1050)	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.
Breitenbecher, 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.
Marx, 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.
Gidycz, 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.
Davis, 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).

Table 5.7: Measures of Implementation and Outcome by Evaluation (chronological)

Title	Implementation - Measures and Issues	Outcome Measure 1 – reduction in repeat victimization?	Outcome Measure 2 – reduction in overall victimization?	Other Issues Arising
Hanson, 1993	The way in which participants were allocated to treatment or control sessions was not discussed.	The intervention group was 17.8% less likely to be revictimised relative to the control group in the 9 week follow up.	Not measured	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 treatment group.
Breitenbecher, 1998	“Experiment sessions were randomly designated as either treatment or control sessions.” (p.475)	The treatment group was 2% less likely to experience a sexual victimisation during the follow up period of 9 weeks relative to the control group. (p.481) Not statistically significant.	Not measured	Not discussed.
Marx, 2001	Random assignment to intervention and control groups.	Victimisation was examined in the 2 months after the intervention. When all levels of victimisation were examined, the intervention group was 36% less likely to be revictimised relative to the control group. When rape victimisation was examined, the intervention group was 58% less likely to be raped than the control group.	Not measured	“It is unclear whether the intervention or pre treatment risk recognition was more important in determining subsequent victimization” (p.30)
Gidycz, 2001	The way in which participants were	23.1% of the intervention group were victimised compared to 19% of the	36.9% of those previously victimized in the treatment	Men were also included in this

Title	Implementation - Measures and Issues	Outcome Measure 1 – reduction in repeat victimization?	Outcome Measure 2 – reduction in overall victimization?	Other Issues Arising
	allocated to treatment or control sessions was not discussed.	control group in the 9 weeks following the intervention.	group experienced at least one more victimisation in the 9 weeks after the intervention; compared to 32.9% of the control group.	program in an attempt to reduce self reported sexual aggression
Davis, 2006	Random assignment to intervention and control groups with 13 exceptions.	Prevalence was measured in the 6 months before and after the intervention. Prevalence fell from 33% to 26% in the treatment group; in the control group prevalence fell from 32% to 29%. (p.52)	Not measured	Not discussed.

Chart 5.3: Odds ratio effect sizes of sexual victimisation interventions, drawn on a logarithmic scale.



DOMESTIC VIOLENCE

Just one eligible domestic violence intervention was identified for inclusion in this systematic review. Although domestic violence has frequently been the focus of research studies, the offender is often the central character engaged with in these projects. A list of excluded interventions can be found in Appendix A. Although there is only a single case here, the same tables are employed as for other crime types. This is to allow comparisons to be easily drawn across the four crime types discussed here. Therefore, Table 5.8 presents a summary of the key features of the programme. Table 5.9 provides an at-a-glance summary of implementation issues and outcome measures for the study. Table 5.1 shows an overview of the success of the programme according to the counterfactual calculations.

NDV PROJECT (AUSTRALIA)

Morgan (2004) evaluated efforts to prevent repeat domestic violence in two areas of Australia: Port Adelaide and South Coast. The tiered intervention was based on the Killingbeck model (see Hanmer et al., 1999)⁵. Similarly to Killingbeck, there was an equal focus on victim and offender, but there were distinct differences in the NDV model, notably to allow “room for a wide range of gender and age combinations of intimate or family violence” (Morgan, 2004: 20). Interventions consisted of letters and information sent to victim and offender; a personal safety plan for the victim; directed police patrols; and in certain cases a duress alarm was provided. In all cases where children were present, the “Child Abuse Hotline” (ibid. p.5) was notified. During the intervention period, there were 1269 incidents at the three main levels, of which 85% received a letter and victim information kit. 73% were provided with a safety plan, though interestingly the report notes that “male victims were perceived not to need the same kind of safety plan that female victims received” (ibid. p.44). Unusual cases were classed as ‘level X’ and were provided with a more tailored intervention, however specific details and implementation rates were not recorded for these cases.

The comparison area was a larger compilation of “other metropolitan areas” (ibid. p.60). The outcome was measured as a change in the number of domestic violence calls to the police. Overall calls fell by 12.3% in the treatment area, and fell by 5.3% in the comparison. Repeat calls fell by 12.1% in the treatment area and 1.7% in the comparison area. The report states that the “result is consistent with a positive impact of NDV” (ibid. p.61). Displacement was not mentioned in the report. The odds ratio was 1.09, suggesting a small positive outcome.

⁵ Excluded from this review due to a lack of comparison area.

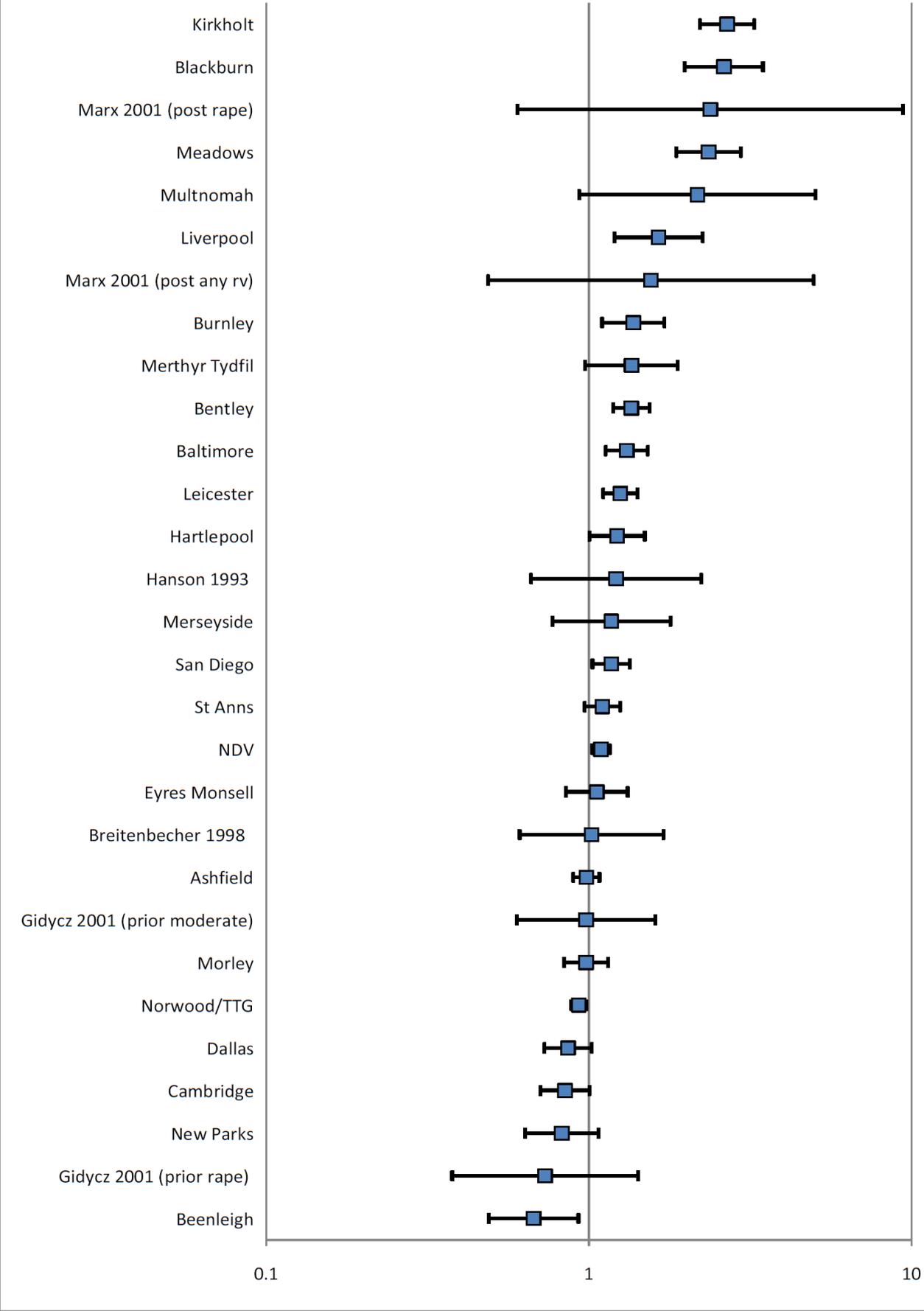
Table 5.8: Features of Treatment and Comparison Groups and Intervention Tactics by Evaluation (chronological)

Title (Authors)	Treatment Area	Comparison Group(s) (Any differences to treatment area?)	Intervention Tactics
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.

Table 5.9: Measures of Implementation and Outcome by Evaluation (chronological)

Title	Implementation - Measures and Issues	Outcome Measure 1 – Reduced Repeat Victimization?	Outcome Measure 2 - Reduction in overall spouse abuse?	Displacement? / Other Issues Arising
NDV Project Evaluation (Morgan, 2004)	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.	Yes - Repeat calls fell by 12.1% in the treatment area and 1.7% in the comparison area	Yes - Domestic violence calls to police fell 12.3% in absolute terms, 8.2% relative to the comparison area.	None discussed.

Chart 5.4: Odds ratio of repeat victimisation prevention programmes, on a logarithmic scale



DISCUSSION

Chart 5.4 (above) presents an at-a-glance view of the effect sizes of the repeat victimisation prevention programmes which were included in this systematic review (and provided data which were suited to the odds ratio calculations). This clearly shows the success which extends across all four crime types examined herein. The weighted mean effect size, calculated using the random effects model, is 1.212 (low = 1.093, high = 1.345) indicating that overall, programmes which focus crime prevention resources on existing victims of crime are successful. The extent to which it can be concluded that repeat victimisation prevention works to reduce crime is discussed below. It should be noted that with the exception of sexual victimisation, all of the success criteria discussed relate to incidence. When examining repeat victimisation, prevalence is the directly relevant measurement. Therefore, the conclusions relate to the success of the programmes in reducing crime incidence, not repeat victimisation per se.

WHAT WORKS

The evidence suggests that preventing repeat victimisation within both domestic burglary and commercial burglary works to reduce crime. In the case of domestic burglary, 12 of the 19 studies included in the odds ratio calculations had an odds ratio of greater than one, suggesting success in these cases – although three of these had confidence intervals which indicated uncertainty about this measure. The unsuccessful studies could generally trace failure back to specific causes, usually problems with the implementation of the intervention. Interestingly, in four cases (Baltimore, San Diego, Beenleigh, Morley), there was a disparity between the change in repeat victimisation and overall incidence of domestic burglary. This could call into question the presumed mechanism (that by preventing at risk households from becoming disproportionately victimised, overall incidence will necessarily fall). However, this is unlikely to be the case as caution must be taken with the measure of repeat victimisation, which refers solely to the treatment area with no comparison group measurement. There were a further two domestic burglary interventions which were not included in the odds ratio calculations. The first of these, the Huddersfield Biting Back project, showed success in relation to the comparison area, but the raw data necessary for calculating the odds ratio were not available. The second, Lambeth, was excluded because the severe implementation problems meant that the success was unlikely to be linked to the intervention, and would thus have unfairly skewed the chart if it had been included. No information in these studies contradicts the conclusion that targeting repeat victimisation within domestic burglary can be successful.

There were far fewer repeat commercial burglary victimisation prevention programmes eligible for inclusion in this systematic review. All three were included in the odds ratio calculations and deemed to be successful according to these, although the confidence intervals were varied. There was no disparity between the change in repeat victimisation and the incidence of commercial burglary. There is no other evidence about the success of commercial burglary interventions more generally that would cast doubt on these findings.

WHAT'S PROMISING

The single spouse assault intervention which was eligible for inclusion here was successful. However, by itself this does not provide evidence that this works. Other work which was not eligible for inclusion here does suggest that focusing on domestic violence victims can have a positive impact (see for example Farrell and Buckley, 1999). However the preponderance of domestic violence research has focused on the offender (see for example the Minneapolis Domestic Violence Experiment and replications, Sherman, 1992).

The repeat sexual victimisation prevention programmes certainly show promise. However the evidence in the few studies is mixed, and the follow up periods for measuring the success is generally very limited, which restricts the conclusions which can be drawn. The confidence intervals are large, and coupled with the conclusions from the reports themselves, there is a suggestion that further refinement is needed, both of the interventions and of the tools used to measure aspects of the risk factors revictimisation (Hanson and Gidycz, 1993). More research is needed with victims of this crime type.

WHAT DOESN'T WORK

Perhaps surprisingly, the evidence in this systematic review points overwhelmingly to the success of repeat victimisation prevention programmes across crime types. However, evidence is limited in some crime types, and indeed, non-existent in others. With this in mind, further work is desperately needed to fill this gap in knowledge and understanding.

Chapter Six: Realist Synthesis Methodology

Science is built up with facts, as a house is with stones. But a collection of facts is no more a science than a heap of stones is a house.

- Jules Henri Poincare

The notion of using scientific realism in aspects of evaluation is not a new idea, and notable realist evaluations in the field of crime prevention include Laycock (1985; 1992) and Foster and Hope (1993). The concept of a realist synthesis as a methodology for meta-evaluation was first put forward as “a new method of conducting systematic reviews of the evidence base” in a working paper by Pawson et al. (2004). Due to the novelty of this realist synthesis methodology, there is little in the way of alternate examples to draw on, and this methodology therefore follows many of the suggestions and processes laid out in that working paper. Each stage is explained here in relation to the realist synthesis process used for examining the studies and theories of repeat victimisation prevention. A flow chart summarising the process can be found at the end of this chapter.

DEFINING THE SCOPE OF THE REVIEW

IDENTIFICATION OF THE RESEARCH QUESTION

In broad terms, the aim of the realist synthesis was the same area as that of the systematic review – namely to examine the effectiveness of programmes designed to prevent repeat victimisation. The systematic review showed that many of these programmes were effective at reducing crime incidence. However, the different features of successful and unsuccessful programmes were not examined within the systematic review. This synthesis therefore had a solid base to work from to establish what factors were associated with successful outcomes. A number of interventions did not produce results of a type suited for inclusion in the systematic review, commonly due to issues with the implementation of the intervention which resulted in a lack of availability of appropriate data, although occasionally because of an inappropriate initial evaluation design. However, exclusion of such studies from the systematic review did not preclude their examination as part of the realist synthesis process. Indeed, the very difficulties which many such studies encountered which prevented their inclusion within the systematic review are the very same issues which provide a wealth of data and explanations for the purposes of the realist synthesis.

Reduction of repeat victimisation has been a Key Performance Indicator for police forces in the UK since the mid 1990s (Laycock, 2001). In theory, this approach to crime reduction should have a disproportionate effect on crime figures as it follows the Pareto Principle – a small number of victims experience a large number of crime events. There are a variety of approaches for reducing repeat victimisation, although target hardening for domestic burglary is perhaps most commonly associated with this approach. This realist synthesis examined the attempts to prevent crime by focusing on repeat victimisation, and tracked theories, implementation issues, and mechanisms for each crime type as appropriate.

The available evaluations covered a complex range of repeat victimisation interventions. The crime type; context; target groups; and techniques implemented all varied widely. Specific

combinations of these were rarely duplicated except where the intervention explicitly set out to repeat an earlier model – and even then some differences were inevitable. This made the possibilities for analysis seemingly endless, and thus a narrower focus was needed. The explanatory basis for this synthesis focused on what works, for whom, and in what circumstances (as per Pawson and Tilley, 1997).

CLARIFICATION OF THE PURPOSES OF THE REVIEW

In keeping with the realist philosophy, stakeholders were consulted at the development stage of the planned synthesis. This served three key purposes. Firstly, this involvement enabled refinement of the research questions. Secondly, consultation revealed current assumptions and practices which could form the basis of the theories for testing. Finally, involving stakeholders ensured that the end product would be of interest and potential use to key groups.

Thanks are due to the stakeholders who offered words of wisdom, comments, and other input at this stage of the realist synthesis: Pete Bumpus and Rebecca Thompson from Leicestershire Police; Alan Edmunds from Hampshire Police; Sylvia Chenery from Applied Criminology Associates; Mark Wilson from Victim Support, and Robert Street from the Crime Prevention Group Research and Analysis Unit, Home Office.

The consultation process began with initial contact via email, to explain the purpose of the research, and to ask for some time to discuss some related issues. The discussions were originally conceived as being unstructured interviews broadly covering the question of what areas the realist synthesis could examine which may be of most use to them in practice. In practice, they were more akin to conversations, addressing a far broader range of sub-topics around repeat victimisation than had been originally envisaged. One conversation (with Sylvia Chenery) took place entirely by email, the remainder were either face to face or by telephone.

There were similarities in the comments received; specifically all felt repeat victimisation was an important (and sometimes neglected) area. Comments from Alan Edmunds and Sylvia Chenery pointed to the difficulties of convincing people that more work still needed to be done with repeat victimisation. Confidence in the police was mentioned as an issue by all those consulted, featuring particularly within comments from Leicestershire Police. All were also interested in formalising the knowledge of why some projects fail (although there was an awareness of probable causes, this was generally from personal experience rather than evidenced externally). These areas formed the basis of the development of questions for the realist synthesis.

There were other areas which emerged which could not be examined in depth due to the lack of relevant evaluations available. Robert Street expressed an interest in the application of repeat victimisation prevention to victims of anti-social behaviour. Pete Bumpus and Rebecca Thompson from Leicestershire Police were particularly interested in preventing repeated hate crimes. There was a dearth of existing research in these two areas. Alan Edmunds works particularly with 'repeat streets' (small hot spots), which formed the basis of his interest, but was outside the scope of this research.

After consolidating the ideas that came from the consultation process, the questions to investigate were broadly established as:

- Are there identifiable differences between successful and unsuccessful interventions in the way programmes are implemented?
- Are there identifiable differences between successful and unsuccessful interventions in the techniques utilised by the different programmes?
- Does preventing repeat victimisation increase the level of confidence in the police?

This realist synthesis was therefore primarily driven by the strategy of reviewing the same theory in different settings to determine which circumstances were conducive to a successful intervention, with secondary considerations of implementation issues (which also helped to shape the primary focus), and confidence in the police.

In practice, the differences between successful and unsuccessful programmes (both in terms of the techniques they utilised and the implementation) could only be examined for domestic burglary programmes. The three other crime types examined here had only successful outcomes, according to the statistical calculations from the previous chapter. This determination of success or failure was retained to maintain consistency across the thesis. Any studies which had not been suitable for inclusion in the odd-ratio calculation were used in this synthesis for other data extraction purposes than a success/failure measure. The final, specific, hypotheses are listed in the relevant sections in Chapter Seven.

IDENTIFICATION AND ARTICULATION OF PROGRAMME THEORIES

“All interventions carry an implicit set of programme theories” (Pawson et al., 2004). These are ideas about how and why aspects of an intervention may work. These theories are often not explicitly stated in the evaluations, but are underlying assumptions which shape the intervention in a specific way. There were two stages to identifying such theories. Firstly, where appropriate, stakeholders were asked about their experiences with repeat victimisation programmes they had been involved with. Secondly, the breadth of repeat victimisation literature was examined. Much of this had been done in earlier phases of this research, and many relevant theories of crime reduction and repeat victimisation prevention can be found in Chapter Three. Further reading and extraction of relevant theories was carried out specific to the questions listed above. The experiences and assumptions which were identified were formed into theories for testing, below. Included are well-tested premises, for example the time course of repeat victimisation which is known to exist and relates to the speed at which repeated victimisation occurs (see Polvi et al., 1991; Morgan, 2003). The specificity of applying this concept to produce a speedy application of an intervention aimed at prior victims, to increase the chances of successful risk reduction, is one that requires further examination. Also included in these theories for testing are underlying assumptions, for example providing information which educates the victim about their risks. This in theory empowers them to take action to reduce those risks - see for example Yeater and O’Donohue (2002: 1136) who examined this issue in relation to victims of sexual assault and posited that “it is possible that women who are sexually revictimized may know how to identify risk and how to respond to high-risk situations but for some reason are unable to apply these skills in real-life situations”. Whether educating someone of their risks is a technique which works in practice to prevent repeat victimisation needs further inspection – the technique has certainly

been of questionable benefit in other crime prevention contexts (see for example Ennett et al. 1994 on the DARE drug reduction programme). The mechanisms and theories presented in Table 6.1 are those which can apply across crime types and contexts, and draw on the ideas from Tilley (2009b) and Pawson and Tilley (1997) of mechanisms which can act in both positive and negative ways within an intervention, and the different impacts these attributes can potentially have on the outcome. Identification of potentially adverse impacts which may result from the implementation of programmes is important so that they may be withdrawn before they cause harm (McCord, 2003).

Table 6.1: Mechanisms and theories for testing in the realist synthesis

Positive acting mechanisms	Negative acting mechanisms
<p>Time course It is known that a victim is likely to be targeted again within a short time of their original victimisation. This can be interrupted by increasing the risk and/or increasing the effort for the offender so that the victim is not such an easy target. Timeliness is therefore imperative.</p>	<p>Time course Conversely, where there are delays to the work, a re-victimisation may occur before any preventive measures are implemented, or reduction in confidence may occur (perceptions of how seriously the crime is taken by authorities). Delays may also point to problems with motivation (see below).</p>
<p>Education The provision of information to victims implies teaching someone about their risks, and risk avoidance. In theory, this encourages and empowers them to change their behaviour to avoid these risks.</p>	<p>Changing Behaviour Education based interventions may go unheeded, and have no impact on behaviour, thus no reduction of crime/victimisation levels.</p>
<p>Motivation Successful repeat victimisation interventions occur where the partnerships involved have a clear understanding of the potential, and clearly defined roles to achieve this.</p>	<p>Motivation Where partnerships are not clear on their roles, or do not have confidence in the potential of an intervention to succeed, this could: a. come across in the way they deal with victims, hence decreasing confidence or interfering with the preventive measures; b. reduce their effectiveness generally.</p>
<p>Disproportionate effects By reducing the number of repeat victims, there will be a disproportionate impact on crime incidence (Pareto Principle).</p>	<p>Unclear mechanisms If the change in repeat victimisation is contrary to the change in overall incidence, the mechanism by which the interventions work is not clear, and could therefore be more likely to be a coincidental change in crime rate than due to any project effects.</p>
<p>Increased confidence Identifying and working with victims to reduce their risk increases their confidence in the police.</p>	<p>Increased fear Drawing victims' attention to their heightened risks serves to increase their fear of crime.</p>
<p>Diffusion of benefits Increased crime prevention measures have a greater impact</p>	<p>Displacement Crime shifts due to the intervention area as perceptions of risk</p>

than the immediate target, due to changing offender perceptions of risk.	increase, but other targets (or time, place, modus operandi, or crime type) are found by the offender to replace the original.
Tailoring of intervention Making specific adjustments to take into account particular contextual factors serves to target the problems directly – victims need to be protected by whatever means are appropriate.	Lack of flexibility By following set plans without allowing for a degree of flexibility in the implementation, the risk is that the problem addressed is not the actual problem responsible for crime, resulting in an ineffective intervention.

These mechanisms, which were identified through both the consultation process and literature searches, illustrate ways in which repeat victimisation interventions may be successful, but also ways in which they may be unsuccessful. A single aspect of an intervention has the potential to have either positive or negative outcomes. So, for example, educating sexual victims about their risky behaviour could:

- a) Encourage them to desist their risky behaviour (positive outcome)
- b) Make them aware of the facts without changing their behaviour (neutral outcome), or
- c) Give them false confidence in risky situations, thus actually increasing their risks (negative outcome)

There are also possibilities for other outcomes, not directly linked with the original aims of an intervention. To keep with the example of education for victims of sexual abuse, an alternate or additional outcome may be:

- d) Education allows the victim to reduce their self-blame.

Although a positive outcome, as this is not directly linked to the aims of the research, an outcome like this may be missed where it is unexpected and therefore not measured.

The theories identified above are not an exhaustive list of possible mechanisms. However, they do form the focal points for the realist synthesis as a whole.

SEARCHING FOR RELEVANT EVIDENCE

LITERATURE SEARCHES

The systematic review (see Chapter Five) identified the available literature on programmes designed to prevent repeat victimisation. Many of these evaluations were rejected for the purposes of the systematic review either because they did not have pre- and post-measurements of crime, or because there was not a comparison group. However, there was inevitably a wealth of other information available in these rejected studies. For example, information about implementation difficulties may have been particularly apparent in those studies without any conclusive outcomes. Although studies in this category were unsuitable for inclusion in a systematic review, this other information could be invaluable for practitioners, and thus was important to include in this realist phase of review. This synthesis therefore returned to the rejected studies to examine them for useful information.

Identification of additional papers through snowballing and grey literature searches were carried out for the purposes of the systematic review, so were not repeated here.

The realist approach follows the principle of theoretical saturation (see for example Glaser, 1992). The need to search for extra studies only continues as long as they are adding new evidence to the synthesis. New searches for the realist synthesis were unlikely to identify directly relevant evaluations due to the comprehensive searches carried out for the purposes of the systematic review. Therefore, the thorough searches conducted for the systematic review were decided to be an adequate starting point for analysis. Additional searches were only carried out where hypotheses identified a specific need, or where insufficient evidence existed within the originally identified literature. For example, further theory searches were required for the links between repeat victimisation and confidence in the police, as this was not identified as a possible area until the consultation process - which took place after the completion of the systematic review.

APPRAISING THE QUALITY OF THE EVIDENCE

Unlike a systematic review, the quality appraisal process in a realist synthesis does not follow any hierarchy of evidence. Instead 'fitness for purpose' is assessed under terms of relevance and rigour (Pawson et al., 2004). Segments of studies may be utilised where only part of an evaluation is deemed relevant to the theories tested. Rather than excluding studies at the start of the synthesis, it is an iterative process, informed by knowledge developed throughout the review.

In practice, useful information was gleaned from across the full range of identified evaluations. Although, for example, data on outcomes may not have been suitable for inclusion in this realist synthesis due to doubts about reliability, that same study may have provided useful information about implementation problems, or confidence in the police. In this way a body of evidence was established, within which commonalities and consistency suggested reliability.

EXTRACTING THE DATA

Following the realist approach, the data extraction utilised a bespoke approach to each study. This involved both annotation of studies, and separate note-taking. The notes system developed for this extraction consisted of a series of free text forms with sections allocated for key areas of interest. The form was designed to be one side of A4, in order to maximise ease of reference at the synthesis stage, and were initially handwritten, before being transferred to a word-processed format at a later stage. The sections were deliberately broad areas, and not all questions applied to all studies: common sense was applied to skip irrelevant questions. An example of the completed comments form is shown in Appendix C.

SYNTHESISING THE EVIDENCE

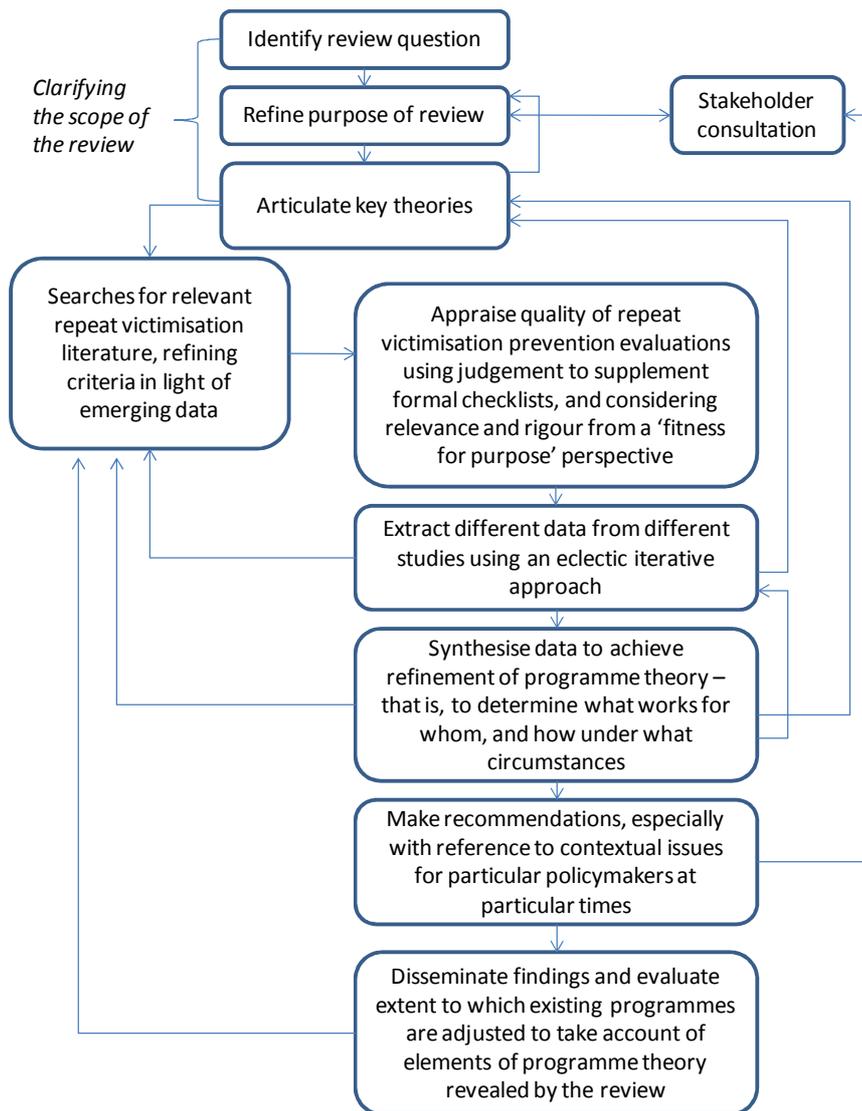
Once the annotation and comments forms were completed, a number of hypotheses were developed by crime type, based on the mechanisms and theories identified (above). This was an iterative process, making use of emerging evidence, with searches for supporting and conflicting evidence conducted as the synthesis developed. This explanatory process sought

to establish what worked, for whom, and in what circumstances. It aimed to determine common weak points in the implementation, delivery, and underlying assumptions of the interventions. For example, Chapter Seven demonstrates that the selection of the staff involved in delivering repeat domestic burglary interventions is related to the success of that intervention. The availability of evidence varied considerably across crime types, and thus different questions were asked of the information available accordingly. The aim throughout was to track theories across the literature, and therefore the synthesis was conducted by drawing out evidence from different sources to support or oppose these theories. Hence, Chapter Seven is laid out with the emphasis on the theories discussed, rather than on the individual study.

DRAWING CONCLUSIONS, FRAMING RECOMMENDATIONS AND DISSEMINATING FINDINGS

Rather than a final conclusion (as was seen in the systematic review of Chapter Five) of what works, what's promising and what doesn't work, the realist synthesis makes a contribution to the policy discourse which is oriented more toward revisions of *how* an intervention works. Caution must therefore be taken when framing recommendations, as the progress made is from 'some knowledge' to 'some more knowledge' (Pawson et al., 2004) rather than providing a definitive solution. This links back to the principle of evolutionary epistemology, where unsatisfactory explanations are discarded, and new problems emerge, incrementally expanding and improving the relevant theories along the way. Recommendations from this realist synthesis can be seen in Chapter Nine, along with those from the systematic review, and more generally about the meta-evaluative techniques utilised to advance knowledge about crime prevention initiatives. The dissemination of the findings, both to stakeholders and academia, aims to influence on the ground decisions by improving the knowledge available for the decision making process.

Figure 6.1: The realist synthesis process. Adapted from Pawson et al. (2004)



Chapter Seven: Realist Synthesis of Repeat Victimisation Prevention Evaluations

Truth in science can be defined as the working hypothesis best suited to open the way to the next better one.

- Konrad Lorenz

INTRODUCTION

The evidence base for inclusion in this chapter was broader than that of the systematic review. The context-mechanisms-outcomes configuration was examined regardless of whether an evaluation met the inclusion criteria for the systematic review, as valuable information was found outside these limited studies. To determine the links between whether a programme had been successful and which characteristics were associated with success or failure, the synthesis included those studies which were included in the systematic review. For pragmatic purposes, success here was defined as for the systematic review, typically a reduction in incidence as shown by the effect size calculations. The synthesis was supported by studies which were excluded from the systematic review (see Appendix A for details) and theories were tracked through associated literature.

The synthesis examines the prevention of repeats within domestic burglary, commercial crime, sexual victimisation and domestic violence, with a focus on contextual, implementational, and theoretical issues as appropriate.

DOMESTIC BURGLARY

Following the much lauded success of the Kirkholt project in 1988-90, there have been a multitude of putative replications. There have been questions raised about whether Kirkholt was a success because of the crime prevention initiative or because of the other work occurring on the site at the time (Osborn, 1996). However, this has been refuted and success linked definitively to the crime prevention initiatives implemented (Farrington, unpublished). This being accepted, there seems to be a case that following the methodology employed at Kirkholt would result in successful crime reduction elsewhere. However, Kirkholt remains the most successful domestic burglary revictimisation prevention programme to date (see Chapter Five). This suggests that there must be differences between this programme and the replications which followed. This could be linked to a number of difficulties or differences in subsequent attempts to replicate Kirkholt's success. Here, seven hypotheses are examined. The first five focus on differences between the successful and unsuccessful repeat domestic burglary prevention programmes (refer to Chart 5.1, Chapter Five for the levels of programme success). The final two hypotheses examine secondary outcomes from the programmes.

Hypotheses:

1. The selection of frontline staff involved in the project had an impact on the final success of the programme
2. The size of the experimental area and the clarity of its boundaries had an impact on success

3. Specific identification of the problem, and targeting solutions appropriately to the situation increased success
4. Common implementation problems contributed to failure
5. Initial crime rate impacted on success
6. Where displacement occurred, this could have been predicted and preventive measures put in place
7. Where a programme was successful, confidence in the police increased

HYPOTHESIS ONE: THE SELECTION OF FRONTLINE STAFF INVOLVED IN THE PROJECT HAD AN IMPACT ON THE FINAL SUCCESS OF THE PROGRAMME

Two of the most common problems with staffing resulted from the use of volunteers or the police, rather than people specifically employed for the purpose of implementing the intervention, or aspects of the intervention. Where police were used, there were often problems with convincing them of the effectiveness or priority of the intervention in question. Training was difficult when using serving police officers, as the nature of police work often meant that finding a suitable time to run a training class for all officers involved was not possible. This was an issue that the Beenleigh intervention was unable to address effectively, to the extent that staff arriving after the start of the project did not receive any form of training. Beenleigh, as the least successful of the interventions (where outcomes could be assessed) also had problems explaining the need for the project to the police staff involved: “The underlying philosophy of the project was not well understood by Beenleigh Police” (Budz et al., 2002). Other unsuccessful programmes also had problems with the police. In Ashfield, other projects took priority, and a police restructure in Norwood/Tea Tree Gully created a lack of consistency for the programme.

Projects commonly found difficulties with getting volunteers to have the same level of commitment and availability as would be expected from paid staff. Norwood / Tea Tree Gully had volunteers who did not always give notice of availability, and who did not fill out survey sheets accurately or even legibly. This could be a contributory factor to the failure of the programme in this area.

In contrast, the most successful programmes tended to have very committed staff. Kirkholt spent a long time selecting the staff which would be right for the project – even though this meant the compromise of starting the project with fewer staff than intended. The successful projects in Blackburn, The Meadows and Merthyr Tydfil all had either a carpenter or lockfitter who went beyond the expected duties in order to support victims, which in turn often boosted the profile of the project in each area. For example, the Homesafe report author commented:

The lockfitter was crucial to the success of Homesafe, not only in that he or she had to do a sound job in order that the security hardware was effective, but that they also had to be able to reassure the householder and give that extra bit of care over and above what might be normally be expected. They also had the opportunity to pass on crime prevention advice.

Webb, 1996

This is not without exceptions: Bentley and Hartlepool were both saw falls in crime incidence, but experienced volunteer dropouts; delays were caused in Baltimore and San Diego because of staff problems. However, it is notable that these four programmes still showed lower levels of success than those without such staffing problems. It is possible that without these issues, the repeat victimisation prevention programmes would have been even more effective at reducing crime.

HYPOTHESIS TWO: THE SIZE OF THE EXPERIMENTAL AREA AND THE CLARITY OF ITS BOUNDARIES HAVE AN IMPACT ON SUCCESS

There was an underlying assumption within some of the repeat victimisation prevention interventions that the size of the target area would affect the success of the programme. Webb (1996) chose areas for the Homesafe programme taking heed of advice from Crime Concern, which suggested 3000 households as an ideal size for the intervention. Weisel (1999) suggested that the areas of Baltimore, Dallas and San Diego selected for the hot dots programme were larger than would normally be used with problem solving approaches; and in a different paper (Weisel, 2002) emphasised that “different burglary patterns appear even within quite small areas”.

There was an underlying assumption in some of the evaluations that areas with clearly defined boundaries were more suitable for a crime prevention intervention. This was explicit in the Kirkholt project, which stated of the housing estate: “It has the desirable characteristic (for our purposes) of being an area with well-defined boundaries” (Forrester et al., 1988). Underpinning this assumption is the Brantinghams’ theory of awareness spaces (Brantingham and Brantingham 1993 [2004]) which links in to the idea that offenders will be limited in the area they frequent. It is intuitive to assume that physical boundaries such as a main road or train line would act as a limit to where routine activities would naturally take an offender, and thus where nodes of activity and associated opportunities for crime overlap. In the studies where outcome was determined in the systematic review, there was no significant relationship between success and the treatment area having clear boundaries (two-tailed Fisher exact $p=0.443$). However, information on the clarity of boundaries was only provided for half of the included interventions. There is therefore insufficient evidence to suggest a link, or conversely to confirm that there is definitely no link.

Harries (1981 [1991]) suggested that the best denominator for rates of residential burglary is number of residences. The size of the area in the evaluations examined in this thesis was commonly measured in number of households, but several measured the area, and crime rate, in terms of population instead. This presented a difficulty when comparing areas with different terms of measurement. In order to estimate the size of an area in households for the five interventions where population was used, census data for the relevant country was used to determine the average number of people per household. In both Australia and America, this was 2.6, and so the population figure was divided accordingly to provide an estimate of the number of households in an intervention area. Although not definitively accurate, this figure was sufficient to allow a *t*-test to be performed to assess whether there was a difference between successful and unsuccessful interventions in terms of how many households were in an area. A further caveat must be noted, that not all interventions provided an indication of the number of households or the population within the treatment area.

The mean number of households in a successful intervention area ($M=9738$, $SD=20150$) was a third of that in an unsuccessful intervention area ($M=29921$, $SD=29138$). However, the large standard deviations do not result in this being a statistically significant difference ($t=-1.58$, $DF=13$, two-tailed $p=0.138$). Hence it cannot be unequivocally concluded from this data that the size of the area has a significant impact on whether an intervention is successful.

HYPOTHESIS THREE: SPECIFIC IDENTIFICATION OF THE PROBLEM AND TARGETING SOLUTIONS APPROPRIATELY TO THE SITUATION INCREASES INTERVENTION SUCCESS

Consultation with community groups to give them a sense of ownership of the project was a technique used and to gain local knowledge. For example, in Merthyr Tydfil extensive consultation with local people and agencies was carried out to identify the specific area in which the intervention should be implemented. This local involvement was not universal, as for example the Blackburn programme, whilst successful in reducing crime incidence, received data on the crime rates only at evaluation stage – which suggests that the resources could not have been targeted to the local problems in advance. However, as with all reports on social interventions, there are inevitably limits to the detail and the decision making processes which can be explained, sometimes leaving the reader to make assumptions and infer details (see Tilley, 2009). It is entirely possible that some interventions had a greater preparatory stage than the reports describe.

Five of the thirteen unsuccessful repeat domestic burglary programmes did not provide a locally-based explanation for the choice of the package of measures chosen to prevent repeat victimization. For example, on the New Parks estate, although target hardening was offered, most households already owned the security available. Beenleigh used generic measures taken from Kirkholt and Biting Back, but without a discussion of whether they were applicable to the problems in the Beenleigh area. Where local information was described, it was not always used effectively - for example in the unsuccessful Ashfield project, where most burglaries occurred along transport routes, but a more intense application of prevention measures was not available to victims in these areas. This clustering of burglaries along transport routes as seen in Ashfield is predictable, and follows crime pattern theory (see for example Brantingham and Brantingham, 2008) where pathways between nodes of common routine daily activities create spatial-temporal patterns of crime.

There are therefore some indications that those interventions which were flexible enough to take into account local needs were more successful than those which applied a set package of measures regardless of specific local needs. Thirty nine percent of the unsuccessful programmes were not tailored to the specific area. Although a quarter of the successful programmes also lacked specific tailoring, two thirds of these were in the programmes with low effect sizes. Kirkholt is perhaps the best example of successful tailoring to the problem. Coin payment meters for fuel were common on the estate, and the temporal peak of burglaries linked to the time that these meters would have the most money in them. This particularly local problem was identified and then addressed by the Kirkholt project by removing these coin payment meters.

HYPOTHESIS FOUR: COMMON IMPLEMENTATION PROBLEMS CAUSE FAILURE

The common implementation problems fell broadly into six categories: eligibility; communications; inflexibility; resistance to measures; staffing; and data. These were identified through extensive examination of the evaluation reports, and listing of implementation problems. The implementation difficulties faced by multiple interventions were then sorted into groups of shared issues, before naming them as these categories. Staffing presented a particularly large problem to some interventions, and has been discussed in depth as hypothesis one. The other problems are discussed below:

Eligibility: Despite the interventions selected for inclusion here having a focus on repeat victims, in eight cases a lack of clarity around the eligibility criteria took the focus away from victims, and funding was diverted to ‘vulnerable’ groups. The definition of vulnerable groups varied across these eight projects assessed, and did not have tangible links to the actual risk of victimisation in a community (for example Greenwich, Liverpool). Instead, they were chosen due to perceptions of risk, or because they were felt to be particularly fearful of crime (for example in Merthyr Tydfil). This approach risks diluting the effect of focusing attention on those known to be at greater risk of being victims – those who have been previously victimised. This is not to suggest that groups which are known to be at a particularly higher risk in a certain area should not receive help should funding allow, but rather that there are explicit, relevant reasons for targeting some groups over others. There is however a case to be made for providing intervention to victims of other crimes, particularly criminal damage or attempted burglary, as it has been suggested that prior attempts may be predictors of a potential future crime (Farrell and Pease, 1993). This tactic was used in Blackburn, where those who had keys stolen or an attempted burglary were eligible for the intervention. Blackburn was second only to Kirkholt in terms of successful reduction of burglary incidence, and also had a reduction of two-thirds in repeat victimisation.

Communications: Many of the projects involved working with multiple agencies, and all of them involved working with the public. This creates a number of places where communications can break down. Perhaps the most damaging to the overall aims of crime reduction were any that impacted on referrals of victims or of following up such referrals. For example, the Norwood/Tea Tree Gully Project had problems both with victims not being referred, and then with arranging visits to victims. The New Zealand Target Hardening Programme experienced similar difficulties. In New Parks, there was poor communication of the lock fitting scheme to potential recipients, and many eligible households were not aware of the scheme, a problem shared by the linked project run in Eyres Monsell. The public did not have the same perception of volunteers as of the police, and so volunteers sometimes faced difficulties convincing households that the scheme was important, and free. Even successful programmes came across communication problems, although these were more commonly internal. For example, San Diego planned for daily burglary reports to be given to staff – this did not always happen, often due to absences. They attempted to circumvent this problem and change the tactics used to make it easier for staff to comply, but still had problems which created delays in the delivery of the intervention.

Inflexibility: Often a contributory factor to the other implementation problems listed, difficulties ensued where the intervention was not flexible enough to take account of changing demands. In Merthyr Tydfil, the team could not distinguish between genuine attempted burglaries, and those reported to the team in order to acquire the target hardening.

Fortunately, in this case the intervention was flexible enough to allow the target hardening to be provided regardless, thus ensuring that no-one was excluded unnecessarily, and allowing the programme to reach a successful conclusion. However, other programmes did not always share this flexibility. In the New Parks project, for example, many in the target area already had the security which was due to be delivered. Rather than adapting the intervention or target area, the project continued to follow the plan without consideration of the mechanism by which this would then work. This programme was not successful at reducing crime in the area, although repeat victimisations were halved (this seeming contradiction itself highlighting the difficulty of how success should be measured).

Resistance to measures: In both of the cases examined in this thesis where a considerable component of the intervention package included the alley-gating of burglary hotspots, the teams faced public and legal opposition. This was related to practical considerations, for example in Hartlepool where if a permanent structure had been erected, responsibility for the maintenance of the alley would have been transferred to residents. Both councils and residents also expressed concerns about refuse collection, and the number of people who would have keys to allow access to the alleys. Bowers et al. (2003: 4) stated of the Liverpool project that “the legal process of applying for closure orders for each of the alleyways impeded the progress of this scheme”. Indeed, in that study, only ten of the planned 69 gates were installed by the end of the evaluation period. Sturgeon-Adams (2005: 3) suggested that:

successful use [of alley gating] depends on a positive attitude to the use of the space, for example as a play area as well as improved security.

Where an intervention was to be put into an apartment block or similar, there was sometimes resistance from the apartment manager. For example, in Dallas, apartment managers cited that they thought that some of the measures, such as hanging warnings on the doors of neighbours of victims, would increase fear of crime. This created difficulties for the implementation in some areas.

Similarly, landlords of rental properties were not always co-operative with interventions. In the New Zealand Target Hardening Programme, one of the housing companies approached refused to allow the installation of deadlocks on their properties due to the perception of an increased fire risk. In all, nearly half of the repeat domestic burglary interventions examined in this realist synthesis experienced some level of opposition to measures from a group of stakeholders.

Data: There were a number of data related problems experienced by a number of the programmes at various stages. At the initial stage, there was difficulty with identifying repeats. This was often due to insufficient data entry or data management systems at the reporting stage of the crime, where for example the system could not differentiate between crimes in different apartments within a single block, or where addresses were manually entered instead of having an auto-complete address bar. This can result in one address being recognised as separate addresses, and hence not flagged as a repeat. For example:

1A Smith Street, Bridgnorth

1.a. Smith Street, Bridgenorth

1 Smythe Street, Bridgnorth

etc.

Some of the interventions attempted to introduce GIS crime mapping to identify repeats on an ongoing basis. Although far more widely used now, at the time of many of these interventions this was a new technology, not commonly utilised by the police. Opposition to this was common due to a lack of knowledge, time, training, or funding. This resulted in a potential data source being underused and often completely neglected.

Even at the evaluation stage, there were problems with data. Many of the evaluations which faced problems at this stage were excluded from the systematic review because insufficient data was available to enable comparisons to be drawn and success to be appraised.

HYPOTHESIS FIVE: INITIAL CRIME RATE IMPACTED ON SUCCESS

A number of the interventions were designed to be put in place in areas with specific crime rates. For example a number of the Homesafe schemes were specifically established in places with a “medium crime rate” (Webb, 1996). One of the recommendations was that

An area with a crime rate higher than 10-12% need not necessarily be avoided as being too difficult. It is however necessary to look at the context and see how that might affect implementation. Areas with crime rates lower than 6% should be avoided for intensive action as it is much less likely to be cost effective.

Webb, 1996

However, it is unclear why a crime incidence rate of 10 to 12 per 100 households might be deemed ‘too difficult’. A high crime area might, alternatively, be the more appropriate location for such a project, particularly since repeat victimization was found to occur disproportionately in high crime areas (Trickett et al., 1992).

The studies included in this meta-evaluation had burglary incidence rates ranging from 4.8 to 24.6 per 100 households. Six interventions did not report the burglary rate per household but, rather, they reported them by population or did not quantify the rate. Population appears to have been a denominator of convenience as it is not the most suitable denominator for a measurement of burglary, as burglaries occur at a place rather than specifically to a person.

The mean crime rate before the intervention was implemented for successful ($M=10.07$ $SD=6.20$) and unsuccessful ($M=6.17$ $SD=2.20$) interventions did not differ significantly ($t=1.046$ $DF=12$ two tailed $p=0.316$). Therefore, while there is some suggestion that targeting higher crime rate areas is preferable, there is no unequivocal evidence from this data to support the hypothesis that the initial crime rate impacted on success. However, caution must be exerted due to the relatively few studies which provided this data, and the greater number of missing data in the unsuccessful spectrum of interventions. It cannot be conclusively said that the

initial crime rate did not impact on success, merely that there is currently no evidence to support this.

HYPOTHESIS SIX: WHERE DISPLACEMENT OCCURRED, THIS COULD HAVE BEEN PREDICTED AND PREVENTIVE MEASURES PUT IN PLACE

Where displacement was identified, this was never total. In other words, a small amount of the crime prevented displaced to another time, place, or method, but not all of it. In all cases where there was definitive displacement identified in the evaluation report (five interventions) the displacement was predictable, and therefore arguably it could have been prevented with sufficient forethought and resources. For example, in The Meadows, target hardening was to doors and to window locks. Here, there was some displacement to smashing a window. Had double glazing been installed at the same time as the other measures, this tactical displacement would have been made more difficult. This is not to criticise this intervention on this count, as there are always constraints on what is possible in any programme, but merely to illustrate that with sufficient resources, it would be possible to design (at least some) displacement out of interventions.

Occasionally, other crime types increased in programme areas, but there was no suggestion that this increase was directly related to the crime prevention measure in the form of crime-type displacement. For example, in Kirkholt, criminal damage increased. The authors suggested that as criminal damage is not an acquisitive crime, it made for an unlikely shift from burglary. It was suggested that pride in the estate had increased as a result of the crime prevention programme, and it was therefore reporting of criminal damage which had increased, rather than criminal damage per se.

Liverpool and Bentley interventions also showed evidence of a diffusion of benefits. Liverpool was particularly unusual in that evidence of both geographic displacement and diffusion of benefits were noted in the buffer zones around the treatment area. In addition, there was “evidence of a diffusion of benefit to untreated households” in the treatment area (Bowers, 2003, vii).

HYPOTHESIS SEVEN: WHERE A PROGRAMME WAS SUCCESSFUL, CONFIDENCE IN THE POLICE INCREASED

Nine evaluations commented on whether there had been an impact on police-public relations during or after the intervention was in place, but only three of these gave any detail, the remainder made a comment about the impressions that the police had about the effect on their relations with the public. “The Government has in 2009/10 highlighted the importance of local Policing by setting Police Forces a single performance measure - confidence in Police and Local Councils to deal with Crime and Anti-Social Behavior that matter to local communities.” (Leicestershire Police Authority, 2009). This recent emphasis on satisfaction with and confidence in the police post-dates the studies examined in this synthesis, and therefore it is perhaps more likely that this measure will be examined in future evaluations. Additionally, in projects where the police were not directly involved in the intervention, the measurement was irrelevant to those conducting the intervention or evaluation and therefore the variable was not measured.

There were however a small number of studies which did discuss the impact of the intervention on the level of confidence in the police. In these, there were often more positive perceptions in these once an intervention had been put into place, for example Beenleigh, Huddersfield, Ashfield, Bentley and Morley (although there were often no control groups for this measure to allow comparisons to be drawn). However, there did not seem to be a link between whether an intervention was successful and the increased confidence in the police.

Table 7.1: Implementation issues experienced within domestic burglary interventions:

Programme (see Appendix A for details of studies not in main text)	Success? According to effect size	Staff problems?	Lack of tailoring ?	Unclear eligibility criteria?	Comms break down?	Inflexibility?	Resistance to measures?	Data problems?
Kirkholt	Y							
Blackburn	Y		✓	✓				
Meadows	Y			✓			✓	✓
Liverpool	Y			✓			✓	
Burnley	Y				✓			
Merthyr Tydfil	Y			✓				
Bentley	Y	✓					✓	
Baltimore	Y	✓			✓	✓		✓
Hartlepool	Y	✓			✓		✓	✓
San Diego	Y	✓	✓		✓	✓	✓	✓
St Anns	Y	✓						
Eyres Monsell	Y		✓		✓	✓	✓	
Ashfield	N	✓	✓				✓	✓
Morley	N	✓					✓	
Norwood/TTG	N	✓	✓		✓			
Dallas	N					✓	✓	✓
Cambridge	N						✓	
New Parks	N	✓	✓		✓	✓		
Beenleigh	N	✓	✓				✓	✓
Orange	?					✓		
Lambeth	?	✓		✓	✓			✓
THP	?	✓	✓	✓	✓	✓	✓	✓
Huddersfield	?							✓
Manchester	?	✓		✓				✓
Greenwich	?			✓				✓

The remaining three crime types examined within this thesis are commercial burglary, domestic violence, and sexual victimisation. In comparison to the breadth and depth of information available for domestic burglary, there was a relative scarcity of relevant repeat victimisation prevention evaluations and related publications. This is reflected in the different questions and hypotheses put forward in the following sections: it was not possible to address the same issues across all four crime types.

COMMERCIAL BURGLARY

Unlike domestic burglary, no unsuccessful interventions were identified within commercial crime repeat victimisation prevention programmes. However, only three such programmes were assessed in the systematic review for outcome measures, so the lack of diversity is not completely unexpected. There is a dearth of research in preventing repeat commercial crime. This is somewhat surprising, as businesses are known to be at greater risk of chronic victimisation than residential properties (Bowers et al., 1998). Further, crime prevention advice has often been given specifically to businesses – one such example stems from 1956, where Leicestershire and Rutland Constabulary distributed booklets giving crime prevention advice to businesses as shown in Table 7.2. However, there is no record as to the effectiveness of such advice. This limits the conclusions which can be drawn about particular features which are known to enhance the chances of success in preventing commercial repeat victimisation.

Table 7.2: Crime Prevention Advice for Businesses (Taylor, 1956)

DO:

Take regular precautions to protect your property against theft

See that the locks, bolts and catches on doors and windows are efficient

Have any repairs attended to at once

Make someone responsible for seeing that your business premises are securely locked up at night

Notify the police of any variation in working hours or of change in key-holder or his address

Arrange for someone to visit your business premises at week-ends

Instruct persons holding keys to your premises not to loan them to anyone

Make a written note of the number of your motor vehicle or bicycle, or your typewriter or camera

Inform the Police at once of anything suspicious seen or heard, and if a motor vehicle is concerned, make a written note of the registration number, and if possible, make, type and colour

Lock the doors and boot of your car when leaving it unattended

Contact the Police on any matter regarding crime prevention you consider necessary. If you are not happy about the security of your property or premises, discuss the matter with your local police

DON'T:

Leave your business premises unattended during the lunch hour

Keep large sums of money in lock-up premises or your home

Leave the key to the safe on the premises – carry it with you

Leave personal belongings easily accessible to callers

Have ladders or steps left about for the burglar to use

Forget when going away to notify the police, to cancel tradesmen's delivery and your daily papers, and avoid all outward signs which might indicate your absence

Tell anybody but your relations, friends or neighbours that you are going away

Take callers for granted – ask to see their credentials

Tidy up if you have the burglars until the police have been

Leave valuables in your car when parked so that anyone passing by can see them – cover them with a rug, if possible

Listen to the wireless or watch television after dark unless doors and windows are securely fastened

Frighten away anyone you see trying to break into a house or premises – even your own. If possible, get someone to notify the police, by telephone or otherwise, whilst you keep observation on the person until the arrival of the police

A number of broad areas for discussion have been established:

1. Specific issues faced by researchers and practitioners when attempting to prevent repeated commercial burglary.
2. Implementation issues when tackling repeated commercial burglary.
3. The mechanisms by which preventing repeated commercial burglary may work.
4. Displacement and diffusion of benefits.

Here, as in the systematic review, the focus is on commercial burglary. Although there is information given in a number of reports about other crime types (for example fraud) suffered by commercial premises, the only one commonly detailed in any depth is that of burglary. Tilley and Hopkins (1998) suggest that commercial burglary has a high rate of reporting to the police (80% or more). However, Mawby and Jones (2004) examined hotel burglaries, many of which were recorded as dwelling burglary or other burglary, depending on the exact nature of the crime, making data extraction more complex than relying on a single category. Bowers (2001) confirms the high reporting rate of commercial burglary, and compares it to reporting rates for other commercial crime types (below) which are not reported so frequently. Although there is some variation in the pre-intervention and post-intervention findings, the percentages are reasonably static. This suggests that when using police recorded data, it is likely to be more reliable for commercial burglary than other types of commercial crime. As the data is likely to be reliable, and burglary is detailed in all three evaluations, other crime types are disregarded here. However, caution must therefore be stressed as interventions are not necessarily universally successful for other commercial crime types as they are for burglary, and the discussion here can only confidently comment on the issues in relation to burglary.

Table 7.3: Commercial crimes reported to the police as a percentage of crimes experienced by businesses. Source: Bowers (2001: 37).

Crime Type	% of businesses reporting crime to police in baseline survey	% of businesses reporting crime to police in evaluation survey
Burglary	89.3	83.3
Attempted burglary	53.3	58.2
Criminal damage	44.3	37.0
Shoplifting	23.5	26.9
Employee theft	15.4	50.0
Fraud and forgery	23.2	13.1
Robbery	83.3	60.0
Assault	44.6	42.2
Theft from customer	42.9	52.4

SPECIFIC ISSUES FACED BY RESEARCHERS AND PRACTITIONERS WHEN ATTEMPTING TO PREVENT REPEATED COMMERCIAL BURGLARY.

There are specific difficulties which apply to commercial enterprises, and make it harder to work to reduce burglary with them than with residential properties. There is evidence to suggest that a certain level of crime is accepted by many businesses as part of their built-in losses. Challenger (1997: 40) states that:

Getting business to acknowledge a crime problem exists at all is the first hurdle for a crime prevention practitioner to clear. Then it is necessary to get agreement that it makes good business sense to do something about it.

This view was evident in the programmes evaluated here. In Belgrave and the West End of Leicester “problems appeared to be treated as a normal aspect of business life” (Tilley and Hopkins, 1998: vii). In Merseyside, it was felt that there was an element of “learnt helplessness” amongst businesses and that crime was a cost to be endured. Mawby and Jones (2004) found that hoteliers did not see burglary as a problem, despite high levels of victimisation, and were instead more concerned with issues including traffic, litter, vandalism, alcohol abuse and public disorder.

There is inherent difficulty with identifying and making initial contact with businesses. Not all businesses are listed in a single, publicly accessible, source – “there is no definitive annual census of the retail business community” (Hollinger, 1997) let alone other business types. For example, Companies House lists the head offices of all limited companies in the UK, but this does not cover all businesses, nor does it cover local branches of these businesses. Not all commercial properties are listed in the Yellow Pages (or its equivalent outside the UK) or other advertisement listings. In the Leicester programme, the researchers identified businesses manually, by walking every street in the target areas (Tilley and Hopkins, 1998). Bowers (2001) identified businesses using GIS, and then approached a stratified sample of these. In the Multnomah intervention, businesses were those that either had reported at least

one burglary in the one or two months prior to the survey, or were identified “from a complete listing of Multnomah County's Personal Assessment Tax Roll” (Pearson, 1980: 6).

Once the businesses have been contacted, and managers or owners persuaded that crime is a problem that they should consider tackling, the interventions themselves may still be rejected. The compliance rate with suggested measures was poor across the evaluations included here. Less than a quarter of the businesses targeted in the Multnomah programme had a compliance rate of greater than 50%, where compliance was examined by how many of the suggested measures were implemented by the business. Cost may be an issue, as all of the Multnomah suggested interventions of a high cost (alarms, windows and glazing, and safe improvement) had low compliance with less than a third of businesses installing such recommended measures. In contrast, the same project saw a high compliance rate of 60.4% when improvements to the way in which money was handled were suggested. The Merseyside project also experienced problems with the cost to the businesses. In this case, small grants of up to 50% of the costs of installing suggested measures were available to eligible businesses (with a maximum £1500 available per business), but this still left a prohibitive amount to be funded by the business. Of the 46 businesses offered this financial assistance, only 17 accepted. In the Leicester project, the aim was to find solutions which did not depend on external funding, so only a small amount of funding was made available to businesses. The whole project was funded at £150,000 per year for three years (Taylor, 1999). Two thirds (28) of chronically victimised businesses included in this project were receptive to the improvements suggested. The remaining fourteen chronically victimised businesses did not make any improvements, which was not obviously due to cost considerations. Tilley and Hopkins (1998: 5) explain:

According to the lead officer dealing with this part of the project, there were various reasons including: lack of interest; the fact that the Crime Prevention Officer had already attended and given advice; discovery that the problem now appeared to be less serious than was evident at the time of the survey; and, that the business had already attempted to deal with the problem.

There is a need to quantify the losses which occur through crimes to the businesses in order to encourage them to take preventive measures as “business will only consider a crime prevention initiative if its cost benefit ratio can be clearly demonstrated” (Challinger, 1997: 42; see also Burrows, 1991).

IMPLEMENTATION ISSUES

In the domestic burglary projects it was apparent that the staff and frontline team that worked to reduce burglary were important to the success of the project. Within commercial burglary projects, the evidence is limited by the few studies that were available. In the Multnomah project there were five team policing areas. In team one's area the non-compliance rate, that is, the businesses which applied none of the advice given, was 85.1%, compared with between 20.8% and 36.4% in the other four team areas. This suggests a distinct difference in this single team policing area. Further information is not supplied in the report, but it is possible that staffing played a part, or that there were other differences in this team area, such as over-representation of a type of business which was inherently less likely to comply with security suggestions. The Merseyside project evaluation did not make specific comments

about staffing implementation, but the intervention was delivered by professional Crime Prevention Officers. The Leicester project relied on a small team and Crime Prevention Officers to deliver the initiative, again no specific implementation comments were made.

Other projects ongoing in either a target or control area inevitably can have an impact on the intervention, either because of directly impacting the programme or by changing the apparent success or failure. For example, a police crackdown in the target area could make a programme appear more successful than it was, but the same police crackdown in the comparison area could make it appear as though the intervention had no impact. In the three preceding years to the Multnomah project, a “Community Crime Prevention and Education” programme had been implemented (Pearson, 1980: 1). During this earlier project, the focus had not been on commercial premises, but rather the wider community. In this time, residential burglary had fallen by 24%, but commercial burglary rose by 43%. It is possible therefore that the impact of the Multnomah project was overstated due to regression to the mean. Within both the target and control group, a large number of businesses were chosen because of an initial victimisation. The consequent “natural statistical regression was accounted for and treated” by removing this selection crime (Pearson, 1980: xii). The Leicester project suggested that regression to the mean may be responsible for some of the fall in crimes seen for chronic victims between the first survey sweep and the second, but that due to the size of the swing it was unlikely to account for the whole fall (Tilley and Hopkins, 1998). In addition, the benefits were seen beyond the specific businesses targeted which decreases the likelihood that regression to the mean accounts for the fall in crime. There was no mention of whether any other projects were ongoing in the target or comparison areas through the duration of the programme. The Merseyside project noted that “a substantial proportion of the properties surveyed had target-hardened their properties within the intervention period, although they were not involved in the SBS intervention” (Bowers, 2001: 29). During analysis, where the effectiveness of the scheme as a whole was being measured, those properties which had target hardened outside the scope of the intervention were disregarded. However, where the individual measures (i.e. target hardening) were being assessed, all target hardened properties were included in the analysis, regardless of how this target hardening was funded.

MECHANISMS

The mechanisms of commercial burglary prevention measures here depend predominantly on businesses making changes to their routine activities and physical security measures. These mechanisms were encouraged by increasing awareness of victimisation and risky behaviours and giving advice on appropriate counter actions. The changes that ensued would act by increasing the effort, increasing the risks, and reducing the rewards of committing a burglary, and thus decrease opportunities and perceived opportunities. In the Leicester project, the average number of security devices increased in the visited businesses. Different suggested measures had different compliance rates in Multnomah, which would suggest that the mechanisms were not in place in all businesses after the intervention. The effectiveness of some target hardening measures was examined in the Merseyside evaluation, where alarms and reinforced doors were found to be the most effective at foiling an attempted burglary. However, this does not account for measures which deterred potential burglars from getting to the attempt stage.

Another two possible mechanisms were discussed by the Leicester evaluation, due to the more widespread reduction in non-domestic burglary seen in the West End area. Firstly, it was considered possible that police service delivery patterns may have been altered in response to the project. Secondly, there was the possibility of heightened awareness of the increased security that was possible both among potential victims or potential burglars (with victims taking more precautions, and a publicity deterrent effect on). Due to the geography of the West End within the Central Area, this area wide impact was possible; this would not have been likely in Belgrave as it is part of the large, disparate, East Area.

DISPLACEMENT AND DIFFUSION OF BENEFITS

Tilley and Hopkins (1998: 19) identified five clusters where displacement from the visited businesses in Leicester had occurred, for example:

In one, for example, a supermarket selling alcoholic drinks which was very heavily victimised in sweep 1 had far fewer incidents in sweep 2, but an off-licence fifty yards away in the same small parade of shops had next to no incidents in the first sweep but was one of the most highly victimised in the second.

However, they also identified that diffusion of benefits may have occurred in one of the intervention areas, in a “relatively compact geographically confined area” (Tilley and Hopkins, 1998: 22).

There was no discussion of displacement or diffusion of benefits in the Multnomah evaluation. To put this into context, this intervention was carried out during the late 1970s, a time when the phenomenon of displacement was not as widely discussed as now, and well before the term ‘diffusion of benefits’ was coined by Clarke and Weisburd in 1994.

The Merseyside evaluation found no evidence for displacement. The potential displacement types examined were crime type, MO, and geographical area. Diffusion of benefits was not discussed.

The lessons to be learnt, therefore, are firstly that displacement should be examined in evaluations of this type. Secondly, where displacement did occur in the Leicester area, there was the potential for pre-emptively addressing this by expanding the interventions provided to near-neighbours of victims – the phenomenon of near-repeats is now a known factor, and is discussed in Chapter Three.

SEXUAL VICTIMISATION

Strikingly, evaluations of sexual revictimisation prevention tend to be written in a more medical style of report than other crime types included in this review. All those identified were randomised controlled trials, and tended to have a psychological emphasis. According to the Maryland Scale of Scientific Methods, these evaluations are more methodologically sound than those of other crime types included elsewhere in this review. However, when examined for the purposes of the realist synthesis, they also appeared to include less ancillary information than other evaluations. This restricted the additional value of these evaluations

from the perspective of a realist synthesis, but some valuable information was still gleaned, in the areas of mechanisms and implementation issues. It would be useful if, in future evaluations of this type, additional information were provided. The mechanisms and implementation issues which were identified are discussed below.

MECHANISMS

The mechanisms by which the sexual assault prevention programmes were designed to work did not have clear links with how they would translate to real world scenarios. Gidycz et al. (2001a) found that although the men and women on their programme learned a lot from it, they did not see it as being relevant to them. Yeater and O'Donohue (2002: 1142) identified that it was unclear whether the information included both in their programme and in other prevention programmes decreased women's risk of sexual victimisation, and indeed their study "did not test whether the program is effective at decreasing rates of sexual assault". Davis et al. (2006) found that higher levels of awareness and assertiveness had no association with avoiding revictimisation, which is suggestive that increasing knowledge of risky situations is an insufficient mechanism for preventing repeat sexual victimisation. Breitenbecher and Gidycz (1998: 486) suggested that the mechanisms of revictimisation addressed ("dissociative pathology, depression, anxiety, learned helplessness, dependency, and low self-esteem") may not actually be associated with revictimisation. Therefore, a greater understanding of the mechanisms which may be linked to revictimisation is needed in order to develop a more successful prevention programme.

IMPLEMENTATION

Problems with scheduling impacted adversely on the rate of attrition. For example, Yeater et al. (2004) scheduled a follow up session for their student participants during the holidays, and the attrition rate at this point jumped to 21.8%, despite the researchers attempting to circumvent the problem by sending out questionnaires by post or email. Davis et al. (2006: 12) found that the time of day was problematic for some of the participants, as they were attending at the end of the work day and were too tired to "absorb such emotionally laden material" – this study had a low 3-6 month follow up rate.

There was not a consensus amongst the studies examined about whether a woman should be included as a victim if she had been abused before the age of 14. For example, Breitenbecher and Gidycz (1998: 480) say that "contact child abuse is associated with adult revictimization" although they also state that non-contact abuse does not share this association. However, Marx et al. (2001: 27) disagree, stating that the risk for revictimisation is "significantly lower among women with a victimization history prior to age 14". Therefore, the definition of prior victimisation varied between studies. In all the studies examined for this review, the period examined for victimisation before the intervention was much longer (in some cases whole-life) than the follow up period (often 9 weeks).

In addition, the format of a randomised controlled trial was problematic for some of the programmes. Davis et al. (2006: 64) found that

In this regard, our experimental design was problematic: some of the counselors were only willing to refer clients to the study (and some of the

potential participants were only willing to consent) if they could be guaranteed they would get the intervention. They saw the intervention as a possible help to them, but the assessment without the intervention as a possible harm. Therefore, the experimental design may have eliminated some of the most motivated women.

The social nature of the programmes meant that there was the potential for intervention information getting to control group participants, particularly where there were multiple stages to the intervention, and the participants of both treatment and control groups were known to each other. To circumvent this difficulty, Davis et al. (2006) made an exception to the randomisation process in seven cases where women were sharing accommodation and there was therefore risk of contamination.

Schewe and O'Donohue (1993: 673) comment on another issue which can affect these sexual assault prevention programmes, that of socially desirable responses skewing the results. They criticise experimenters for failing to control for the possibility of such responses artificially inflating the scores of intervention groups.

DOMESTIC VIOLENCE

Within the systematic review, only one study met the eligibility criteria. Here, further evidence is used from that sole study, as well as the others which were rejected, to examine three areas. Overwhelmingly, interventions were targeted at female victims of domestic violence. The implications of this, and how this was dealt with by a number of studies is discussed in the first section here. There are a number of implementation difficulties which are specific to the nature of domestic violence interventions, and thus the second section is dedicated to such issues. Thirdly, the police do not always have an approachable reputation for domestic violence issue – it is only relatively recently that domestic violence has begun to be taken seriously as an offence (see Sherman, 1992). Therefore the final section here examines the effect that interventions have had on the relationship between the police and victims of domestic violence.

ELIGIBILITY FOR INTERVENTION

The studies identified here had a number of eligibility criteria for victims to access the available services, although generally these criteria were very broad. Typically, they were that victims of domestic violence lived in the geographic area covered by the programme, and occasionally an age limit was imposed – young victims could perhaps be better served by other services. However, some of the programmes had an implicit or explicit assumption that victims would be female, and perpetrators male. In the Killingbeck project (Hanmer et al., 2002) only female victims were eligible for the programme, and the focus throughout was on this presumed division of victim-offender roles. For example, the programme was designed to “acknowledge the women’s vulnerability... and directly confront the men’s behaviours” (Hanmer et al., 2002:3). This simplification of the distribution of guilt between the two parties was commented on by the police involved with the project:

a majority of the interviewed officers remained concerned that the project did not acknowledge that women were also aggressive and that Cocoon Watch was not always warranted.

Hanmer et al. 2002: 35

Not all interventions shared this focus on female victims. For example, Walklate, (1992:21) mentions that the victim was “usually” a woman – but from thereon in, the report solely talks about interventions in terms of females. However, even where there were clear eligibility criteria which incorporated the notion of male victims, few (if any) men were involved in the interventions in this role. For example, Lloyd et al. 1994 defined domestic violence as between those who cohabit or cohabited rather than in terms of age, gender or sexuality (p.1), but referrals to the project were adult women (see, for example, “the view from the women” *ibid.* p.10). The Cardiff multi-agency approach to domestic violence was open to the notion of non male on female violence, but dealt with no male victims, and only one case of female on female violence (Robinson, 2006). Of the 94 victims who met with the project worker in the Tamworth domestic violence project, only three were male (Wangmann, 2002). There was no mention in the report of how many victims who were eligible but did not participate were male.

The NDV intervention was perhaps the most successful at allowing broad parameters of interventions for domestic violence situations, as it had a ‘Level X’ designation for any unusual situations which allowed a directly tailored approach. Such situations included:

- Incidents where one or both parties are intellectually disabled;
- Incidents where one or both parties are from outside the project areas;
- Incidents involving “dysfunctional families” where both parties offend;
- Other incidents deemed inappropriate for the normal intervention structure.

Morgan, 2004: 6

This programme also “seemed to leave room for a wide range of gender and age combinations of intimate family violence” (Morgan, 2004: 20). As such, this was the most flexible programme designed to reduce repeat domestic violence which was identified in the literature.

IMPLEMENTATION

Discretion was an issue for a number of victims. Morgan (2004: 65) commented that one victim involved in the intervention was uncomfortable with contact being made whilst she had visitors, and this was a deterrent for using such a service. “She found the police presence in her home embarrassing and said she would contact police only if a future incident was severe”. This need for discretion was supported by comments from victims involved with other interventions. One victim involved in the Tamworth project commented:

if [the project workers or police] come round here, people start wondering what the devil is going on, so I would rather go down and see them, you know, so nobody’s looking over the back fence and saying ‘what in the hell’s going on here?’

Wangmann, 2002: 30

A number of victims involved in the Merseyside demonstration domestic violence project appreciated the officers installing panic alarms whilst in plain clothes in order to minimise attention (Lloyd et al., 1994; Farrell et al., 1993). A consideration for future domestic violence project may be how best to approach victims without unnecessarily drawing unwanted attention to the victim.

Safety of project workers is an implementation issue which warrants careful consideration. This is particularly valid for domestic violence incidents where the perpetrator resides with the victim. However, safety concerns can interfere with the delivery of an intervention. The project officer in the Tamworth project found it very restrictive to co-ordinate home visits with the police, and was prevented from following up on letters which received no response on safety grounds (Wangmann, 2002). The use of MARACs in the Cardiff intervention allowed risk assessments to be shared amongst the agencies present, providing greater levels of safety awareness (Robinson, 2006). The need to balance safety of project staff with providing a suitable service to victims is essential.

Data problems within domestic violence interventions are common, as with other crime types. Acquiring referrals from the police and other agencies can be a complex process. Difficulties encountered included the need for data to be entered on multiple databases (Hanmer et al., 1999; Lloyd et al., 1994), paper based records being overlooked (Wangmann, 2002), and problems with the inflexibility of recording fields on databases (Morgan, 2004). Wangmann (2002) suggested that providing the project worker with direct access to the police records would circumvent some data related problems. However, this may not always be possible, particularly when the researcher or project worker is not based at the same site as the police, or where there are security concerns (as would be expected with most police records). It is worth noting that databases are constantly improving, and are now ubiquitous, so some of the issues encountered (particularly by the earlier studies) may be far less prevalent by the time of writing.

PERCEPTIONS OF THE POLICE BY VICTIMS OF DOMESTIC VIOLENCE

Strikingly, across the programmes examined, the same message about the police emerged: that there was a lack of consistency in the approach to victims. Where good practice was followed, with positive and sensitive attitudes being displayed by the police, the victims in turn were generally positive about the police. Victims dealt with by other agencies in the Killingbeck project expressed a “concern with the lack of consistency in police practice” (Hanmer et al. 1999:36) and this was explained as follows:

Some officers were perceived as sensitive and helpful while others were viewed as negative and unsupportive of women’s situations.

Hanmer et al. 1999: 36

Victims were generally impressed with police attitudes and responses within the domestic violence demonstration project on Merseyside, although this satisfaction lapsed somewhat when discussing the police responses to false alarm activations (Lloyd et al. 1994). Morgan

(2004: 66) similarly found that victims were generally positive “about the level of service they received from police connected with the NDV project”. When dissatisfaction was expressed by these victims, it generally related to a specific aspect of the response that they had received, for example with difficulties in contacting the police (victims may not want to leave a message as a returned call may be picked up by the offending party).

DISCUSSION

There were three broad purposes of the realist synthesis laid out in the methodology. A discussion of the extent to which there is sufficient information to address these points, and the broad conclusions which can be drawn, is presented below.

ARE THERE IDENTIFIABLE DIFFERENCES BETWEEN SUCCESSFUL AND UNSUCCESSFUL INTERVENTIONS IN THE WAY PROGRAMMES ARE IMPLEMENTED?

There were distinct differences between unsuccessful and successful programmes in terms of implementation difficulties and how they were dealt with, although some implementation problems were also commonplace in successful interventions. Staffing problems were common in repeat domestic burglary interventions with a low effect size, or where crime incidence did not fall. These problems were often coupled with other difficulties, including communications breakdowns and a lack of flexibility in making necessary changes and the way problems were dealt with. Data problems were not insurmountable, as four successful programmes dealt with these, but often meant that measures of success (i.e. effect size) could not be ascertained – all but one of the excluded domestic burglary studies cited data difficulties in their evaluation reports. Two thirds of the successful repeat domestic burglary studies had fewer than three of the implementation problems examined here.

The differences between successful and unsuccessful programmes (as measured by the effect size) could not be examined for the other crime types included in this review. With the exception of one of the measures in the Gidycz (2001) sexual revictimisation evaluation, all of the other studies (which were suitable for assessing effect sizes) were successful, and therefore no contrast to be drawn. There were identified implementation issues in the remaining crime types. Staffing considerations were again seen in commercial crime and domestic violence, with safety concerns paramount in the latter. The sexual victimisation programmes had a particular problem with attrition, which was not seen in the same way with area based programmes, and the randomised controlled trial design presented some problems for recruitment. In all, implementation problems across the four crime types examined in this realist synthesis were common, but not inevitably insurmountable.

ARE THERE IDENTIFIABLE DIFFERENCES BETWEEN SUCCESSFUL AND UNSUCCESSFUL INTERVENTIONS IN THE TECHNIQUES UTILISED BY THE DIFFERENT PROGRAMMES?

The different crime types examined here utilised a broad range of techniques, which makes generalisation difficult. As an overview, the effect sizes were greater where a broad package of measures was tailored to the individual contextual circumstances. Basing the techniques on

a previously successful intervention was no guarantee of success, as this did not take into account the inherent situational differences between the prior and the current intervention.

DOES PREVENTING REPEAT VICTIMISATION INCREASE THE LEVEL OF CONFIDENCE IN THE POLICE?

This question was specifically raised during the consultation process, but there is a dearth of information available on confidence in the police, in the context of repeat victimisation prevention, and none at all for the sexual victimisation studies included in this meta-evaluation. The information which was available suggested that the success of a programme was irrelevant to levels of confidence: confidence in the police increased regardless of whether the intervention reduced the incidence of crime, it was the attempt to address crime issues which was important.

CONCLUDING COMMENTS

This realist synthesis has provided a different type of information from that which was concluded from the systematic review. Rather than examining whether or not an intervention has worked, the focus has been on underlying theories, mechanisms and contextual factors. The contributions each methodology examined in this section have made to the meta-evaluation of repeat victimisation prevention interventions, and their complementary and contradictory elements, are discussed in the following chapter.

SECTION III:

ANALYSIS AND CONCLUSIONS

I think and think for months and years. Ninety-nine times, the conclusion is false. The hundredth time I am right.

- *Albert Einstein*

Chapter Eight: A Comparative Analysis of Systematic Reviews and Realist Syntheses

If you have an apple and I have an apple and we exchange these apples then you and I will still each have one apple. But if you have an idea and I have an idea and we exchange these ideas, then each of us will have two ideas.

- George Bernard Shaw

INTRODUCTION

Chapters Four to Seven made use of the methodological approaches of both the systematic review and realist synthesis to conduct a meta-evaluation of programmes designed to prevent repeat victimisation. In conducting this meta-evaluation it transpired that each approach appeared to make distinct positive contributions, which enabled richer, more rounded conclusions to be drawn. Van der Knaap et al. (2008) have also conducted a trial of this combined approach, with promising results. To anticipate the conclusions of this chapter therefore, it is that the approaches are compatible, that aspects of both need to be utilised to improve evaluative quality, and that the resultant 'realist systematic review' should prove a fruitful third way for evaluation research.

In what follows, it is often necessary to refer to both approaches alongside each other, and for simplicity and consistency the experimentalist approach (systematic reviews) is typically referred to first followed by the realist approach (realist synthesis). No preference should be inferred from this practice.

CORE PRINCIPLES

This section examines some of the key aims and concepts underpinning the methodologies. It illustrates that despite some important differences, core principles of each methodology share significant common ground.

POLICY-ORIENTATION

The experimentalist and realist approaches set out to accomplish positive changes in policy and practice. Neither are theoretical exercises, designed to answer philosophical or abstract questions. Rather, they both share an aim to improve the way in which interventions are incorporated into policies, and reject any pressure to provide a quick fix solution. They each aim to provide a balanced interpretation of multiple findings within a specific field of intervention. This is a key element for providing a long lasting, constructive effect within crime prevention efforts.

STANDING ON THE SHOULDERS OF GIANTS

Rosenthal (1991) suggested that whereas within the physical sciences evidence cumulates over time, this is not standard practice within social sciences. Instead, there can be a tendency to start afresh each time a solution to a problem is sought. Both realist synthesis and systematic reviews work towards addressing this, by attempting to provide a clear understanding to policy makers and practitioners of what has been attempted in the past.

There is therefore a clear consensus between these two methodological approaches that in order to make well informed decisions, practitioners and policy makers need to have an understanding of the work that has been done in an area, and the relative successes and failures of that work. By bringing together existing crime prevention evaluations in one place, a reference guide is easily available to the end user. This means duplication of interventions which have little or no impact can be minimised, thus saving on time and resources. In this way, new initiatives have a greater chance for success and have the opportunity to establish the foundations for effective crime prevention. This forms a key contribution of both realist syntheses and systematic reviews: that by understanding and explaining successes in the past, future interventions can be usefully scoped and applied.

SCIENTIFIC METHOD

Experimentalist and realist approaches have scientific principles at their core, although these are sometimes interpreted in different ways. This is significant common ground however. Both seek to use empirical evidence and fact to inform their conclusions. This distinguishes them from some criminological approaches which distance themselves from the 'hard' sciences, as they do not accept the relevance to social phenomena. Some components of 'cultural criminology', for example, which draws on cultural studies, appear to be based on a wholesale rejection of empirical evidence and a dismissal of the use of statistical analysis (Farrell, 2010). The prevalence of 'cultural' approaches in academia means that the scientific approach should not be taken for granted. In this larger picture the realist and experimentalist approaches are allies working in parallel towards a common goal of informing policy.

Pawson frequently draws comparisons between the realist approach to evaluations and the physical sciences (see for example Pawson 2006; Pawson and Tilley 1997). This work emphasises the importance of context and mechanisms within assessments of effectiveness. One of Pawson's examples is the explosion of gunpowder. Scientists know that gunpowder ignites only given the right context – where oxygen exists to allow combustion. Where the context is unfavourable, for example where the gunpowder is damp, there will be no ignition. The parallel with crime prevention initiatives is that contextual factors can provide an intervention with a much higher, or lower, chance of success. Pawson expands on this analogy to include the importance of mechanisms – scientists understand the chemical reactions which take place to allow the ignition to take place, and using this understanding can predict where similar outcomes will be apparent. Likewise, an understanding of *why* an intervention was successful will enable similar predictions to be made about the potential success of similar crime prevention interventions applied elsewhere.

At a more fundamental level, realist synthesis does not look for a final answer, only a theory which will be refined, or even rejected as new evidence comes to light. This is commonplace

within physical sciences – even those basic premises such as gravity and evolution which can be applied to successfully explain everyday phenomena, are still referred to as theories, and as such may potentially be refined should new evidence come to light. Thomas Henry Huxley put it succinctly: ‘Science is organized common sense where many a beautiful theory was killed by an ugly fact’.

The experimentalist response to the tentative nature of all knowledge is an explicit assumption that findings are reviewed as new evidence emerges. Specifically, authors of systematic reviews are actively encouraged by the Campbell Collaboration to regularly return to their reviews to update the findings. In this way, both systematic reviews and realist syntheses are informed by the concept of evolutionary epistemology, whereby new theories are refined and replace old theories over time, and new problems and questions emerge through this process.

Farrington and Petrosino (2001) explain that within the Campbell systematic reviews of crime and justice, explicitly stated objectives, eligibility criteria and search methods are used. Each study is screened and the aim is that all relevant, eligible evaluations are identified and included. Finally, the report is structured in a way that enables each stage and decision to be understood. In this way Campbell reviews are explicitly aiming for parity with scientific methods.

In all then, systematic reviews and realist syntheses both strive to be scientifically sound in the ways they assess and evaluate evidence. They share the understanding that findings may need to be reassessed in the light of new evidence. Moreover, a number of systematic reviews have started to recognise the importance of context and mechanisms. For example, in their 2008 systematic review of the effect of street lighting, Welsh and Farrington recommend further research is needed ‘to help identify the active ingredients of effective CCTV programs and the causal mechanisms linking CCTV to reductions in crime’. This is an important development in the integration of elements of these two approaches.

TRANSPARENCY

Farrington and Petrosino (2001) discuss features such as explicit objectives and eligibility criteria, which contribute to the essential transparency of systematic reviews. In practice a systematic review is planned in advance. This plan is published in the form of a protocol, and any deviations from this must be clearly justified and explained. Every decision, search, reason for exclusion of studies, and analysis is presented clearly in a step by step fashion. This transparency is important so that other researchers can reproduce the findings, and that any flaws or disputes can be highlighted. For example, other evidence which was missed in the searches can be identified by readers and brought to the authors’ attention, or reasons for exclusion of a study disputed.

Pawson et al. (2004:24) suggest that this mechanical approach, and its aspiration to absolute transparency and replicability is, however, ‘economical with the truth’. This claim stems from the argument that there is a whole spectrum of possible decisions which will inevitably vary, however subtly, from one reviewer to another, particularly as theories are built up and tested within a realist synthesis. This is not to suggest that the realist approach does not aspire

to transparency. Indeed, within the same text, Pawson et al. (2004:22) argued that a 'perfect' realist synthesis would have every judgement written down for transparency purposes.

The goal of being able to understand and replicate every stage of a review, be it realist or systematic, is certainly a laudable one. It brings reviews closer to the scientific ethos which stands at the heart of both approaches. This is how physical sciences develop and test theories: by allowing other scientists to access their thought processes, assumptions, and actions, there develops an inherent ability to constructively critique work for the benefit of all concerned. There are obvious difficulties in achieving this with the more qualitative data that the realist approach utilises – inferences, judgement calls and developing broad theories are inevitably more difficult to justify, and certainly cannot be planned for in the same way in which the protocol for the systematic reviews determines. However, there is scope for much greater strides to be taken by realist synthesis towards this model of transparency. Transparency has been at the heart of this thesis, and by treating the two approaches as two stages in the review of repeat victimisation prevention programmes, the transparency of the systematic review has also been carried through to significant aspects of the realist synthesis process.

TECHNIQUES

The generalities of the experimentalist and realist approaches have now been covered. The discussion which follows takes the reader through the practical stages approximately as they would occur in a review of crime prevention interventions, with a comparison of the main elements where similarities and differences between the two approaches can be discerned.

STAKEHOLDER INVOLVEMENT

Both systematic reviews and realist syntheses work with the end user in sight. The final reports are, in both cases, designed to be disseminated to and useable by both academic and non-academic audiences, and are particularly practitioner and policy maker oriented. Realist syntheses take this one stage further, and it is a methodological requirement to involve stakeholders from very early stages of the review process. This enables the research question to be narrowed down, which is particularly important for a realist synthesis because of the wide range of potential questions. The involvement of stakeholders from such an early stage means that the end product is already being shaped into something that will be of practical use. Further consultations can be carried out with the stakeholders as the review progresses, where they can be updated with progress and also provide further input that identifies additional lines of enquiry flagged by the process. In keeping with this philosophy, this thesis kept stakeholders updated of progress, where they had expressed such an interest, and all stakeholders are to be provided with copies of the final thesis. The ability to identify practical applications for the work is essential for both approaches. Although systematic review authors are encouraged to consult with key people working in the field (Konnerup and Sowden, 2008), they do not appear to make stakeholder involvement a methodological requirement as it is in the realist approach.

SEARCH FOR STUDIES

Methodical searches are essential for a successful systematic review. The aim is to locate all relevant studies, regardless of whether these are published or part of the unpublished 'grey literature' of reports and manuscripts. The search plan is specified in the protocol. This includes the electronic databases to be searched, and keywords which will be used in these searches. Additional searches, including snowballing from references in the literature and contacting key authors, are also required. Searches generally take place at the beginning of the review. Although comprehensive in the coverage of the specific question asked by the review, this method risks losing some of the theoretical dimensions covered in literature within other disciplines.

Realist synthesis takes a different approach to searching for relevant studies. This method acknowledges the likelihood that regardless of how comprehensive a search is, relevant material is still (probably) going to be missed. An added dimension to this is that many of the searches are attempting to track theories, rather than searching for specific studies. Here, keyword searches would often be so broad as to be useless. As a result, searches for a realist synthesis rely far more on the snowballing approach, and rather than stopping searches when all studies are found, the searches end at theoretical saturation – where no new information is being added to the review. Searches take place as an ongoing process throughout the review. The potential danger with this approach to searches is that information is only found which supports the view that was in the initial papers identified (which will be more likely to have cited supporting views, which in turn will have cited papers with similar views, *ad infinitum*).

It seems likely that key components from the two approaches can be usefully combined. Whilst conducting this review on preventing repeat victimisation, beginning with a systematic search did give confidence that all relevant studies were identified. This had the advantage that theories could naturally be identified from those relevant studies, and then these theoretical streams followed by additional searches across disciplines to the point of saturation. In this way, proponents of both approaches should be satisfied, and the review as a whole became more rounded. For example, many of the identified studies on domestic burglary revictimisation aspired to fit target hardening measures quickly, and often supplied temporary equipment such as alarms. However, few explicitly explained the reasons behind this beyond a brief mention of the time course of repeat victimization (that there is a peak of risk shortly after the initial offence takes place (Polvi et al., 1991)). Tracking the theory into the literature beyond the studies identified that the time course varies depending on local issues (Weisel, 2005). Future interventions designed to prevent repeat burglary victimization may therefore benefit from examining the local time course of repeat victimization in order to determine whether or not temporary measures are sufficient, and how quickly target hardening should be implemented for optimum results. This theory refinement was identified through questioning the eligible studies closely, and tracking the theory beyond these, an act only possible by inclusion of both experimental and realist approaches to the review process.

FILTERING OF RELEVANT STUDIES

Systematic reviews filter out studies on two main features. Firstly, the evaluation is assessed to see whether it is directly relevant to the research question. Secondly, the evaluation is weighed up against the quality requirements for the review (outlined in the next section).

Each of these decisions is taken against the background of the protocol, in which both quality and relevance criteria for inclusion have been established in advance. This process is conducted at the start of the review, before studies are coded.

Although realist syntheses also filter out studies, this process occurs throughout the synthesis, and is more fluid and iterative than within a systematic review. The reviewer makes a judgement on whether a piece of evidence should be considered with greater caution because it conflicts with the consensus established by the rest of the evidence available to the reviewer. According to Pawson et al. (2004:23) 'the worth of studies is established in synthesis' and therefore should not be prejudged through early exclusion. This does not mean that an entire evaluation need be excluded, but subsections can be disregarded where they are considered to be methodologically questionable, or other findings cast doubt on the reliability of a particular statement or conclusion.

In each of these approaches, leading proponents sanction making all judgements transparent. The key difference here is whether to eliminate at the start of the review, thus ensuring that decisions cannot be subject to bias at a later stage, or whether to gradually eliminate as the review progresses. Within the more rigid guidelines of the systematic review, early elimination makes sense. However, whilst conducting this review of repeat victimisation prevention, there was often a need to go back to those excluded studies for the purposes of the realist synthesis, as a wealth of information was often to be discovered in those evaluations which did not meet the criteria for inclusion in the systematic review.

QUALITY

The Maryland Scientific Methods Scale (SMS) is frequently used by crime and justice systematic reviewers to assess the quality of identified evaluations as part of the exclusion process. This is a five point scale, with randomised controlled trials scoring five points. Evaluations which have non-randomly allocated multiple intervention and comparison units, with measurements taken before and after the intervention, form Level 4 on the scale. Level 3, an evaluation with a pre- post- measure within both a single intervention area and a comparable control group is the minimum interpretable standard of evaluation design according to Cook and Campbell (1979). SMS is often criticised as being an unrefined way of assessing quality, but according to Farrington et al. (2002) it was designed to be more user friendly than the previously existing scales used to effectively measure internal validity, for example that designed by Brounstein et al. (1997). Farrington et al. (2003) explain that Brounstein's scale, whilst effective, was perceived as too complex, as it had 100+ points on which to grade an evaluation. The SMS in contrast was designed to be 'easily communicated to scholars, policy makers and practitioners'. SMS assesses internal validity only, and as such cannot be relied upon for complete quality appraisal. To address this, Farrington (2003) suggested that statistical conclusion validity and construct validity should also be evaluated.

Realist synthesis relies on the ability of the reviewer to make a judgement call on the quality of a study (Pawson et al., 2004). This allows for the inclusion of a wider range of studies, though it can be argued that this can cloud the final conclusions of a review, and risks lowering transparency. An advantage of this approach to quality is that often studies may be excluded in a systematic review for having insufficient data available, but other information could be gleaned which is of use to policy makers and practitioners. For example

implementation failure may prevent results from being accrued, but the problems that were faced can provide useful lessons for others hoping to set up a similar intervention. Systematic reviews currently risk losing this valuable data. However, the disadvantage of this approach to quality is that there is no clear cut off point, and different reviewers may have very different ideas about what constitutes useful information.

A potential problem with the realist approach lies in the notion of a 'judgement call'. Judgement can be subjective and can vary from one reviewer to the next. A more experienced reviewer might include or exclude studies that another reviewer might not, and for different reasons. It is here that the realist approach appears to lack some of the transparency that is the hallmark of the systematic review process. Judgement calls can be transparent insofar as they can be described in the review report, but this does not ensure consistency across reviews. In contrast, the SMS is designed to promote consistency of method in different reviews thereby eliminating subjectivity.

At the same time, a strength of the realist approach is that it allows information to be included in a review that would be lost to a systematic review. There may be important information relating to data analysis, theory formation, and the implementation of preventive measures, that is discarded if the evaluation does not meet Level 3 on the SMS.

It is possible that the two approaches might be reconciled. The following is a preliminary suggestion: Following the identification of studies that meet the requirements of Level 3 of the SMS, a second stage of review would examine excluded studies for pertinent information. Such information could relate to particular aspects of the information specified in the coding frame such as implementation issues and other contextual factors. This was largely the technique used in this thesis. Excluded repeat victimisation prevention evaluations from the systematic review often contained a wealth of additional information on the very implementation problems which led to their ineligibility for the systematic review (for example data collection problems restricting the use of a comparison area).

DATA EXTRACTION

Systematic reviews identify the salient points of studies through coding manuals and forms (see for example Lipsey and Wilson, 2001). This is a reference guide for the coder, which lays out in detail the information to be searched for, and a standard form to fill out. These forms do not allow for many of the subtleties found in a research study, as they are designed to be used in aggregate, and therefore used for information which is reported in most of the available studies. By assigning numeric values to different items of information, meta-analysis within a systematic review can be facilitated. Coding manuals and forms provide a structure to data gathering which ensures that the same information is extracted from each study to be assessed, and that any missing data is identified as such.

In contrast, realist syntheses use annotation more than the traditional data extraction described above. Pawson et al. (2004) characterise the process as being surrounded by piles of notes and papers. Rather than taking a linear approach to identifying the relevant information, in a realist synthesis the studies are returned to repeatedly as, for example,

theories are formed and refined. In this way, there is a step away from using a uniform extraction sheet as used in the systematic review.

However, each of these approaches uses bespoke forms where appropriate. The key difference is that in a systematic review, generally a single form is developed for the review, whereas in a realist synthesis a multitude of forms may be used, or different sections used depending on the study. These are not inherently incompatible, and in this review of repeat victimisation prevention interventions both were used. Firstly, a traditional coding manual and form with specific questions and possible answers drew out details such as: population of treatment and control groups; housing type; crime prevention tactic(s) used (see Appendix B for an example). Secondly, the same studies were addressed using a more qualitative form, with space for narrative annotation under broad categories, for example: context; and implementation comments (See Appendix C for an example). This two-tier approach added a richness which either method alone would not have provided. The coding manuals drew out those key items which were essential to ground the understanding of the repeat victimisation prevention studies, and the qualitative forms filled in the details. To give an example, whether implementation problems occurred would be flagged in the initial coding, which could then be simply compared to success or failure of a study. The features which caused implementation failure were pulled apart in the qualitative forms, and similarities could then be compared and contrasted across both successful and unsuccessful interventions to establish common problems which correlated to failure.

ASSESSING EFFECTIVENESS AND DRAWING CONCLUSIONS

Systematic reviews often make use of statistical meta-analyses to establish whether a type of programme has been effective or not (see Chapters Four and Five for the effect size calculations and resulting charts used in this thesis). The conclusions are generally presented in a concise way, and the whole review is presented with a short summary of findings. Graphs are often used to illustrate the success or failure of interventions, and frequently a single sentence will tell the reader whether or not the intervention shows signs of success, for example in a systematic review of the effectiveness of CCTV:

Results of this review indicate that CCTV has a modest but significant desirable effect on crime, is most effective in reducing crime in car parks, is most effective when targeted at vehicle crimes (largely a function of the successful car park schemes), and is more effective in reducing crime in the United Kingdom than in other countries.

Welsh and Farrington, 2008

This example also shows that systematic reviews do not entirely neglect the concept of context within their conclusions, an accusation often launched by proponents of the realist approach. However, realist syntheses do inevitably give more detail about the contextual factors and mechanisms which may influence the success of an intervention. A small scale realist review, also of CCTV, but in just two stores (Gill and Turbin, 1999) indicated:

It is plausible to suggest that, in this context, CCTV encouraged staff to approach suspected shop thieves and that the system helped them to monitor suspicious individuals.

The same review went on to explain:

A system introduced to a store where staff welcome the CCTV and want to work with it may create the appropriate context for triggering crime-reducing mechanisms. Equally, a store where staff resent the system may trigger different mechanisms, with the potential to increase losses (by reducing staff vigilance or concern about shop theft).

and explicitly discussed a mechanism investigated:

One way in which CCTV appears to work is by interacting with the staff, who then influence the shop thieves.

The above realist review extracts illustrate the complexity of the conclusions drawn by this style of synthesis. Although this additional complexity can feel excessive at times to the casual reader, it is of course essential for a practitioner to understand not only *if* an intervention works, but also *how* to replicate that success. This is something which a systematic review, by the very essence of its simplicity, can sometimes omit.

IMPLICATIONS FOR FUTURE META-EVALUATIONS

There are many positive aspects to be taken from both systematic reviews and realist syntheses. Many of these positive aspects are common to both approaches. The optimist would conclude that the approaches are compatible allies. They are united in their scientific and evidence-based approach to informing policy – key underlying commonalities that are easy to overlook but which should not be taken for granted in the social sciences. They both promote transparency that allows for the gradual and incremental accumulation of knowledge. There is also evidence that the two approaches have each learned from each other. For example, there is clear evidence that key proponents of systematic reviews have sought to pay greater attention to contexts and mechanisms than previously.

Systematic reviews provide the broad brush strokes to answer the question of *whether* a crime prevention intervention has been shown to be successful, whilst realist syntheses fill in the fine detail of *why* an intervention has been successful, and, in the better examples, of *how* to replicate that success.

It would of course be theoretically possible to integrate elements of each approach into the other. Indeed, this has already been seen to an extent. The constructive criticisms each presents to the other have often been taken on board by the recipient and have served to push the methodologies forward in the manner of co-evolutionary organisms. However, systematic reviews and realist syntheses are at heart essentially asking different questions of the available data. They are not in direct competition with each other to assess the information available. Systematic reviews and realist syntheses both work scientifically to improve policy and practice, but have complementary techniques to contribute to this push for evidence

based policy. Systematic reviews assess the highest quality evidence according to experimental measures, to show whether or not the intervention can be proven to work, but in doing so might miss the subtle nuances of why this occurs. Conversely, realist synthesis provides an excellent analysis of the context and mechanisms which provide the greatest chance for success, but does not necessarily provide clarity on what constitutes a successful outcome.

An integrated two-tier approach to evaluating the success of crime prevention interventions would seem to be compatible with both approaches. A systematic review would be carried out following the stages as laid out by the Campbell Collaboration, using extensive literature searches to identify relevant evaluations and studies. However, once the systematic review is complete, reviewers should return to the literature - both that which was included and that which was identified during the systematic review process as being relevant (i.e. those evaluations and studies which were excluded due to not meeting the high scientific quality required by a systematic review, rather than those with a focus on a different area). This return to the literature should then turn to those detailed questions on contexts and mechanisms. Each stage of this realist systematic review would be preceded with extensive consultations with relevant practitioners, policy makers and fellow academics.

The chief advantage of combining both systematic reviews and realist syntheses is that it should enable reviewers to have confidence in what works, whilst also establishing grounds for improving theory and practice in a more applicable way. It also allows each methodological approach to meta-evaluation to focus on what it does best, without distraction. In this analysis, systematic reviews and realist syntheses are complementary, not contradictory, and the combination of the two to address the question of 'what works, for whom, and in what circumstances' can only produce positive synergies, resulting in further progress for crime prevention initiatives.

The aim of proposing a 'realist systematic review' as a third way is not to compromise either approach. The risk is that proponents of the two earlier approaches perceive it as compromising the integrity of their work. The aim, rather, is to take the strongest components of each to achieve an improved methodology that ultimately leads to more informed crime prevention policy.

Chapter Nine: Conclusions & Recommendations

I haven't failed. I've found 10,000 ways that don't work.

- Thomas Edison

The conclusions and recommendations which follow are split into two broad categories. Firstly, the overall conclusions about repeat victimisation prevention programmes are pulled together, using both quantitative and qualitative evidence from across the review as a whole (i.e. using evidence from both the systematic review and realist synthesis). This includes recommendations on the implementation and evaluations of such programmes, as well as thoughts on the validity of the underlying theories of repeat victimisation prevention interventions. Secondly, the advantages, disadvantages and lessons learnt about systematic reviews and realist syntheses are discussed. Chapter Eight provided a comparative analysis of the two approaches, so the focus here is on the overall contributions each has made in the context of this review of repeat victimisation prevention programmes, and suggestions for the possible future directions of meta-evaluations of crime prevention interventions.

REPEAT VICTIMISATION

IMPLEMENTATION AND METHODOLOGICAL ISSUES

The review showed that there were common implementation issues with both successful and unsuccessful prevention programmes. However, the most successful programmes generally avoided the staffing problems, by carefully recruiting appropriate people who were committed to the project. It was difficult, although not impossible, to establish the same level of commitment from volunteers as from paid staff. The police were occasionally distrustful of interventions which were introduced, but this was generally where there was a lower level of training and information given in the early stages. Implementation problems were not necessarily related to the research team, but to the multiple agencies involved in delivering the interventions. Notably, the most successful programmes worked flexibly, in a manner which enabled them to tailor their responses according to local need and to address problems as they arose.

RECOMMENDATION ONE:

Staff should be carefully recruited and be allowed a level of freedom to enable them to respond and make alterations to interventions as they see fit. All changes to a planned programme should be documented and justified.

The eligibility for repeat victimisation measures varied between the different programmes, and was the source of some confusion. Some of the interventions were designed to be implemented only when the business or individual had been victimised on more than one occasion. In other interventions, there was a dilution of the focus on *repeat* victims by introducing 'vulnerability' (which had differing definitions but typically related to seniors or

single mothers) as an appropriate target – often with no underpinning explanation or apparent justification for why some groups were considered to be at greater risk.

RECOMMENDATION TWO:

The risk of being victimised is increased after a single victimisation. To maximise chances of success and most widespread benefit, the point at which an intervention should be introduced is therefore after the first victimisation

RECOMMENDATION THREE:

Targeting resources at victims is an effective way of targeting resources to those at greatest risk. Other vulnerability factors should not be considered as these can be socially divisive and risk diluting the impact of a repeat victimisation prevention programme.

Weisburd et al. (2010) assessed the implementation of problem-oriented policing programmes, and discovered three significant problems. These same problems are common to repeat victimisation prevention programmes.

1. The conduct of the repeat victimisation prevention programmes can be formulaic. Interventions from elsewhere are sometimes applied without tailoring them to the particular needs of the situation. This can lead to inappropriate responses being employed and may result in an ineffective programme.
2. There are difficulties delivering the repeat victimisation prevention programme on the ground. Not all victims want to take advantage of the help offered, for a variety of reasons, including disillusionment, apathy, embarrassment, or distrust. Legal barriers to some crime prevention measures can be encountered (particularly with alley gates which may need to be placed on shared or public property) and there can be difficulties identifying victims from the data which is available to researchers.
3. Evaluation of repeat victimisation prevention programmes can be poor, or non-existent. Evaluations often fail to present the core information and data which would enable a thorough assessment. The issue of evaluation is discussed in greater depth in the next section.

Simple measures can improve implementation. Tilley (2010) compared this to the positioning of washing facilities to improve rates of hand washing in hospitals. It is hoped that the simple recommendations in this chapter go some way towards producing similar improvements in future repeat victimisation prevention interventions.

EVALUATIONS OF REPEAT VICTIMISATION PREVENTION PROGRAMMES

Assessing the success of repeat victimisation prevention programmes presented some difficulties due to the information available in the evaluations. To be accurate, both prevalence and incidence need to be measured in order to determine whether there has been any change in repeat victimisation, the element which was targeted by the interventions examined in this review and derive concentration rates. In practice, the measure of incidence was usually the only available outcome measure, with the exception of the sexual victimisation studies (which, conversely, did not measure incidence). The reason for this was

that incidence was the measure recorded in the studies, and compared to the counterfactual. Where prevalence was also measured, thus allowing insight into change in repeat victimisation, this was just in the treatment area (in three quarters of domestic burglary evaluations, other crime types were somewhat more likely to have repeat victimisation measured in a comparison area) and was not recorded for the comparison area. The only comparable measure was therefore crime incidence. Although it is desirable that interventions reduce incidence via the mechanism of reducing repeat victimisation, it is not the primary measure of success of the techniques. In addition, a minority of studies identified in the early stages of the review claimed to be examining repeat victimisation, but did not continue on to do so (for example Plymouth and Mid North Coast).

RECOMMENDATION FOUR:

Evaluations of repeat victimisation prevention programmes should include measures of both prevalence and incidence in treatment and comparison areas. Changes in prevalence demonstrate the impact on victims, whilst changes in incidence are illustrative of the broader impact on crime.

The extent to which an effect is anticipated needs careful consideration. Where the intervention is provided to a small number of victims, it is perhaps not appropriate to measure the impact on an area-wide basis (see for example Cambridge, where 28 victims were provided with measures to prevent repeat burglary, and the change in incidence was measured across the ward as a whole). Individual-level data, manipulated in a fashion such as that recommended by Bowers, Lab and Johnson (2008) would be appropriate.

RECOMMENDATION FIVE:

It is helpful, where possible, to evaluate the success in terms of those directly in receipt of the intervention, as well as on a wider crime reduction basis. This enables a 'proof of concept' to be identified if the project can be deemed to have been successful for those individuals – other forces may be at work which mask wider effects.

The evaluations in general had inconsistent information available, which made direct comparisons difficult. Often, the information was very clear in some aspects – for example the outcome measures, whilst lacking in others – for example implementation difficulties or socio-demographic features. In addition, the reason for selecting particular interventions was not always justified in the evaluation report. This may or may not have been an indication that the package of interventions was not tailored to the situation, but this was certainly the implication, particularly where a minimal justification was used (for example, these interventions were used because Kirkholt or Biting Back used these interventions). A lack of clarity, or contradictory evidence within a minority of evaluation reports added to the difficulties experienced with drawing comparisons across multiple studies.

THEORETICAL UNDERPINNINGS

The underpinning mechanism examined within this review was the use of repeat victimisation to identify higher risk targets, and then to interrupt the victimisation cycle with a tailored package of measures. From the evidence synthesised in this review, there is no

reason to doubt the validity of this mechanism as a tool to reduce crime. Where repeat victimisation fell in the interventions so too did the overall crime incidence (with two exceptions). Only prevalence was measured for the sexual revictimisation studies, and so the validity of this mechanism cannot be tested here.

Two other supporting theories threaded throughout the repeat victimisation prevention interventions were those of the journey to crime (Brantingham and Brantingham 1975), and routine activities theory (Cohen and Felson 1979). The journey to crime theory suggests that offenders are more likely to commit crimes near to their transport routes, or within a limited distance of nodes of activity. There was insufficient evidence to suggest whether or not consideration of this improved the success of an intervention, however, the emphasis here was on victims rather than offenders, so perhaps unsurprisingly offending patterns were not considered in all (or even most) of the interventions. Routine activities theory was used explicitly in the commercial crime interventions, by attempting to improve levels of guardianship and making simple changes to the routine activities of staff (for example ensuring two people banked the cash at the end of the day). These interventions were invariably a success, and thus suggestive that routine activities theory is a viable theoretical foundation. The sexual victimisation studies predominantly attempted to use education to reduce the vulnerability of the target. However, in practice there was no explicit link between the education and information provided, and how this was expected to make a change to the routine activities of potential victims. This does not detract from the usefulness of routine activities theory, but rather emphasises the need for clarity of mechanisms in repeat victimisation prevention interventions.

RELIABILITY OF CONCLUSIONS

Tilley (2010) expresses four concerns with the reliability of the positive effects noted in Weisburd et al.'s (2010) review of problem oriented policing interventions. These same concerns are equally valid when considering the positive outcomes of this review of repeat victimisation prevention interventions, and are worthy of consideration here.

Causal attribution: The repeat victimisation prevention interventions examined in this review operated within an open system. There was no control exercised over the other interventions available to participants, whether these were individuals, businesses, or areas. This meant that in practice some participants were exposed to other tactics which (whether directly or indirectly) may have impacted on their risk levels. In some cases other interventions were identified in the evaluation report (for example home improvements in Kirkholt, a police burglary crackdown in New Parks, a counselling service to victims in the NY and Seattle field test). Where other interventions were active at the same time as that evaluated, there is reason to be cautious about causal attribution. In other cases information was not provided, which may or may not mean that the intervention acted in isolation. Ancillary work such as increased publicity or extra resources may be the causal factor, rather than the intervention focused on repeat victims. It is also conceivable, and perhaps probable, that some victims take crime prevention measures themselves but do so outside of the scope or funding of any project. The extent to which this occurs appears unknown, and any impact of these independent efforts would not only go undetected in the evaluations but would tend to reduce the perceived impact. Efforts were made by Bowers et al. (2003) to measure this effect, and results were presented on the effectiveness of the programme, and of the effectiveness of target hardening, regardless of whether this had been funded by the programme or elsewhere.

Counterfactual: Crime rates in local areas are susceptible to fluctuations. Even where a comparison area has been carefully chosen to match on socio-demographic factors, “unless rates have tracked one another closely for a suitably long period of time then the counterfactual provided by the comparison area is dubious” (Tilley, 2010: 185). This is perhaps an argument that better quality evaluation designs are required, something which is in line with the arguments of the Campbell Collaboration.

The repeat victimisation prevention interventions examined here are susceptible to this possibility. This problem is not applicable to the sexual victimisation studies included in this review, as they were conducted on a randomised controlled trial basis, and therefore the problem of different fluctuations should have been designed out of these interventions.

RECOMMENDATION SIX:

When identifying a comparison area for use within an evaluation of a crime prevention intervention, the selection process should include a consideration of the extent to which crime rates have matched in the years prior to the start of the intervention, as well as other socio-demographic factors.

Compensatory effects: Where there are people working in the comparison area who have an interest in the outcome of an intervention, compensatory effects may be made. This may be to attempt to perform as well as (or better than) the treatment area, or conversely to purposefully under-perform in order to secure future funding. It is possible that this was a contributory factor within some of the repeat victimisation prevention interventions, particularly where comparison areas were different basic command units within the same police force, rather than an abstract administrative border. The possibility this occurred was not directly examined within the evaluations herein, but is a standard risk to the integrity of any evaluation design. The sexual victimisation studies examined within this review, although not suffering from the same area based issues, may have experienced a similar (although perhaps unintentional) effect where the control group were aware of the purposes of the study. With the exception of the NY and Seattle field test, all participants were from the same cohort at university, and so although the purpose of the study was not revealed to the control group in all studies, there may have been the possibility for the knowledge bleeding from the participant to control group.

Comparable control: Repeat victimisation prevention is a broad remit. When tailoring an intervention to the specific problem, it is clear from the evaluations included here that there are a multitude of variations of the differing specifics of a crime problem, even within the same crime type. These crime problem specifics should, in an ideal evaluation, be matched in a comparison area. For example, in Kirkholt there was a distinct problem with coin payment meters, so ideally the comparison area would have a similar problem with coin payment meters, in order to be truly comparable. In practice this, taken in conjunction with the recommendations to match on socio-demographic and prior crime rates, makes it very difficult to find an appropriate control group. This raises questions about whether the control groups used to compare the treatment areas with are suitable.

In addition, it could be argued from a realist perspective that the measures of success as included in the systematic review (i.e. the odds ratio calculations) were not a subtle enough tool to assess the effectiveness of the repeat victimisation prevention interventions.

Quantification of findings is generally desirable but on occasion can be misleading. When it comes to repeat victimization, the odds-ratio calculations and resulting interpretation were necessarily based on crime incidence. Yet, as discussed, incidence was the secondary outcome measure and does not capture change in repeat victimisation. The rather fundamental distinction between incidence and concentration rates should not be lost in the attempt to quantify the outcomes in a systematic and quantifiable manner.

The continuation of the use of the odds ratio measure of success to link the data for the realist review directly to the systematic review findings could be considered to make the realist synthesis invalid. This is because the realist approach rejects the idea of approaching the data in such a prescriptive way, with the emphasis on the hierarchy of evidence and outcome measures, and would not necessarily consider such statistical measures of success to show the full picture. The present author considered this when conducting the research, and concluded that although relying on the statistical meta-analysis for a success measure may not be perfect from the viewpoint of a realist synthesis, the current literature and guidance available for carrying out realist syntheses did not at this time offer either a preferential or viable alternative to assessing intervention outcomes.

THE ‘NO INFORMATION’ OUTCOME OF A STRICT SYSTEMATIC REVIEW

Strictly speaking, close adherence to the systematic review process would produce a review of repeat victimisation prevention which found that nothing at all is known. This is because the primary outcome measure – change in repeat victimisation – was never available for comparison groups. From this ‘maximal-experimentalist’ position, therefore, no studies would be eligible for review. For present purposes, the systematic review relaxed the eligibility requirements. Weisburd et al. (2010) similarly assessed alternate samples of problem-oriented policing evaluations to address this issue to some extent – one with greater relaxation of the review requirements. This itself can be interpreted as a response to the realist perspective, perhaps reflecting John Eck’s co-authorship of that review.

‘THE THIRD WAY’

When considered alongside the greater incorporation of assessment of mechanisms and contexts that appear to be becoming a component of the systematic review process, it seems reasonable to suggest that the systematic reviews are incrementally bending to the realist critique but, as a result, incorporating the most useful components. This is somewhat of a different vision of the distinct process envisaged by Pawson (2006) but, from the perspective of a policy-maker, may be a pragmatic result that leads to the optimal policy-relevant evaluation mix. That is, it appears that the systematic review process is incrementally incorporating key elements of the realist critique. The resulting ‘third way’ evaluation tool appears to be a combination of both perspectives, taking the best of both while carrying the moniker of the systematic review.

A PROOF OF CONCEPT?

Despite a number of criticisms of the repeat victimisation prevention interventions being included here, it remains clear that as a strategy to prevent crime, there has been a definitive ‘proof of concept’ from the evaluations conducted to date. It is clear that prevention of repeat

domestic burglary can be very successful, providing the members of staff implementing the prevention tactics are carefully selected and trained, interventions are tailored to the situation and the programme can remain flexible enough to meet changing demand. What remains is to refine the provision and implementation of tactics and ensure that best practice is shared amongst policy makers and practitioners.

It is perhaps surprising that so little is known outside of preventing repeated domestic burglary. Domestic violence interventions have focused predominantly on the offender (see for example the domestic violence arrest experiments, Sherman 1992) and there has been little work examining the ways in which the victim can protect themselves – still less with male victims of domestic violence. The few interventions aimed at preventing repeated commercial crime have met with enormous difficulties, with a number of business owners considering a level of crime to be unavoidable – where the businesses can even be identified in the first place. The information available may improve with the Home Office's planned expansion of the Commercial Victimization Survey (see Smith and Harvey, 2010) which will at least provide some detail of the extent to which repeat victimisation occurs within businesses. Perhaps a different focus, on risk management, and a greater emphasis on cost-benefit analyses may encourage business owners to take greater precautions against repeated victimisation. Further research needs to be done, although this review again proves the premise here is sound. Finally, the attempts to prevent repeated sexual victimisation included here do show distinct promise. However, the mechanism by which they operate is unclear, and the measurement tools used to assess victimisation and risk would benefit from further refinement. Further, by far the majority of these interventions have been carried out with young women at American universities. This subgroup, whilst at risk of repeated victimisation of this crime type, has distinct risk patterns which may not necessarily be extrapolated successfully to the broader population. Again, male victims have been largely disregarded in this research to date.

There are crime types where the use of preventing repeat victimisation as a tool to reduce crime has not been evaluated. The dearth of research is particularly notable in hate crimes – just one study was identified (Sampson and Phillips, 1995, see also Phillips and Sampson, 1998) which examined repeated racial victimisation, and no evaluations of disablist or homophobic repeat victimisation were identified. There are specific difficulties with preventing these repeated hate crimes. The initial identification of the victim can be problematic as: the crime type may differ (and escalate); the victim may be victimised in multiple locations (perhaps covered by different police forces); or the victim may be embarrassed, or scared about coming forward. There may be different perpetrators, or the problem may be that of 'virtual repeats', where similar targets are victims. These difficulties do not excuse the failure to attempt to prevent these crimes – hate crimes are particularly known for the repeated nature of the victimisations (Iganski, 2008).

RECOMMENDATION SEVEN:

Further research on preventing repeat victimisation needs to be conducted, and best practice shared. Future directions should include:

the refinement of practice for preventing repeated domestic burglary;

the identification of mechanisms and refinement of measurement tools for repeated sexual victimisation;

a greater focus on the victim within repeated domestic violence;

reconsideration of the approach to encourage businesses to tackle repeated commercial crime (whilst retaining the tried and tested techniques);

an urgent examination of repeated hate crimes, followed by adapting existing situational crime reduction techniques to this crime type.

the development and evaluation of efforts to prevent repeat victimisation for the many types of crime not addressed to date

META-EVALUATION: SYSTEMATIC REVIEWS

The use of systematic reviews to assess crime prevention initiatives has resulted in a number of contributions to knowledge. At the same time, there are distinct criticisms which can be made of the systematic review methodology. These are outlined in the table below. Many of these contributions and problems have been encountered during the course of conducting this review. Others have been identified through an examination of the systematic review field of knowledge. The problems described in the table below are seen to a greater or lesser extent in the various existing systematic reviews of crime prevention initiatives. These problems do not necessarily undermine the value of the systematic review process, but rather often demonstrate areas that the systematic review has not yet mastered or even expanded into. Systematic reviews have made many contributions both to the knowledge base of crime prevention and to the promotion of high quality, transparent evaluations on which future policy decisions can be based. Systematic reviews are rightly respected and valued for the contributions they make. The argument here is that there is further room for improvement and tailoring to the specific needs of meta-evaluation in a crime prevention context, building on the already impressive methodological base that exists.

Table 9.1: Critique of systematic reviews

Contributions	Problems
Have enhanced the role of evaluation in criminology and criminal justice	The rigid framework risks encouraging programmes to be tailored to evaluations, rather than vice-versa in order to be compatible with systematic review approach
Have promoted the use of situational crime prevention	The focus on outcomes risks missing important contextual and mechanism signatures which indicate reasons for success.
Are transparent, thorough and encourage high levels of accountability - thus pushing for higher quality in evaluations	Rejects potentially useful data due to reliance on hierarchies of evidence and emphasis on randomised controlled trials
Have drawn attention to the need for evidence based policy	The limited attention to context and mechanisms may result in interventions being applied inappropriately
Have drawn attention to the need to underpin research with high quality evaluation	Limited focus on theory
The findings are largely valid as a snapshot and overview of the research to date	Risks losing the understanding of improvements and developments over time
Present findings clearly, with easy to understand success or failure presented at-a-glance	'The devil is in the detail' (Tilley 2010) and the nature of systematic reviews risk losing important details
Ideal for assessing quantitative findings	Limited usage of qualitative findings
High internal validity	Low external validity

META-EVALUATION: REALIST SYNTHESSES

The realist synthesis methodology is a somewhat less developed and refined approach than its counterpart the systematic review. This is due in no small part to its relative youth. Systematic reviews have a history in medical usage long before the development of their use to evaluate crime prevention initiatives. In contrast, realist syntheses do not have this pre-existing body of evidence to fall back on. The trial and error process which had already ironed out many of the teething problems within systematic reviews before their application to crime prevention is at a much earlier stage within realist syntheses. The criticisms put forward in the table below should therefore be read with this context in mind – to date, far less work has been conducted with realist syntheses and therefore there is inevitably still scope for improvements and refinements of the methodology. This is not to suggest that the realist synthesis approach has no contribution to make to the knowledge base in crime prevention. On the contrary, it has renewed the link between theory and practice, and provides greater explanatory guidance for policy makers and practitioners on the problems they may face and the details of how a programme is expected to work. This greater attention to the theoretical underpinnings is laudable, and the systematic review methodology could gain much from this approach. Of course, the reverse is also true - the realist synthesis also stands to benefit by examining and assimilating aspects of the systematic review.

Table 9.2: Critique of realist syntheses

Contributions	Problems
Have added an increased focus on context and mechanisms to produce a greater understanding of where and how crime reduction occurs	The way in which success and tangible outcomes of interventions are assessed is not clear from methodologies available to date
Has re-established the necessity of linking theory and practice	There is a lack of clarity surrounding the process of theory development and tracking
The rejection of the hierarchy of evidence enables the examination of evaluation details which may be lost in other meta-analyses	Broad inclusion criteria can be criticised for including evaluations of a low methodological quality, thus impacting validity
Provides extra guidance for practitioners and policy makers on how to implement a programme so that it may be successful	Has not yet produced a large enough body of work to make an impact
Provides guidance on where an intervention may be appropriately applied	The paucity of published realist syntheses means that the methodology has not been as refined, nor open to as widespread scrutiny as systematic reviews
Can identify why programmes fail using signatures, for example whether the underlying theory is faulty or whether implementation problems restricted success	There is ambiguity and a lack of clarity within the literature search process. 'Signatures' are compatible with RCTs and quasi-experimental evaluation, providing insight into how, not just whether, interventions work.
Can track the developments and continual improvements to an intervention over time through theory tracking and building	A systematic review could use date as a variable, splitting findings into different time periods for consideration of change over time.

RESEARCH ANSWERS

Finally, thoughts turn to the original set of research questions posed at the start of this thesis. The full research and findings have been detailed in the previous chapters of this thesis. However, the five specific questions this research set out to answer are addressed in summary below:

ARE BOTH SYSTEMATIC REVIEWS AND REALIST SYNTHESSES WORKING TOWARDS IMPROVING THE KNOWLEDGE AND PRACTICE OF CRIME PREVENTION?

Both systematic reviews and realist syntheses do have the shared goal of pushing forward the boundaries of knowledge in order to improve the policy and practice of crime prevention. Although each goes about this in a different way, Chapter Eight demonstrated that the core principles of each methodology were shared. Each has a practical evidence-based and policy-

oriented approach, and strives to provide a clear explanation and analysis of existing work in order to improve future programmes using a scientific base of evidence.

WHAT ARE THE MAIN DIFFERENCES – BOTH PRACTICAL AND PHILOSOPHICAL?

At first glance, systematic reviews and realist syntheses appear to be from very different backgrounds. Systematic reviews within crime prevention are heavily based on Donald Campbell's quantitative, experimentalist approaches to knowledge. In contrast, the realist synthesis method stems from the scientific realist philosophies of Karl Popper. However, the shared philosophy of falsification in the guise of evolutionary epistemology underpins each approach. Campbell was the founder of evolutionary epistemology, in which he generalised Popper's falsification philosophies to the process of development of knowledge across all biological, psychological and social levels (Heylighen, 1996; see also Knutsson and Tilley, 2009 who claim Campbell for the realist camp). This commitment to the falsification of hypotheses forms the philosophical backdrop to both systematic reviews and realist syntheses.

In practical terms, the two have distinctly different approaches to this improvement and expansion of knowledge. Fundamental differences appear at each stage of: literature searches; filtering of relevant studies; data extraction; assessing effectiveness; and drawing conclusions. These differences were discussed in depth in Chapter Eight, but in brief, the practical differences centre around the step-by-step, checkbox-like approach of the systematic review contrasted with the iterative, judgement-based approach of the realist synthesis. These practices are seemingly at odds with each other, and aim to get different answers out of the same material. The systematic review is interested in outcomes, in whether or not the crime prevention intervention under question has made a significant difference to crime rates. In contrast, the realist synthesis is more concerned with the underpinning theories, whether the mechanisms are reliable, and in what contexts interventions can best succeed. At risk of caricaturing the approaches, the systematic review examines whether crime was reduced while the realist synthesis examines how it was reduced. These differences mean that the debate between the two has involved some 'talking past' each other, and in the present author's opinion means that the approaches are not necessarily as incompatible as they may appear – a point returned to below.

ARE SYSTEMATIC REVIEWS AND REALIST SYNTHESSES COMPLEMENTARY APPROACHES, OR DOES ONE SUBSTITUTE FOR THE OTHER?

The two methodologies approach the meta-evaluation of interventions with different, yet complementary aims. The systematic review concern with outcome fits well with the realist synthesis focus on context and mechanisms. Each methodology has aspects which it conducts more successfully than others, and these are complementary. The clarity and transparency of the systematic review methodology, along with its comprehensive literature searches, form a substantive backdrop to the iterative theory tracking and examination of mechanisms developed in the realist synthesis. The systematic review conveys the level of success of an intervention in a scientific, justifiable manner, something which the realist synthesis currently fails to achieve. However, the realist synthesis may utilise some information which would be discarded in a systematic review. This, in theory, allows the realist synthesis to provide a greater level of detailed explanation and analysis of why and how interventions work (or not). These two approaches not only *can* be seen as complementary, they *should* be seen as

complementary, and efforts made to integrate the ‘best’ aspects from each approach to develop a new ‘systematic realist review’.

CAN THE TWO APPROACHES BE RECONCILED IN A POSITIVE WAY?

There is a potential for the two approaches to merge. The combination has the possibility to produce interesting synergies and improve the progression of the evidence base to support policy. There are likely to be points of contention with such integration. Despite their many complementary elements, each of the methodologies is complete in itself, which means there will be inevitable compromises to be made in order to develop a ‘third way’. There needs to be debate and discussion bringing together both sides in order to facilitate this process.

The first step towards reconciling the two approaches would be to develop a two stage methodology, reminiscent of the process portrayed in this thesis. The first stage would be outcome oriented, based on the systematic review approach for literature identification, filtering of relevant studies, and statistical meta-analyses where appropriate. The second stage would develop from there, re-examining rejected studies and using theory-tracking to identify further related evidence. This second stage would be largely based on the realist synthesis process, and would focus on mechanisms, contexts, and underlying theories. In this way, the benefits of the two approaches would be realised with minimum disruption to existing processes. This would enable use to be made of the already substantial network built up surrounding the Campbell Collaboration reviews. The methodology would necessarily be open to improvements and refinements as further work was carried out.

CAN A COMBINATION OF THE TWO APPROACHES DETERMINE WHETHER OR NOT PREVENTING REPEAT VICTIMISATION IS A PROMISING DIRECTION FOR CRIME REDUCTION?

The combination of approaches suggested above has been trialled in this thesis in the context of evaluating repeat victimisation prevention programmes. The systematic review identified that there are distinct successes, and that overall this approach to crime prevention shows distinct promise. This provided the broad brush strokes of meta-evaluation, leaving the fine detail to be filled in by the realist synthesis in the next stage. This identified flaws in the programmes as they had been implemented, and examined contextual factors in greater depth. Common implementation problems such as staffing, a lack of tailoring, and a lack of flexibility were identified and linked to the measures of success as concluded from the systematic review process. The two approaches therefore both had a distinct contribution to improving knowledge about existing repeat victimisation prevention programmes. There is a need to extend research in the field of repeat victimisation prevention into other crime types, and to improve implementation. There is also a need to improve evaluation, and the thesis herein argues for the development of a combination of the systematic review and realist synthesis approach as a ‘third way’ of meta-evaluation. In the meantime, it is important that appropriate means of assessment are used to evaluate crime prevention interventions – regardless of whether that involves quick context specific answers, or long, detailed and externally valid evaluations.

SECTION IV:
REFERENCES AND APPENDICES

Outside of a dog, a book is a man's best friend. Inside of a dog, it's too dark to read.

- *Groucho Marx*

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If I have seen farther it is by standing on the shoulders of giants

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Appendix A: Summary of studies excluded from the systematic review

EXCLUDED DOMESTIC BURGLARY EVALUATIONS

WEBB (1996) GREENWICH – HOMESAFE (UK)

Target hardening was provided to victims and the vulnerable in an area of 2500 households in Greenwich. The local authority also installed entry phones to blocks of flats. There was a change in police recording systems during the intervention. As a result full data was unavailable for the evaluation, and it was not eligible for inclusion in this review.

WEBB (1996) MANCHESTER – HOMESAFE (UK)

Target hardening was provided to victims and the vulnerable in a rundown area of Manchester with 1750 households and a high population turnover. Take up was initially slow, thought to be because of a previously failed scheme in the area, but after increased publicity and local contact, demand increased to the point where the planned target hardening of neighbours of victims had to be abandoned. The reason for exclusion was a lack of a comparison group.

WEBB (1996) PLYMOUTH – HOMESAFE (UK)

Homesafe was targeted at an area of Mount Gould where there was a relatively high burglary incidence rate of 9 per 100 households in the year before the intervention. The predominant focus in Plymouth was on blanket target hardening given to local authority homes, and offered to right to buy households, and one hotspot (low take up). Also eligible for the security upgrades were burglary victims, those deemed 'vulnerable' but also people who had recently moved to the area (the latter based on an identified higher risk). However, as the focus was largely not on burglary victims, Plymouth was excluded from this review. It is worth noting that six covert 'silent' burglar alarms triggering a rapid police response ('Home Office alarms', p. 13) were available for repeat victims in the target area (but could be used throughout the city – p.13). Repeat victimisation was not measured because "police were unable to give exact details of repeat victimisation" (p. 76).

CASEY ET AL. (2004) NEW ZEALAND TARGET HARDENING PROGRAMME (NZ)

This evaluation (Casey et al. 2004) examined a target hardening programme which provided security equipment to households within seven Victim Support areas, which had experienced 2 or more burglaries. There were severe implementation problems in the areas of: installation; inadequate funding; referrals; carrying out interviews; timescale; and data. The last of these problems meant that the length of follow up time measured for each separate intervention was inconsistent, and that not all records were accessible as computerized records did not exist in some areas. These data issues are the reason for excluding the New Zealand target hardening programme from the systematic review.

GOODWIN (2002) TASMANIA (AUSTRALIA)

Goodwin (2002) presented a paper on Project Samaritan, a repeat victimization prevention programme which was extended nationwide after a successful trial in Tasmania. This programme was predominantly based on giving the victims information and advice on crime prevention and their risks, encouraging them to improve their security. The full report was not available, and attempts to contact the author were unsuccessful. Data was therefore not available to include this study in the systematic review.

TAPLIN AND FLAHERTY (2001) MID-NORTH COAST (AUSTRALIA)

This rural area, 450km from Sydney, was part of the Safer Towns and Cities evaluation (Taplin and Flaherty, 2001). There were a range of measures put into place, but these were targeted at burglary hot spots rather than at individual households. Therefore, this evaluation did not meet the eligibility criteria for inclusion in the systematic review.

FISHER (1998) STOCKPORT, SAFE AS HOUSES (UK)

This project aimed to prevent repeat victimization whilst not using additional funds, although it did use £25,000 of Victim Support money for security improvements. A dedicated team gave crime prevention advice to victims and their neighbours. There was insufficient data in this short article available for eligibility for inclusion in the systematic review, and no further report could be located.

HOLDER ET AL. (2004) ACT RESIDENTIAL BURGLARY REDUCTION STRATEGY (AUSTRALIA)

This intervention aimed to provide a heightened response to repeat burglary victims in four high crime areas. An unexpected reduction in the number of burglaries reduced the number of victims who could potentially receive the service. Only two victims took up the offer of a security review, and “With so few taking up the review offer there was nothing to evaluate” (Holder, 2004: 14). The survey of victims which was carried out examined factors relating to victimisation, and whether respondents acted on prevention advice given by police. It did not follow up respondents at a later date to examine effectiveness of repeat victimisation prevention advice.

EXCLUDED SEXUAL VICTIMISATION STUDIES

YEATER AND O'DONOHUE (2002) KNOWLEDGE, RISK PERCEPTION AND THE ABILITY TO RESPOND (USA)

This randomised controlled trial assigned 300 undergraduate students to either receiving an educational based prevention programme, or not. The programme consisted of three stages, covering: rape myths and facts; risk factors and perception; and response strategies. Each stage was followed by a questionnaire. The control group received questionnaires only (i.e. without the prevention literature). The study was excluded because “the study did not test whether the program is effective at decreasing rates of sexual assault” (Yeater and O'Donohue, 2002: 1142).

YEATER ET AL. (2004) A BIBLIOTHERAPY APPROACH (USA)

This study involved the provision of a self help guide to 110 female undergraduate psychology students who were randomly assigned to receive either the treatment or to a waiting list control group. The book and study were designed to give the impression of giving general dating advice, rather than merely providing sexual assault prevention advice. The purpose of the study was revealed to participants at the end of the study. The study included victims and non-victims, and although some of the analysis within the report differentiated between repeats and non-repeats, data on numbers of students experiencing prior victimisation was not provided in the report, hence it was excluded from the systematic review.

BLACKWELL ET AL. (2004) TOWARD EFFECTIVE RISK REDUCTION PROGRAMS (USA)

This was a review of 26 studies designed to prevent repeated sexual victimisation. Of these, 18 were considered to be not methodologically rigorous. The review did not provide new information, nor did it provide data on the studies included. It was therefore not an appropriate study for inclusion here. The eight methodologically sound studies included in the review were assessed for inclusion in this systematic review (where they had not already been identified).

HEPPNER ET AL. (1995) DIFFERENTIAL EFFECTS OF RAPE PREVENTION PROGRAMMING (USA)

Identified from the Blackwell review, this study examined how best to provide people with information about rape prevention: a didactic-video program, an interactive drama, or control group. The participants were 258 men and women on psychology courses at university. No information on prior victimisation was provided, hence this was not included in this systematic review.

CALHOUN ET AL. (2001) SEXUAL ASSAULT PREVENTION (USA)

Not acquired. Poster presentation at a conference, not able to get. Identified from Blackwell review.

PINZONE-GLOVER ET AL. (1998) AN ACQUAINTANCE RAPE PREVENTION PROGRAM (USA)

Another study identified from the Blackwell review which did not focus on repeat victimisation and was therefore excluded from this review. The study focused on changes in attitudes toward women, rape myth acceptance, and perceptions of rape scenarios. The participants were 152 male and female psychology undergraduates.

GIDY CZ ET AL. (2006) SELF DEFENSE AND RISK REDUCTION PROGRAM (USA)

This study expanded and improved upon the tactics used by Gidycz et al. 2001. It added a self defence component to the training, and included a video with vignettes of potentially risky situations, encouraging women to brainstorm potential options in response. The participants were 500 undergraduate females, randomly assigned to the risk reduction treatment or a waiting list control group. The results did not differentiate between victims and

non-victims in the treatment and control groups for the pre- and post- measures, hence the effect on repeat victimisation could not be discerned and the study was not included here.

GIDYCH ET AL. (2001) MULTI SITE SEXUAL ASSAULT RISK REDUCTION (USA)

This study examined 762 female psychology students who took part in a single three hour session on sexual assault prevention. The interactive session was based around videos, discussion and role plays. The results did not differentiate between victims and non-victims in the treatment and control groups for the pre- and post- measures, hence the effect on repeat victimisation could not be discerned and the study was not included here.

EXCLUDED DOMESTIC VIOLENCE STUDIES

Two bodies of evidence were excluded from this review. Firstly, the domestic assaults arrest experiments. These began with the Minneapolis Domestic Violence Experiment, which was then replicated in many other parts of the United States of America. These were excluded here because the focus was on the offender rather than the victim, and also because comprehensive analysis is available elsewhere (see for example Sherman, 1994; Garner et al., 1995). Secondly, excluded from this review were family violence experiments. These have been included in a recent systematic review (Davis et al., 2008) and the focus was not on *repeat* family violence per se. Therefore, the only studies eligible for inclusion were those where the focus was on preventing repeated victimisation, where the intervention was directed toward the victim rather than the offender. Studies which did not fall into the previous two categories for exclusion could still be excluded for other reasons, and these are outlined below:

LLOYD ET AL. (1994) A DEMONSTRATION DOMESTIC VIOLENCE PROJECT ON MERSEYSIDE (UK)

This intervention focused on the use of loaned alarms for victims, and a surrounding infrastructure to support their use, and to encourage longer term solutions to be found. The intervention was generally thought to have a positive impact on the victims and their families. However, there was no comparison group, and no quantitative evidence. Therefore, this evaluation was not eligible for inclusion in the systematic review.

HANMER ET AL. (1999) ARRESTING EVIDENCE (UK)

This project used a three tier model of escalating interventions, with a focus equally on the victim and the offender. Interventions included targeted police patrols, Cocoon Watch, visits from community and domestic violence officers, and letters to victims and offenders. The time intervals between calls for assistance increased. However, there was no comparison group and therefore this evaluation was excluded.

FARRELL AND BUCKLEY (1999) 'D' DIVISION, MERSEYSIDE. ALSO, WALKLATE (1992). (UK)

This study assessed the impact of a domestic violence unit and a pro-arrest stance on the repeat calls to service from victims. There was a suggestion of success, but not for chronic victims. The intervention mechanism was unclear, and most of the focus was on improving the knowledge of the police and on increasing arrests. The support provided to victims was not discussed, except to say that support would be given “in whatever decision they make” (Walklate, 1992:21). As the predominant focus was not on victims, this study was not included here.

MEARS ET AL. (2001) REDUCING DOMESTIC VIOLENCE REVICTIMIZATION (UK)

This evaluation examined the impact of protective orders and arrests on revictimisation. The report suggested that these were not successful, even in combination. There was no comparison area, the predominant focus was on the offender, and therefore this intervention was not suitable for inclusion.

FRIDAY ET AL. (2006) IMPACT OF A SPECIALIST DVU (UK)

This evaluation assessed the impact of a domestic violence unit set up to address the most serious incidents in Charlotte, North Carolina, in order to reduce repeat victimisation. The effect on suspects was that they had lower offending prevalence in the future (29% vs. 37%). However, there was “no significant difference in the prevalence or incidence of future domestic violence incidents for victims” (p. 5). The reason for excluding this evaluation was that there was a focus on arrest (and therefore offending rather than victimisation) as the intervention.

WANGMANN (2002) TAMWORTH DV PROJECT (AUSTRALIA)

This study evaluated a project which had a “follow-up role rather than intervention at the point of ‘crisis’” (p.8). The project proactively approached victims and offered them support, counselling and referral to other agencies. A comparison area was not examined, and therefore this evaluation was not eligible for inclusion here.

ROBINSON (2006) CO-ORDINATED COMMUNITY RESPONSE (UK)

This evaluation examined the impact of Multi-Agency Risk Assessment Conferences (MARACs) on the reduction of harm to 146 domestic violence victims, looking at both the process and the outcome. “[A]ccording to the police data, 97 of the 146 women experienced no further incidents of violence or abuse” (p.781). No comparison area was examined, hence this evaluation was excluded from this review.

HESTER AND WESTMORELAND (2005) EFFECTIVE INTERVENTIONS AND APPROACHES (UK)

This evaluation gave details on three interventions specifically designed to reduce repeat domestic violence through a mixture of advocacy, support, and target hardening. Two of those did not include a comparison area (Northampton, Bradford) and so are not included here. The third (Croydon) did not provide a pre-post measure of prevalence or incidence and was therefore also excluded. The data which was provided for Croydon suggested a varied impact on repeat victimisation of the advocacy programme assessed.

COMMERCIAL BURGLARY

TILLEY (1993) SAFER CITIES (UK)

This review examined schemes which were designed to reduce commercial burglary, aimed at small businesses. Although previous experience of burglary was not a requirement of receiving the intervention, and hence the interventions were not specifically intended to prevent repeat victimisation, in practice most businesses applied for security measures after a burglary. There was no comparison data provided for any of the interventions assessed.

MAWBY AND JONES (2004) EVALUATION OF A HOTEL BURGLARY PROJECT (UK)

The intervention provided a graded response to hotel burglaries, with non-burgled hotels eligible for a bronze response of an information pack; hotels burgled 1-3 times receiving a self assessment crime survey and matched funding up to £500; and hotels burgled four or more times receiving a crime prevention survey and matched funding up to £2000. There were extreme difficulties involving hoteliers and the tourist industry, and few took up the intervention. The report focused on the implementation problems, and did not attempt to examine any changes in victimisation.

Appendix B: Domestic burglary study level coding manual

1. Study ID number. Assign a unique ID number to the study (not the report). Where there are multiple unique studies within the report, add a decimal to distinguish the individual studies and code each separately
2. Report number. Unique identifier from Excel sheet. Where 2 or more reports are being used to code a single study, code each in additional columns. Columns should be identified as reporta, reportb etc
3. What type of publication is the report? If two separate reports are being used to code a single study, code the type of the more formally published report (i.e. book chapter or journal article)

1. book	5. technical report (include gov reports)
2. journal article	6. conference paper
3. book chapter	7. unpublished
4. thesis or doctoral dissertation	8. other (specify)

4. What is the publication year? Use 4 digits, with 99 for unknown. Again, code the year of the more formally published report where two reports are being used to code the same study.

Sample characteristics

5. Specify the predominant type of dwelling
 1. Flats/apartments
 2. Houses/bungalows
 3. Trailers/static caravans
 4. Other, specify
 5. Mixed (select when not one predominant type)
 6. Unknown
6. Specify the predominant type of occupancy
 1. Privately rented
 2. Owner-occupied
 3. Local authority housing
 4. Multiple occupancy
 5. Mixed (select when not one predominant type)
 6. Unknown
7. Had the sample previously been victims of the type of crime the intervention was designed to tackle? Include attempts. Code once only.
 1. Victims of same crime type
 2. Victims of different crime type
 3. All victims, but mixed same and different crime types
 4. Mixed victims and non victims
 5. Sample had not been prior victims (check eligibility of study)
 6. Unknown (check eligibility of study)
8. How recently had the prior victimisations occurred within the sample?
 1. Examined victimisations up to 1 week prior to intervention

2. Examined victimisations up to 1 month prior to intervention
3. Examined victimisations up to 3 months prior to intervention
4. Examined victimisations up to 6 months prior to intervention
5. Examined victimisations up to 12 months prior to intervention
6. Examined victimisations that took place longer than 12 months prior to intervention
7. Mixed
8. Unknown

Research Design Descriptors

8. Did the research use group contrasts or pre-post contrasts?
 1. Group contrasts
 2. Pre-post contrasts
 3. Other (check eligibility of study)
9. Total treatment area. Code for the area the intervention was focused on rather than the number of interventions applied.
10. Either: (if 8=1) Treatment group size (start of study). Code 999 for unknown
11. Control group size (start of study). Code 999 for unknown
12. Or: (if 8=2) start of study group size. Code 999 for unknown
13. End of study group size. Code 999 for unknown

Nature of the treatment descriptors

14. Style of situational crime prevention applied (select all that apply)
 1. Target harden
 2. Control access to facilities
 3. Screen exits
 4. Deflect offenders
 5. Control tools/weapons
 6. Extend guardianship
 7. Assist natural surveillance
 8. Reduce anonymity
 9. Utilise place managers
 10. Strengthen formal surveillance
 11. Conceal targets
 12. Remove targets
 13. Identify property
 14. Disrupt markets
 15. Deny benefits
 16. Reduce frustrations and stress
 17. Avoid disputes
 18. Reduce emotional arousal
 19. Neutralise peer pressure
 20. Discourage imitation
 21. Set rules
 22. Post instructions
 23. Alert conscience
 24. Assist compliance

25. Control drugs and alcohol
26. Other, specify
27. Not applicable
28. Unknown
29. Victim education (include group sessions, leaflet drops and security surveys)
15. Who was responsible for the costs involved?
 30. Research team
 31. Central government (include bodies linked to central gov e.g. NIJ; Home Office)
 32. Local government
 33. Police / law enforcement
 34. Victim
 35. Other, specify (indicate here if more than one of the above)
16. Cost to the responsible party. Indicate whether specific, estimate or unknown; and currency.
17. Duration of the intervention (weeks). Multiply months by 4.3 and divide days by 7. Code 999 for unknown, though estimate is acceptable.

Appendix C: An example of a completed comments form

Study: Homesafe - Merthyr Tydfil	Crime Type: Dom Burg
Author: Webb	Successful (Y/N) Y
Year: 1996	
Context: 1614 LA households. 8.7% burglary rate. Mixed housing (30% private). Wide deprivation - 23.9% unemployment. Disproportionate young offenders RV: 10% population experienced 34% burglaries. 516 homesafed in first year.	
Implementation comments: 32% target hardened - vulnerable and attempts target hardened more quickly than actual victims (because more likely to be around in the day). Referrals by Police, Crime Management Unit and VS and many other referrals sources inc. self and neighbours. Lock fitter prioritised referrals daily - also had mobile for emergency fitting (he assessed, gave advice and carried out work) and worked evenings and weekends. Residents Association played key role - identified vulnerable and elderly, even fitted security and did publicity. Homesafe van also boosted publicity. Lock fitter became well known - some counselling training will be given in future. Consultation with local people and key partners before final decision on target area taken. Lack of computerised records meant not all samples could be traced from referral to completion - therefore % unknown of referrals target hardened. 64% target hardened within 2 days.	
Techniques used to prevent crime: Target harden victims → including victims in year prior to Homesafe, Target harden vulnerable → including elderly, identified through ineligibility for Jury Service Some hotspots also targeted Given passive infra red lights, stand alone alarms and dummy alarm boxes. High continuous publicity Work with residents groups Lock fitter trained to give advice Crime prevention clinic in town centre	
Confidence in police: Not measured. Homesafe had a secondment from police.	
Any other comments: Unreported attempts may have been a ploy to get target hardening done quickly (50 cases). Lots of regeneration activity on the estate e.g. Community House, VS Base, car project, drug aid satellite, drug survey, DV surgeries, 6 new NW groups, anti burglary video, CCTV, furniture recycling for young homeless to be supported as new tenants, drugs campaign, extra PIR lighting, youth activities, play schemes, ant bullying work in schools. Theft of and from cars fell slightly. Non-domestic burglary increased slightly. No indication of displacement. Fear of crime also decreased.	
Include/Exclude: Include	
Included in systematic review (Y/N): Y	

